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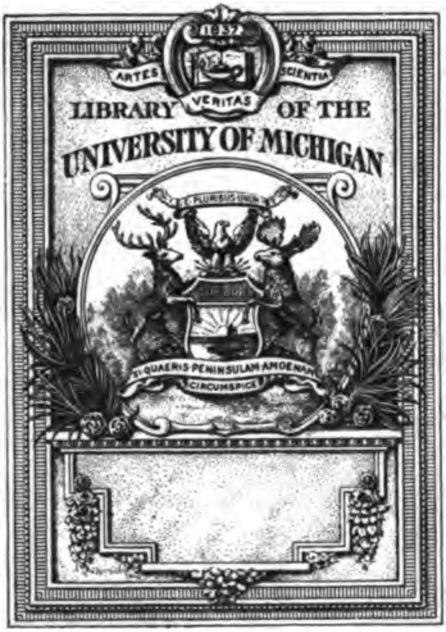

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1848.

PART FIRST.

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2. *Report of the Health of London Association on the Sanitary Condition of the Metropolis.*
3. *The Liverpool Health of Towns Advocate.*
4. *The Journal of Public Health, and Monthly Record of Sanitary Improvement.* No. I.

Sanitary Reform has now taken its place as the great question of the day. To this post of honour it has won its way, much more by its own intrinsic merits, than by the exertions of the Associations, some of whose literary productions will be found at the head of this article. In making this remark, we desire rather to do honour to the cause itself, than to detract from the merits of the earnest and philanthropic individuals who have given themselves to this labour of love; and the fact that the majority of the working members of the associations belong to our own profession, makes us only the more willing to do justice to their patriotic efforts. The cause which they have taken in hand forms one of the few exceptions to that wholesome practice which the members of the profession have, for the most part, very strictly observed, of abstaining from all political agitation. Indeed it may admit of doubt whether the movement in favour of sanitary reform can be justly placed in the category of political agitations. Such an association would degrade it from the dignity which it justly claims of a world-wide question, down to the low level of a party conflict of which England is the narrow theatre. This is not the light in which its friends regard it; and the interest which it has excited on the Continent proves that they are not peculiar in the view they take of it.

When we say that the sanitary question is not a political or party question, we do not mean to affirm that it has not divided the nation into a friendly and a hostile camp. The friends of the cause have not had the field entirely to themselves; but nevertheless the conflict which has taken place has borne a much closer resemblance to an *emeute* than to a civil war. The opposition has not come from any great party in the state, but has consisted of a series of petty local outbreaks, in which the intelligence and property of the country have taken no part, and which the press, greatly to its honour, has, with a rare unanimity, rebuked and discountenanced.

The medical press has been equally mindful of its duty; and we are merely following the example of our two esteemed predecessors, in giving to the sanitary question that prompt and respectful attention to which its great and increasing importance entitles it. The conspicuous place which the members of the medical profession will fill as officers of health, under the promised sanitary act, and the vital importance of the duties they will be called upon to perform, furnish us with additional reasons, if any were wanted, for the early and, as far as our limits will allow, minute attention which we propose to bestow upon it. The question, in fact, seems to have arrived at that stage, in which a deliberate and dispassionate consideration of its merits is peculiarly to be desired, both for its own sake, and in order to point out the exact extent to which it is worthy of the regards and countenance of the medical profession.

A popular question is always in danger of being indiscreetly advocated. Appeals to public sympathy are rarely free from exaggeration, and statements which harmonise with preconceived opinions are too apt to pass current as undoubted facts. The greatest service may be rendered to such a cause by a calm and searching inquiry into its real merits. If it comes out from the fiery ordeal with its weight undiminished and its lustre untarnished, it is henceforth received as current coin of the realm of truth. If it will not bear the trial, it is thrown aside as worthless. But if, as more frequently happens, it is proved to be an alloy of noble and base metal, of truth and error, that which has stood the proof still retains its value, and when reissued is received with a confidence proportioned to the rigour of the test to which it had been submitted.

It is not to be expected that the sanitary question should form a complete exception to a rule based upon experience, and in harmony with the nature of things. We are fully prepared for exaggerated estimates, and calculations founded upon erroneous data; but we believe that the inquiry into which we are about to enter will issue in a rebuke to those sceptics, both in and out of the profession, who have somewhat hastily classed the sanitary movement with other outbursts of quackery, who have charged it with exaggeration beyond the limits of what is natural and excusable, and who have been forward to swell the parrot-cry that "anything may be proved by figures." As we are of the number of those who think that nothing can be disproved without them, we shall have to trespass somewhat on the patience of those who set at nought these valued servants of the statist, while we examine sanitary estimates by the aid of numerical calculations.

At first sight, and to careless observation, the figures put forward by the advocates of sanitary reform wear an appearance of gross exaggeration. Premature deaths count by tens of thousands, attacks of unnecessary sickness by hundreds of thousands, and waste or misappropriation of money by millions; while the moral concomitants and consequences of wasted health and life exhaust all the resources of eloquence, and all the figures and artifices of rhetoric.

And yet, when we reflect calmly upon the matter, and call to mind that the units which make up these sums form but a fraction of the sixteen millions of inhabitants of England and Wales, and but a small proportion of the deaths and attacks of sickness which take place every year among them; and when we further consider the large totals of money displayed in the revenue tables as the aggregates of taxes and charges utterly insignificant in amount, of which the Post Office income is a good example, we shall not be disposed to charge the estimates of the sanitary reformer with exaggeration, simply because they too deal with tens and hundreds of thousands.

Having, by such considerations as these, prepared ourselves to approach the estimates and calculations of the advocates of sanitary reform with minds free from any bias towards incredulity, but, at the same time, with a determination to submit them to a rigorous examination, we will endeavour to set forth in a few words the broad facts and truths which form the motives of this new appeal to popular sympathy, and the materials of the arguments by which that appeal is supported and enforced.

In the publications of the Health of Towns Association, as in the Sanitary Reports to which their authors are so largely indebted, the sanitary question is put forward, first, as a great physical question, in other words, as a question of health and life; secondly, as an economical question, affecting the pecuniary interests of the community; and, lastly, as a moral question. It is in the first of these aspects that it possesses the greatest attraction for the members of the medical profession, and accordingly we shall enter at length into this division of the subject; but, at the same time, we feel that we should be giving but an imperfect idea of the merits of the whole question, did we not briefly advert to its economical and moral bearings. As, moreover, the subject has already engaged the attention of the Legislature, and will soon be again before them in a substantive form, it requires to be considered in its proposed relations to the state. To this part of the sanitary question, if our space will allow of it, we propose also to refer.

In treating of the sanitary question as a physical question, the point which has been most insisted upon is the unhealthiness of towns as compared with rural districts, and the consequent loss of health and life which the inhabitants of towns are doomed to suffer. The precise amount of this loss has been made the subject of calculation, both for the largest towns of England, and for the entire population of the several parts of the United Kingdom. An attempt has also been made to estimate the attacks of unnecessary sickness which accompany this sacrifice of life. The figures put forth are, as we shall presently see, of a magnitude to justify, and even to demand, the most cautious and anxious scrutiny; for if they should be found to represent the real state of the case, we see not how any man's conscience can be at ease who does not render some assistance towards a speedy and radical reformation.

It will be most convenient to begin our examination of the estimates put forward by the advocates of sanitary reform, with those which relate to the *aggregate annual sacrifice of life* in England and Wales, over and above what may be considered a healthy and natural standard of mortality. This standard is assumed in the Reports of the Registrar-General, the first Report of the Health of Towns Commission, and several publications of the Health of Towns Association, at *two per cent. per annum.* Is this a reasonable and moderate standard? If we can answer this question in the affirmative, the annual sacrifice of life in England and Wales will be easy of calculation, and it will not be difficult to form an approximate estimate for the United Kingdom.

The Fifth Annual Report of the Registrar-General (pp. xlii-viii) supplies us with ample materials for the solution of this question, by giving the mortality per cent. for the year 1841, in the statistical districts of all the counties of England and Wales, for males and females respectively, and for equal numbers of the two sexes. Taking the column which represents the joint mortality of the two sexes, we have arranged in classes all the registration districts of England in which the mortality is two per cent. or under; and these are the results which we obtain. In 88 districts the mortality was exactly two per cent., 20 in the thousand, or one in 50; in 52 districts it was 19 in the thousand, or one in 53; in 94 districts it was 18 in the thousand, or one in 56; in 78 districts it was 17 in the thousand, or one in 59; in 39 districts it was 16 in the thousand, or one in 63; and in 15 districts as low as 15 in the thousand, or one in 67. So that in no less than 366 out of a total of 587 registration districts, or little less than two thirds of the whole number, the mortality in the year 1841 did not exceed *two per cent.;* and in 278 districts, or nearly one half the number, fell short of that rate. But though so considerable a proportion of all the registration districts of England and Wales presented this moderate or low rate of mortality, it by no means follows that we are justified in assuming *two per cent.* as a standard of comparison for the whole of England and Wales. The districts so favorably circumstanced might turn out upon examination to be principally, if not exclusively, rural districts; and as between five and six millions of the inhabitants of England and Wales inhabit town districts, in which, as a general rule, a high mortality is known to prevail, the favourable figures just adduced would prove nothing as to the right of the inhabitants generally to claim so low a rate of mortality as that assumed by the champions of sanitary reform. In order to establish this claim to improved health and prolonged life, it would be necessary to show that a fair proportion at least of the 366 districts consist of towns of considerable size, or, at least, comprise such towns within their limits.

On referring to the group of districts in which the mortality is 20 in the thousand, it appears on a cursory examination that no less than sixteen of the districts in question are towns with a population ranging from upwards of 10,000 to upwards of 37,000, and having an aggregate population of upwards of a quarter of a million. The districts which have a mortality of 19 in the thousand comprise one town of upwards of 10,000 inhabitants; those in which the mortality is 18 in the thousand have one exceeding 10,000, and a second above 12,000; those in which the mortality is 17 in the thousand have one town, the population of which exceeds 10,000; and in the group of districts having the favorable mortality of 16 in the thousand, there is one town with a population of upwards of 10,000, a second with nearly 12,000, and a third of nearly

17,000.* The total population of the towns which have a mortality of 20 in the thousand, or less, is 359,570. Among the more populous of these towns may be cited Chesterfield, Dewsbury, Dover, Hastings, Halifax, Huddersfield, Ipswich, King's Lynn, Kidderminster, Maidstone, Ormskirk, Plymouth, Swansea, and Taunton.

Now these facts appear to us to justify most completely the assumption of two per cent. as the point to which the mortality of England might be reduced by sound sanitary measures. If it had happened that the districts in which this or a more favorable rate of mortality obtained were exclusively rural, it might have been alleged that the high mortality of towns, by overpowering the more favorable rate of those districts, rendered any material improvement for the whole country hopeless. As it is, however, a mortality of two per cent. for the whole of England and Wales is not only a possibility, but an event in the highest degree probable; and to which we are justified in looking forward with confidence as the ultimate effect of a sound sanitary measure.

The first assumption of the friends of sanitary reform, to which we have had occasion to refer, being thus justified by the most rigorous examination to which it appeared possible to submit it, the estimates founded upon it are such simple matters of calculation as to present little or no room for error. Faithful, however, to the principle with which we set out, and determined to spare no pains to test the calculations of the sanitary statist, we proceed to examine the figures which have been put forward as representing the annual waste of life in England.

The estimates founded upon this basis of two per cent. give an annual sacrifice of life in England and Wales of 35,000, and in the United Kingdom of upwards of 60,000, the rate of mortality of England and Wales being taken at one in 45. As this rate, however, though a near approximation, and sufficiently exact for general purposes, is not the precise rate which has obtained in any one year, nor even a precise average of any term of years, let us enter into a more strict calculation, and endeavour to determine the true waste of human life on the supposition that the rate of mortality to be attained by sanitary measures will not exceed two per cent.

By referring to the Seventh Annual Report of the Registrar-General (pp. 3-4), it will be seen that the deaths in England and Wales for the year 1841, the year of the census, were 343,847, and the corrected population for the same year amounted to 15,912,773. The rate of mortality for that year was accordingly more favorable than that upon which the calculation of 35,000 unnecessary deaths is founded, being one in 46·28. A simple calculation will show that the excess of deaths over two per cent. for that year, instead of being, in round numbers, 35,000, was only 25,582.

But if, instead of taking a single year, we take a series of years, calculate the mean mortality for the entire period, and deduce from that the average annual waste of life, we obtain results more nearly approximating to those put forward in the publications of the Health of Towns Association. In the Report of the Registrar-General to which we have just referred, the deaths in England and Wales are given for each of the seven years

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* These figures have been extracted from the population returns for 1841, the population being that of the *towns* themselves, and not of the parishes of which they form a part.

1838-44, of which the year of the census is the middle term; and assuming the population of 1841 to be the mean of the whole seven years, we obtain an annual mortality of one in 45·69, and an annual excess above two per cent. of 30,021. These rough estimates of the sanitary statist, therefore, when submitted to close scrutiny, and compared with the most accurate calculations, are not doomed to suffer any very material abatement, and for popular purposes may be looked upon as sufficiently exact.

In these and similar calculations there is always much room for discrepancy, as well as valid excuse for exaggeration, in the variable mortality of different years. Thus if we take the seven years 1838-44, we find one year (1839) in which the number of deaths falls as low as 338,979, and another year (1840) in which it rises as high as 359,634. Between these two successive years, therefore, there is a difference of no less than 20,655 deaths. The rate of mortality in the first named year is as low as one in 45·69, and in the last as high as one in 43·35; and the excess above two per cent. in the one case is only 29,220, and in the other (the calculation being in each case based on the estimated population of the year) 47,831.

If we assume that the excess above two per cent. in Scotland and Ireland, which do not yet enjoy the advantage of a sound system of registration, is not greater than in England and Wales, the total sacrifice of life in the United Kingdom in the year 1841 would amount to 43,599; on the average of the seven years 1838-44, it would be 51,059; in the year 1839, 50,024; and in the year 1840, 74,423. Taking the average of the seven years 1838-44 as least open to objection, we have in round numbers an annual excess above two per cent. in England and Wales of 30,000 deaths, and in the United Kingdom of 51,000.

Now these are startling figures; and those who make them the instruments of a loud appeal to the Legislature, on behalf of that large and helpless class on which the evils of national negligence are wont to fall with crushing weight, have demonstrably a case of unusual strength and soundness, and may be acquitted of any exaggeration beyond that which arises from substituting round numbers for more minute and exact calculation.

Thus far, then, we are disposed to go hand in hand with the advocates of sanitary reform. Their statements have stood the test of as strict an examination as we had the means of instituting. But we must confess our misgivings as to the soundness of the standard which the Health of Towns Association have adopted in a collection of tables recently issued under the title of 'Tables of the Vital Statistics of England and Wales.' That standard consists in the average age at death of the inhabitants of the most healthy registration district of each county of England and Wales; such healthy district being used as the measure of the sanitary condition of the entire county. Taking the deaths in 1841 as the starting point, these tables are made to show an annual waste in England and Wales of upwards of 57,000 lives. In justice to the Health of Towns Association, however, it is but right to state that in the recent quarterly returns of the Registrar-General the very same standard is employed; so that if they are in error, they err in company with the very highest authority on such matters. In comparing the registration district of Ulverstone with

Manchester and Liverpool, the Health of Towns Association are countenanced by the comparison of the registration district of Lewisham with the metropolis at large. With all respect for these high authorities, and every disposition to find and to use the strongest arguments in favour of sanitary reform, we cannot but express a doubt whether, even if a sound, comprehensive, and really practical measure were brought into universal operation, the inhabitants of our large towns and town districts could be made partakers of the same degree of health or the same favorable duration of life, as those whose privilege it is to live in the thinly-peopled environs or in the rural districts. Even if the air of our populous towns could be rendered as pure, the water supply as abundant, the cleansing and drainage perfect, and the existing over-crowding of houses, shops, and workshops could be diminished to that point at which no injury should be inflicted on the health of their occupants, we still think that the sedentary and necessarily unhealthy occupations of the mass of their inhabitants would entail a rate of mortality much higher than that which exists among a population whose occupations are of a more healthy and invigorating character. We should, therefore, be disposed to prefer, as less open to objection and cavil, the standard of two per cent.; in the full assurance that every intelligent, prudent, benevolent, and Christian man will find all the motive to exertion which he can require, in an annual sacrifice of 30,000 lives in England and Wales, and upwards of 50,000 in the United Kingdom.

But in expressing some misgiving respecting a mode of estimating the annual sacrifice of life, which would raise the unnecessary deaths occurring every year in England and Wales to the enormous amount of upwards of 57,000, or nearly double the average of seven years calculated on a basis of two per cent., we would not willingly overlook the arguments which may be advanced, if not in favour of the standard of the Registrar-General, at least of some measure intermediate between two per cent. and the deaths occurring in the most healthy districts of each county.

One of the arguments which may be advanced for a more favorable standard than two per cent., is supplied by the facts to which we have already referred. If, in little less than two thirds of all the registration districts of England and Wales, the mortality in the year 1841 did not exceed two per cent., how strong the probability that the remaining districts admit of being brought to that healthy standard! When, moreover, it is borne in mind that no less than twenty-four towns, with populations exceeding 10,000, and an aggregate population of upwards of 350,000, have a rate of mortality of two per cent. or less, in spite of many circumstances unfavorable to health which exist, to a greater or less extent, in all of them, we can scarcely avoid the conclusion that the mortality of all towns might, by proper sanitary measures, be reduced to the healthy standard of two per cent.; and that the rural districts, by similar precautions, might be so much improved as to lower the average mortality for the whole country considerably below the moderate standard which we have assumed.

Another argument for a more favorable standard of comparison and calculation for the United Kingdom than two per cent., is supplied to us by two tables, which will be found at pp. 34-5 of the Seventh Annual Report of the Registrar-General; from which it appears, that on an average of seven years the rate of mortality in three out of the eleven divisions of England and Wales,—namely, the south-eastern, the south-western, and the Welsh divisions,—the mortality was less than two per cent.; and in three others (namely, the north- and south-midland and the eastern) it very little exceeded that rate; and that out of the 43 counties of England and Wales, no less than 19, or nearly one-half, had a mortality of two per cent. or under. North Wales and part of Surrey had a mortality of only 1 in 55, or 18 in the thousand. Now, when it is considered that these counties comprise towns and villages as well as scattered rural residences, and that many of these towns and villages are acknowledgedly subject to much preventible disease, especially to typhus fever (which, for the same amount of population, is little less destructive in rural than in town districts), it must be admitted that we have another strong presumption in favour of the ultimate attainment of a lower rate of mortality for the entire kingdom than two per cent.

We are inclined, therefore, to believe that in assuming a standard of two per cent. as a rate of mortality which the whole population of this country may fairly expect to enjoy, and as a basis for calculating the annual waste of life, the promoters of sanitary reform have taken up a very safe and tenable position; and that there is fair ground for assuming a yet lower rate of mortality and a consequent greater annual sacrifice of life.

But the fact upon which the advocates of sanitary reform mainly insist, is *the extreme unhealthiness of large cities when compared with rural districts*,—a fact which is beyond dispute, and concerning which the only question is one of degree. Here, too, a standard of comparison has been assumed, which requires to be submitted to a searching inquiry; namely, the same standard of two per cent., which, as applied to the entire country, we have already justified by an appeal to facts. Dr. Lyon Playfair for Manchester, and the Registrar-General for London, have also employed the rate of mortality prevailing in healthy districts in the same county or neighbourhood.

Let us first examine the standard rate of two per cent., as applied to the mortality of large towns. Are we justified in assuming so favorable a rate as of possible attainment in our large cities, and in using it as the basis of our calculations of the annual sacrifice of life which takes place in them? This question might at first sight appear to have been satisfactorily answered by the facts already adduced. If in no less than twenty-four towns, with populations ranging from 10,000 to nearly 40,000, a rate of mortality of two per cent. or under actually obtains, have we not a very strong presumption in favour of the possibility of reducing the mortality even of the largest and most unhealthy cities to that favorable rate? We must not answer this question in the affirmative, without taking into consideration a circumstance to which our attention has lately been forcibly directed; namely, the accumulation within them of a young population, subject, under the most favorable circumstances, and simply because it *is* a young population, to a high rate of mortality.

The only perfectly satisfactory way of determining whether or not the advocates of sanitary reform are justified in assuming a mortality of two per cent. as of possible attainment in all our large cities, is to suppose the population of London, Liverpool, or Manchester, with its peculiar distribution of ages, to be transferred to some town of considerable size, having a mortality of two per cent. or under, and to be subject to the mortality for each age which prevails in that town. This mode of proceeding is pointed out and amply illustrated by striking examples in a paper published by Mr. Neison, in the 'Quarterly Journal of the Statistical Society,' for April 1844. At page 46 of that number of the Journal a table is given, which is based on the supposition that the population of the metropolis is transferred to some of our counties and principal cities, subject to the rate of mortality for each age actually obtaining in those counties or cities. From the table it appears that the actual rate of mortality in the metropolis is one in 39·10; but if its population were transferred to the county of Suffolk, on the terms now specified, that rate would fall to one in 54·57, or considerably less than two per cent.; if transferred to Essex, it would fall still further, to one in 56·34; if to Norfolk, to one in 56·38; if to Devonshire, to one in 66·57; if to Herefordshire, to one in 68·49. The unhealthiness of large cities is thus set forth in a very striking manner, and is rendered only the more apparent by the severity of the test to which it is submitted.—But a still more startling result is brought out, when the population of the metropolis is supposed to be transferred to some of our most populous cities. The actual rate of mortality in London being, as we have just stated, one in 39·10, it would become in Sheffield one in 29·28, in Liverpool one in 34·92, in Leeds one in 35·44, in Manchester one in 39·93, in Exeter one in 41·79, and in Birmingham one in 50·63, or less than two per cent. So that we have a manufacturing town of nearly 140,000 inhabitants enjoying so good a sanitary condition, that if the population of the metropolis were transferred to it, subject for each age to the rate of mortality existing there, it would not lose one inhabitant in 50, or two per cent. in the year. It is scarcely possible to imagine or desire a more satisfactory proof, that, by proper sanitary measures, the sickly populations of large cities may be made to enjoy a state of health represented by the favorable rate of mortality of two per cent. The calculation to which we now refer is based, it is true, upon the deaths for the single year ending June 1841; and it would probably be slightly varied, if a term of years were taken instead of a single year. We are inclined, however, for reasons which it would occupy too much space to specify, to the opinion that the result of this single year would not differ materially from that of the average of the seven years, from 1838 to 1844 inclusive.

The actual rate of mortality in Birmingham for the year ending June 1841, was one in 48·65, or less than 21 in the thousand—a fact which establishes the strongest probability, that by due attention to sanitary measures, the mortality of all large cities might be reduced to one in 50, or two per cent. Birmingham, indeed, may be put prominently forward in contrast with Manchester and Liverpool, as a standing argument in favour of sanitary reform. Birmingham stands high above the level of the sea, upon an undulating surface of red sandstone and gravel, and is far better drained by nature than most towns are by art; it has no cellar residences; its courts, which are few in number, are unusually spacious; most of the streets inhabited by the working classes open out of the main thoroughfares; and almost every family has a house to itself. In all these points, which are of such acknowledged importance to health, Birmingham contrasts most favorably with Manchester and Liverpool, especially with the latter town, which, for imperfect drainage, over-crowding, and the wretched condition of the habitations of the poor, has become notorious above all the cities of England. Between these two extremes of good and bad structural arrangements for the accommodation of the working classes, the metropolis takes its place; and accordingly its rate of mortality is intermediate; for its population in the year ending June, 1841, had a rate of mortality of one in 39, but that same population would have perished in Liverpool in the proportion of one in 35; while in Birmingham, the rate of mortality would have reached the very favorable point of less than one in 50. If the drainage of London and Liverpool were made by artificial means to equal the good natural drainage of Birmingham, and the streets and residences of the labouring classes were laid out on the same excellent principles, there is no reason why these two unhealthy cities should not be made to assume as fair a sanitary condition as Birmingham itself.

The considerations into which we have just entered would seem to furnish a very complete justification of the assumption of two per cent. as a fair standard rate of mortality for all towns and populous places, and of a still more favorable rate for the country at large. Whether the standard can be safely raised to the level of the most healthy registration districts of the several counties, is a question, which we have no means of answering in a satisfactory manner; though we have certainly no sufficient ground for charging the Registrar-General and the Health of Towns Association with exaggeration in adopting the standard in question.

The next test of the sanitary condition of different counties, districts, or towns which, in pursuance of our purpose, we are called upon to examine, is the *average age at death.* Much use has been made of this test by the advocates of sanitary reform, and it is, therefore, doubly important to submit it to careful scrutiny. Is it a sound test? To this question we are constrained to give a decided negative. We are not now speaking of those extreme cases in which the fallacy is obvious at a glance; where a large assemblage of children or old men gives a low or high average age, as the case may be, to the locality in which it is situated. No one for instance can fail to perceive the gross fallacy of comparing the immediate locality of the Foundling Hospital, including the charity itself, with the district surrounding and embracing Greenwich Hospital. As Mr. Chadwick has well observed, the death of one man aged 50 years will have the same effect on the average age at death, as the decease of 50 children a year old. This would be the case even if the infant were not subject to a greater mortality than the adult of 50. But when the high mortality of infancy is combined with the low figure of age, the effect on the average must be very striking; and we shall not be surprised to find that even such a difference of distribution in respect of age as prevails in different countries, provinces, or towns, will suffice to produce a marked disparity in the average age at death, even where the rate of mortality does not materially differ.

On account of the importance which attaches to the average age at death, as a test of the sanitary condition of a population, we shall make no apology for entering into it somewhat minutely, and drawing rather largely from the striking facts adduced by Mr. Neison in the Essay already referred to.

The rate of mortality in England and the United States of America differs very slightly, being one in 44·55, and one in 44·60 respectively, or in both cases, about 22 in the thousand; but the average age at death is in England 29 years, and in America only 20 years. This striking difference in the age at death is obviously due, not to the superior sanitary condition of the English population, but to its greater age. The difference between the two populations in respect to age is well shown in the following statement, "that whilst in England there are 5025 persons between 15 and 50, who have 3610 children or persons under 15, in America there are 4789 persons living between 15 and 50 years of age, who have 4371 children dependent upon them. In England there are in every 10,000 persons 1365 who have obtained above 50 years' experience; in America there are only 830."

Again, if we compare England and Wales in 1841 and 1821, we arrive at the remarkable result that though the rate of mortality differs in a very insignificant degree, the average age at death, if the mortality of the several ages for the two periods be assumed to be that of 1841, differs by little less than five years; the average age for 1821 being 24·89 years, and for 1841, 29·46 years.

The same striking differences are observable if we turn to the table already laid under contribution, which contrasts the metropolis with the counties and large towns of England, assuming the population of the metropolis to be transferred to those counties and towns. The average age at death in the metropolis is 29·06, and in the county of Hereford, 38·42; but if the population of London were placed under the sanitary influences of Herefordshire, the average age at death would become 30·54; in other words, the inhabitants of the metropolis, one with another, would have about a year and a half added to their lives. Or, suppose the county of Devon to take the place of the county of Hereford. The average age of death in Devonshire is 37·97, or nine years more than in London; but the inhabitants of the metropolis, if transferred to Devonshire, would have the average age of 31·48; that is to say, they would gain, one with another, less than two and a half years.

Let us now compare the metropolis with other English cities. Birmingham, it will be recollected, when measured by the rate of mortality, proved much more healthy than London; but nevertheless the average age at death, which in the metropolis is 29 years, in Birmingham is less than 24 years. The population of London, however, if transferred to Birmingham, would die at the average age of nearly 27 years. In Manchester, again, the average age at death is less than 23 years, though its rate of mortality for the year 1840-1 was somewhat more favorable than that of London. So also in the case of Liverpool; if we took the average age at death as our sole guide, London would appear far more healthy than Liverpool, for while the average age at death in London is 29 years, it is less than 21 in Liverpool, being a difference of no less than eight years, whereas the population of London, if transferred to Liverpool, would die at an average age of 25 years, that is to say, the difference would be reduced from four to eight years.

We have still, however, in reserve the most interesting comparison of all—that of the several parts of the metropolis itself. Bethnal Green has been pointed out as the most unhealthy district in London, because the average age at death happens to be lower than elsewhere. It is less than 26 years; whereas it is upwards of 26 in Clerkenwell, 28 in St. Giles's and St. George's Bloomsbury, 29 in Marylebone, 31½ in St. George's Hanover square, and upwards of 32 in Kensington. But this order is materially changed when we equalize the distribution of the population according to age in these several districts; and it turns out that, for the same population, St. Giles's and St. George's Bloomsbury is the most unhealthy, having an average age at death of 24·34 years. Marylebone comes next in order, with an average age of 24·52; then Clerkenwell, with an average of 24·84; then Bethnal Green, with an average of 25·80. The average for Kensington is 26·71, and for St. George's Hanover square, exclusive of the hospital, 28·13. The rate of mortality follows precisely the same order. The average age at death, therefore, cannot be taken as a sound or safe measure of the sanitary condition of a population. The use of it may lead to very serious errors. The same remark applies, though in a less degree, to the rate of mortality; for it stands to reason that one death in 30, occurring among a population of young persons under 25 years of age, would represent a much more favorable state of things than the same rate of mortality in a population from 25 and 50 years. There is, therefore, no other perfectly satisfactory test but that which takes into account both the ages of the living and the age at death, or which has the properties, if not the exact form, of a life table. Mr. Neison's method of supposing the actual population of one county, province, or town, with its own peculiar distribution of ages, transferred to another locality, and subject to the mortality at each age of the inhabitants of that locality, is equally sound, and leads to very striking and satisfactory results.

In thus assuming the average age at death, taken alone, as a sanitary test, an unintentional error has been committed; but in most cases it is merely an error of degree, exaggerating the difference between one city and another, but rarely affecting their relative position in the sanitary scale, unless the difference in the average age at death is inconsiderable. For example, the average age at death is greater in London than in Liverpool, Manchester, Leeds, Birmingham, or Sheffield; and the average age of death keeps the same relative situation, when the population of London is supposed to be transferred to those cities. On the other hand, the districts of the metropolis do not retain the same relative situations when measured by the two tests; for the average age at death, taken by itself, would place them in the following order, beginning with the lowest average:—Bethnal Green, Clerkenwell, St. Giles's and St. George's Bloomsbury, Marylebone, St. George's Hanover square, and Kensington; while the more correct method would lead to their transposition as follows: St. Giles's and St. George's Bloomsbury, Marylebone, Clerkenwell, Bethnal Green, Kensington, and St. George's Hanover square.

If the observations which we have made on the average age at death, taken by itself, and employed as a sanitary test, are well founded, it follows that the estimates of the years of life lost by the inhabitants of our large cities or by the community at large are too high, and must submit to considerable reduction. The exact extent of the exaggeration, to which this mode of estimating the years lost to the community leads, cannot be shown without long and tedious calculations; but some idea may be formed of it by again referring to Mr. Neison's tables. The most healthy of the districts comprised in the table for the metropolis is Kensington, in which the average age at death, for the year ending June, 1841, is 32·39. The least healthy, according to the same rude test, is Bethnal Green, where the average age at death for the same year was 25·80. Here is a difference of nearly six and a half years; but if the same population which then inhabited Bethnal Green had been subject to the rate of mortality prevailing in Kensington, the average loss of life, instead of being six years and a half, would have been only one year. The exact figures are—Bethnal Green 25·80, Kensington 26·71. So, also, if we compare London with Liverpool. The loss of life in Liverpool, estimated by the rough test which we are considering, would be about eight and a quarter years; but the actual loss of life due to the unhealthy condition of Liverpool, compared with London, would be as nearly as possible four years, or less than half the amount. The kind of reduction to which estimates of the loss of life sustained by the inhabitants of our large cities, or of parts of them, would have to submit, if the proper correction were applied to the figures founded on this rough and incorrect measure, will be best represented by comparing the total of years of life wasted every year in Bethnal Green and Liverpool, when compared with Kensington and London respectively. The number of deaths which took place in the Bethnal Green district in 1840–1 was 1764, which, multiplied by six and a half (the number of years of life lost, on an average, to each inhabitant by the rough test of the average age at death), gives a total of 11,466 years; but the real sacrifice of life due to the inferior sanitary condition of Bethnal Green, compared with Kensington, is only 1764 years. A similar, though not an equal, reduction must be made in the case of Liverpool, in which the mortality in the years 1840–1 was [illegible]119. This number, multiplied by eight and a quarter (the mean loss to each inhabitant when measured by the average age at death), gives 66,981, instead of 32,476 (the loss which would be sustained by a population the exact counterpart of that existing in the standard city itself).

The employment of the average age at death, then, as a test of the sanitary condition of a population, and as a measure of the loss of life sustained by it, is open to serious objection, and must inevitably lead to much exaggeration. But still the sacrifice of life in Bethnal Green, as compared with Kensington, and in Liverpool, as compared with London, remains a serious matter. These 1764 years of wasted life are worth an anxious thought. With all the expense, and sorrow, and waste of time and labour which they represent, they might have been saved to the community. Liverpool must have paid many a heavy and unsuspected tax for these 32,476 years of life of which, year by year, her inhabitants have been robbed. Let the inhabitants of the metropolis, who have hitherto remained indifferent to these and similar facts, ask themselves how they would like to be transferred to a city where each of its more favoured inhabitants must sacrifice, one with another, four years of life, or—ere an entire generation of its busy population had passed away—8,000,000 years. If this is the cost at which London would purchase the unsound sanitary condition of Liverpool, its people may rest assured that it is at no less an outlay of life that they have bought the luxury of negligence and local self-government. The superiority of London over Liverpool is assuredly not greater than its inferiority to a really healthy standard; though, for obvious reasons, it is not easy to determine what that standard is to be.

From what has been now stated, it must be obvious that a low average age at death is generally to be taken as the result of one of two circumstances, a young population, and exposure to unhealthy influences. The two are sometimes, but by no means uniformly, found in combination. Thus, if we compare the district of St. Giles's and St. George's Bloomsbury, which is characterized by a low average age at death, with St. George's Hanover square, which is more favorably circumstanced, we find that the number of children under five years of age in the former is 103 per thousand, and in the latter only 86; while the numbers under 20 are 358 and 310 respectively. So, also, if we contrast London with Liverpool; the former has 118 children under 5 years of age, and the latter 132; the number under 20 being 399 in the one, and 424 in the other. On the other hand, if we compare Marylebone and Kensington, of which the former is decidedly the least healthy, we have a population, under 5 years of age, of 104 in the former, and 114 in the latter; and under 20 years of age, of 350 in the former, and 401 in the latter. So that in this case the order of things is inverted; the healthy district having the largest number of children and young persons, and the unhealthy district the smallest number of both. A comparison of the metropolis and the county of Hereford yields the same result; for the children under 5 years of age are in London 118, and in Herefordshire 120; and young persons under 20 years are 399 in the one, and 438 in the other. It would appear, therefore, that the ages of populations are much less dependent than some have been inclined to suppose upon the sanitary condition of the district or town in which they live; and this circumstance prepares us to find another general principle laid down by the advocates of sanitary reform,—namely, the coincidence of a large number of births with an excessive mortality,—le[illegible] firmly established than they have represented it to be.

Mr. Neison, in the essay so often referred to, glances at this question; and, with a view to its solution, contrasts the county of Hereford with the metropolis. The average age at death in the metropolis is 29 years, in Herefordshire it is nearly 38½ years; the rate of mortality in the former is one in 39, in the latter one in 58; and the proportion of births to the population is respectively one to 37, and one to 44. Here, then, we have that coincidence of a high mortality with a rapid reproduction, which has been so much insisted on. But here, too, Mr. Neison advances his old objection, that the ages of the population must be taken into the account; and it is obvious that he is right. We must take the number of the reproductive population in London and the county of Hereford, as we have already taken the number of inhabitants of different ages, in the comparisons we have instituted in reference to the average age at death. Now when we apply this very necessary correction, we discover somewhat to our surprise and to the discomfiture of the sanitary reformer, that the healthy population produces the greatest number of children in proportion to its reproductive members. The population in Herefordshire, from 20 to 40 years of age, is 284 in the thousand; but in the metropolis it is 363 in the thousand. A very simple calculation will show that the births in the metropolis, in order to be on a par with those in Herefordshire, instead of being one in 37, ought to be one in 34·38. There is, therefore, in the unhealthy metropolis an actual deficiency of births when compared with the comparatively healthy county of Hereford. Again, if we compare the metropolis with Liverpool, we arrive at a similar result. The population of the metropolis, between the ages of 20 and 40, in 1841, was 680,816, and the births for the same year amounted to 57,342; but in Liverpool, with a reproductive population of 110,535, the number of births was only 7735; whereas, had the births borne as high a proportion to the reproductive population in Liverpool as in the more healthy metropolis, they would have amounted to 9308. As a last comparison, let us take Birmingham and London. The reproductive population of Birmingham in 1841 was 61,811, and the births 5031; the reproductive population of London was 680,816, and the births 57,342; but had the addition made to the population of London been at the same rate as in Birmingham, the births would only have been 55,414. So that in this case the unhealthy city proved proportionably more productive than the healthy one.

These cases are sufficient to prove that the rule laid down by the advocates of sanitary reform, that the births keep pace with the mortality, is not to be received as a general truth. It is at the best only an occasional coincidence, and cannot be safely employed as an argument in favour of sanitary measures. All that can be justly advanced is, that an excessive mortality is *sometimes* accompanied by a proportional reproduction; and that in consequence, partly of immigration, and partly of a fecundity not materially impaired by unwholesome influences, the very worst districts of our large towns contrive to maintain their numbers, and often to increase with great rapidity.

When the correction which we have just insisted on, namely, the number of the reproductive population, is not taken into account, an excess of children seems to be a very general concomitant of an extreme mortality. Thus, in an essay by Mr. Chadwick, in the 'Statistical Journal' for April, 1844, a table is given from the returns of the Registrar-General, in which the several sub-districts of the metropolis are divided into five groups, under the titles of the unhealthiest, less unhealthy, average, healthier, and healthiest sub-districts; and in this table the proportion of deaths to births certainly follows, though not with much regularity, the rule just laid down. In the unhealthiest sub-districts, for example, the proportion of deaths to births is 1 to 1·17; in the less unhealthy 1 to 1·19; in the average 1 to 1·38; in the healthier 1 to 1.22; and in the healthiest 1 to 1·32. The fact that the healthier sub-districts approximate so closely to the two unhealthiest as 1·22 to 1·17 and 1·19 respectively, would seem to show that the law so often insisted on is not of a very absolute character.

There is one sufficient cause of a high rate of mortality, accompanied by a low average age at death and a large proportion of births, in districts notoriously unfit for the residence of human beings; and that is the inevitable effect of poverty and embarrassment. Poor men with large families of young children gravitate of necessity into districts where the rent is proportioned to their means, and the accommodation consequently of the worst possible kind; and the same inducement of economy crowds the wretched lodging-houses of the same miserable localities with young adult immigrants. It is thus that the worst districts of our large towns become the seats of an excessive mortality at a low average age—a mortality attributed too exclusively to defective cleansing and drainage. Take, for instance, the parish of St. Margaret, Leicester. The average age at death in 1840 in the streets that were drained was 23½ years; in those partially drained

17½ years; in the streets that were entirely undrained 13½ years. Here, as Mr. Chadwick justly observes, the defective cleansing and drainage was not the only cause of the variation. The accumulation of a young population in the manner just pointed out doubtless contributed to that result.

In comparing the best with the worst districts of a city, it is also essential to be borne in mind that the higher classes are remarkable for their slow rate of increase, when compared with the lower class of the community. This was long since demonstrated by Sadler in his rejoinder to Malthus, and has been pointed out as occurring to a very marked extent at Paris, as shown by a comparison of the rich and fashionable with the poorer districts of that capital. The same circumstance is distinctly displayed in the tables which represent the ages of the living in the several districts of the metropolis. The population of the aristocratic parish of St. George's Hanover square, for instance, has only 86 in the thousand under five years of age, and 311 in the thousand under twenty years of age; but Bethnal Green has 145 in the thousand under five, and 476 in the thousand under twenty. St. George's and St. Giles's Bloomsbury, and Marylebone are, as might be expected from the mixed nature of their populations, nearly on a par, the one having 103 and the other 104 in the thousand under five, and both of them 358 in the thousand under twenty years of age; while Clerkenwell, with a poorer population, has 120 in the thousand under five, and 407 in the thousand under twenty. A still greater disparity would doubtless be found to exist, if the several classes were separated and compared with each other.

Another test much in use among sanitary reformers, but also open to objection, is the *average age at death of the different classes of the community*. A series of elaborate tables originally published in Mr. Chadwick's Sanitary Report, reproduced in the paper already referred to, and again in a modified form in the tables of the Health of Towns Association, present the average age at death of the several classes of the community. The disparity which is here made to appear between the several classes may be judged of by the average results obtained for the metropolis. The age at death of the gentry is 44, of tradesmen 25, and of artisans, &c., 22; and the average of all who die at 21 years and upwards in the three classes is 60, 51, and 49 respectively.

These differences are so considerable as to demand a very strict examination. The tables have about them an air of exaggeration, which is but too well calculated to furnish the objectors with weapons of attack. There must be a lurking fallacy somewhere. It does not seem very probable that either the independent or the trading class have this enormous advantage over the labouring poor. The occupation of a working man is so much more healthy than the ease and luxury of the rich, or than the sedentary confinement of the shopkeeper, that even the miserable dwellings in which the labourer lives, and the unwholesome shops and workshops in which he serves or works, are not likely to account for such startling differences. Let us then submit these tables to a searching scrutiny.

One source of fallacy in the tables of Mr. Chadwick, as in those which refer to country towns, is apparent on the most superficial inspection. The deaths in workhouses are either entirely omitted, or form a class by themselves, which class is overlooked and disregarded when the tables are employed for sanitary purposes. Mr. Chadwick's tables, for instance, comprise five classes: Gentry, Tradesmen, Artisans &c., Undescribed, and Paupers. In the tables of the Health of Towns Association, the class of Paupers is omitted. The exclusion of this class, however, must lead to great inaccuracy; for it not only forms a considerable item in the tables, but presents a very high average result. Thus, while the class of gentry and professional men numbers only 2253, the paupers are 3655 in number. So also with the undescribed classes, the tradesmen are 7682, this anomalous class 5757. Again the average ages for the whole metropolis, of the five classes, including children, is as follows: gentry, 44 years; tradesmen, 25 years; artisans &c., 22 years; undescribed, 28 years; and paupers, 52 years. Now, though we have no means of determining what proportion of the several classes has found its resting-place in the workhouse, it is probable that those belonging to the class of gentry form so small a fraction, as to be safely disregarded in the calculation of the average age at death. Not so, however, with tradesmen and artisans, to which two classes, though in very different proportions, those dying in workhouses properly belong; and the same remark probably applies to the remaining class of undescribed. We are not aware that there are any facts on record, which could guide us to a safe distribution of these two groups of paupers and undescribed among the three classes to which they all originally belonged, with the exception of those unfortunate members of society who are paupers from their very birth. We are, therefore, obliged to base our corrected calculations upon two assumptions; of which the first is, that the number of paupers and undescribed who originally belonged to the class of gentry, is too insignificant to modify to any marked degree the average for the higher classes; and the second, that the number of tradesmen in these two classes does not form a very large proportion of the whole. Seeing that the total of tradesmen is 7682, and of artisans &c., 25,930, or more than three times that number, and that the proportionate number of tradesmen who sink into the condition of paupers is very much less than that of artisans who are reduced to the same unhappy condition, we shall probably not take the number of pauper-tradesmen at too low a figure, if we assume it to be one tenth of the class of artisans &c. A calculation founded upon these assumptions gives the following average ages at death for the three leading classes including children: gentry, 44; tradesmen, 26; artisans &c., 25. Here, then, we have a close approximation of the average age of the tradesmen and artisans, but an advantage on the part of the gentry of no less than 18 and 19 years over these respective classes. Again, if we take the average age at death of all dying above 21 years, we obtain for the three classes the following numbers: gentry, 60 years; tradesmen, 51 years; artisans &c., 49 years:—but if we make the same corrections as before, we obtain the following figures: gentry, 60 years; tradesmen, 52 years; artisans &c., 52 years; so that the two latter classes are brought on an equality, and each loses 8 years when compared with the gentry.

But here, again, the fallacy already pointed out—namely, the different distribution of the ages of the living—has been overlooked. There is good reason to believe that the ages of the living in the three classes of society do not differ less than in the several districts of large towns; and that, as a general rule, the population of artisans and labourers is younger than that of tradesmen, and the population of tradesmen younger than that of gentry and professional men. Thus, if we take that district of the metropolis in which the gentry and the better class of tradesmen most abound (St. George's, Hanover square), we find only 86 in the thousand under 5 years of age, to 145 in the thousand under the same age in Bethnal Green, which contains an unusually large proportion of working men; and the deaths of children and adults respectively in the several classes are in accordance with this different distribution of the living. Again, if we examine the series of tables published by Mr. Chadwick, we do not find a single district in which the deaths of adults in the class of gentry do not greatly exceed those of children under 10 years of age; while the average for the whole metropolis gives, for the same class, one death under 10, to 3·23, or as nearly as possible 3¼, above 21. On the other hand, in the class of artisans and labourers, no less than 23 out of 32 districts present an excess of deaths under 10 years of age; and the average of the deaths under 10 is to the average above 21, as 1 to 0·89, or as about 7 to 6. The class of tradesmen occupies, as might be expected, an intermediate place, but inclining towards the proportion existing among artisans and labourers; for out of the 32 districts, there are only 9 in which the deaths under 10 exceed those above 21, though there are several instances in which they are very nearly upon an equality, and the general average gives one death under 10, to 1·06 above 21. Now it is scarcely a tenable proposition that these striking disparities in the age at death are solely due to the greater mortality to which the children of the labouring class are subject; a disproportion of deaths so great as 3¼ to 1, or, to be more exact, of about 62 to 17, can scarcely be accounted for by any other supposition, than a great disparity in the numbers of the living in the two classes of society. That this is the true explanation, will be evident to any one who will take the pains to examine a table marked C. in the returns of the Registrar-General for the quarter ending March 31, 1847; in which the deaths occurring at each period of five and ten years throughout life, are given in relation to the numbers living at those several ages. If, for example, we take the deaths in 1000 females living under 10 years of age in the metropolitan districts, the least number (91 in the thousand in Bethnal Green and Shoreditch) does not fall short of the greatest number (121 in the thousand in St. Giles's and St. George's Bloomsbury) in a proportion at all approaching the striking difference in the ages of the living in the class of gentry and artisans; while, between Bethnal Green and Shoreditch, on the one hand, and St. George's Hanover square, on the other, the difference in the rate of mortality is only that of 91 and 92, Bethnal Green and Shoreditch having slightly the advantage over their more aristocratic neighbour to the west. If we take the mortality of males, instead of that of females, Bethnal Green and St. George's Hanover square, are exactly on a par, being 100 in the thousand; and yet the difference in the number living under 10 years of age, is that between 269 (the number in Bethnal Green) and 155 (the number living at the same ages in St. George's Hanover square). Between the ages of 10 and 15 the deaths are also in excess in the aristocratic parish, but from 15 to 65 the excess is on the side of Bethnal Green, from 65 to 85 on the side of St. George's Hanover square, to be again in excess in the poorer district for all deaths above 85 years of age. So that there would appear to be an excess of deaths in districts inhabited by the higher classes (and *a priori* among the aristocracy?) under the age of 15; an increased mortality in districts inhabited by the working classes (and among those classes especially?) from 15 to 65; again, an excessive mortality, from 65 to 85 in the better districts; and, lastly, a greatly increased mortality above that age in the inferior districts. The inhabitants of the inferior districts, therefore, attain a more advanced age, and would seem to be subject to a somewhat less amount of mortality in early childhood. In the middle period of life, they appear to be subject to a greater proportional mortality; but the great excess of servants of both sexes existing in the more aristocratic parish, and who, if they fall ill, rarely die in the houses of their masters, but either in the country among their friends, or in the hospitals, introduces an element of disturbance both into the ages of the living and the mortality at different periods of life, which prevents us from laying so much stress as we should otherwise do on the results of this useful table. Still, it furnishes abundant proof that the rate of mortality of the gentry and artisan class is not that which the average age at death, taken separately, would lead us to suppose. To ascertain what that rate really is, we must turn to the life tables contained in another work of Mr. Neison's (contributions to Vital Statistics, by F. G. P. Neison, 2d edition). These tables are constructed by the aid of the two elements of the age of the living and of the dying, and contrast the experience of the Insurance offices among the higher and middle classes, with that of the Benefit Societies among the working classes of both town and country. At 10 years of age, the expectation of the higher and upper part of the middle classes of society, as determined by six different classes of observations, on male lives, is 44·27; it is 50·53 in the members of Benefit Societies inhabiting town distrcts, and 47·91 in those living in city districts (i. e. the largest towns). At 20 years of age, the expectation for the three classes is 37·40 for the first class, 42·27 for the second class, and 40·01 for the third. The expectation for the peerage at 20, is 38·47. At 50 years of age, the expectation for the first class is 19·44, for the second 19·97, and for the third 19·92. For the peerage it is only 17·92. Lastly, the expectation at 70 years of age, is 8·65 for the first class, 8·70 for the second, and 8·76 for the third, and for the peerage, 8·15. Even in Liverpool, the most unhealthy city in England, the expectation for members of benefit societies is nearly on a par with that of the higher and middle classes. Thus taking the same ages as before, the expectations for the peerage, insured lives, and the members of benefit societies in Liverpool respectively, are, for 20 years, 38·47, 37·40, and 37·95; for 50 years, 17·92, 19·44, and 17·09; and for 70 years, 8·15, 8·65, and 8·61.

With these facts before him, no candid inquirer after truth, however anxious he may be to uphold the great cause of sanitary improvement, can fail to perceive, or refuse to acknowledge, that the advocates of sanitary reform have fallen into numerous errors, and been betrayed into unintentional exaggerations. We say unintentional, because we are fully convinced of the honesty of their motives, and because precisely the same mistake has been committed by men of talent and high character, who have not been engaged in promoting the triumph of any popular cause, but simply searching after abstract truth according to the best of their ability and information. It is true that we have at present no means of separating the very worst districts of our large towns from the general population, of which they form the lowest and most neglected portion. The members of benefit societies do not inhabit the very worst parts of our large towns. They are assuredly not to be found constituting any considerable proportion of their cellar population, or of that which crowds the narrow alleys and blind courts so favorable to vice, and probably equally so to disease. It may happen that there is in these worst parts of the worst districts, an amount of disease which keeps the average mortality of the working classes so nearly on a level with that of the more favoured children of fortune, instead of allowing it to fall far below it. It may be that the extreme of filth is even more fatal to life than the extreme of luxury, and that the naturally higher mortality of the rich will furnish a more powerful argument in favour of sanitary measures, than the figures founded on an unsound basis of calculation. If, with all their immense advantages of fortune, the peerage at 30 years of age have an expectation of 9½ years less than the ill-paid, ill-clothed, and ill-fed agricultural labourer, is it not possible that, by bringing the condition of our cities as nearly as may be to that of rural districts, and the houses in which our city workmen live, the shops in which they serve, and the workshops and factories in which they labour, to a condition as much superior to that of the cottage of the agricultural labourer, as the air of our large towns is now inferior to the pure atmosphere of the country, we may be able to exhibit in the case of the civic artisan and labourer an average duration of life not merely, as now, equal or nearly so, but very far superior to that of the privileged class? We cannot release the rich man from his cook or his carriage, but we may deliver the poor man from his filth and squalor. It would be very difficult to induce the one to close his account with his wine merchant, but we might easily enlarge the dealings of the other with the water-company. We cannot hope to persuade the peerage to labour in the field, but we can, if we will, shorten the hours of work to the artisan. We cannot save the rich man's child from the tame proprieties of home, but we may provide the child of the pauper with a cleaner and healthier airing ground than the gutter in the street. But, till these things are done, the solution of the sanitary problem will be incomplete. In the meantime, we are disposed to hazard the opinion that, when they are brought about, the independent gentry will be the shortest lived of the inhabitants of our towns.

It will be seen that we are not of the number of those who would be satisfied with raising the labouring man to the low sanitary level of the rich; still less do we agree with the inference which has been drawn from the striking facts brought forward by Mr. Neison, that, because the working classes even of our large towns are on a par with the members of the aristocracy and gentry, the alleged unfavorable circumstances in which they are placed have no injurious effect upon their health. The fallacy of these views consists in the assumption that the higher classes of society constitute a healthy standard of comparison; whereas the fact is notoriously the reverse. We would especially enter our protest against an argument which we have heard couched somewhat in these terms. "You, the advocates of sanitary reform, assert that the filth and squalor with which the poor are surrounded, the foul air they breathe, the overcrowding to which they are subject, the scanty supplies of water with which they are furnished, and the absence of efficient cleansing and drainage of the streets in which they live, impair their health and shorten their lives; and yet the wealthy and independent classes, who enjoy all these things that the poor want, are at least as short-lived as they are. Filth and all its unseemly concomitants, therefore, must be unjustly accused. It is better that a man should live in a pig-stye than in a drawing-room." Reduce this argument to a syllogistic form, and a child will see the fallacy. "Men who live amid filth are unhealthy and short-lived; now the higher classes of society do not live in filth, but still are unhealthy and short-lived, therefore filth cannot impair health or shorten life." If the advocates of sanitary reform were to assert that filth was the only cause of impaired health or premature decay, and to reject the authority of Celsus, who properly attributes to luxuries the multitude of diseases with which the world is plagued and physicians perplexed, then the argument just adduced would be a valid one; but for our parts, we are firm believers in the baneful effects of the two opposite modes of life, between which, however, there is this fortunate difference, that the evils of the poor man are remediable, but those of the rich man admit of no relief, unless he can be induced to follow the not very palatable advice "to live on sixpence a-day and earn it." But even if this wholesome counsel continue to be rejected, and this somewhat severe remedy be thrown aside, we do not despair of doing something even for the rich; or, at least, for that mixed class designated in sanitary tables as *Gentry*. It appears that the class so designated attain in the metropolis 44 years, and in Liverpool only 35, being a difference of 9 years in favour of London; and as there is probably no material difference in the distribution of ages in the same class in different towns, this number may be fairly taken to represent the years of life lost by the notoriously unhealthy state of Liverpool as compared with London. By improving the general atmosphere of our towns, by introducing a more efficient drainage of the houses of the better class, and by reducing to a minimum the attacks of fever and contagious disorders among the poor, and thus arresting the cause of the fatal diseases which attack even the more favoured class, and render the profession of medicine and the duties of the clergy so dangerous, we shall doubtless succeed in lengthening the lives even of those who would appear at first sight scarcely to stand in need of sanitary improvements.

But we have yet one popular estimate of the sanitary reformer to take into consideration. We mean the *amount of unnecessary sickness* which accompanies the annual waste of life in England. Dr. Lyon Playfair, in his Report to the Health of Towns Commission on the Sanitary Condition of the large towns in Lancashire (Appendix, Part ii, p. 59), states that "out of 324,041 cases (excluding slight accidents) admitted into the Manchester Medical Institutions during 12 years, there have been 11,587 deaths, that is, about 28 cases of sickness to one death;" and he goes on, in a short summary to be found in the next page, to state that "the excess in number of cases of sickness is obtained by multiplying the excess of deaths in each district by 28." On this assumption the advocates of sanitary reform have continued to base their calculations of the annual amount of unnecessary sickness in our large towns and in the country generally; and the question naturally arises—is this a safe assumption? For our own parts we must confess that we are not quite satisfied with this standard. The assumption, when made to take the shape of an argument, would be thus expressed. In Manchester experience, there are 28 attacks of sickness for one death; and as unnecessary sickness bears a like proportion to unnecessary deaths, there are 28 cases of unnecessary sickness to every unnecessary death. The middle term is evidently an assumption, unwarranted by any facts or arguments adduced by Dr. Lyon Playfair, and certainly challenges examination; for it is quite conceivable that the unnecessary deaths may be due to certain diseases of very great fatality, or, in other words, to diseases of which the fatal cases bear a very high proportion to cases of recovery, and that the causes of these fatal diseases are not chargeable with any other forms of sickness. If, for instance, we take the extreme case of pulmonary consumption, and admit that it is not every attack of that fatal disease which destroys life, but that every person who falls a victim to it has already sustained two other milder attacks,—and further assume that a certain number of deaths from consumption are due to preventible causes, and that the antecedent attacks which did not prove fatal were equally preventible,—we have only two attacks of sickness for one death: and the instances in which a patient attacked with consumption recovers and ultimately falls a victim to some other disease are so rare, that they will not affect the calculation in any appreciable degree. This is certainly an extreme case; but the same line of argument is equally applicable to diseases of a less fatal character—to teething, convulsions, tabes mesenterica, and the contagious disorders of childhood; and to typhus fever, the most fatal malady, next to pulmonary consumption, which attacks the adult. The mortality in typhus fever varies greatly in different epidemics. It has been as high as 1 in 6, and as low as 1 in 30. But taking one year and one epidemic with another, it may be stated approximately at 1 in 10. The mortality of the natural smallpox is about 1 in 4, and of smallpox after vaccination about 1 in 10. Several of the diseases of childhood, which are certainly promoted by the impure air of our cities, are less fatal; but taking one with another the diseases which figure in the tables of the Registrar-General, one fatal case to ten recoveries may certainly be considered as a very liberal estimate. To raise this proportion to 1 in 28, will require the addition of a large number of diseases not of a fatal character, such as catarrh, chronic rheumatism, diarrhœa, and diseases of the skin, traceable to the neglect of sanitary precautions. Whether such an addition can be safely made, is a question which we have not the means of answering. The utmost, perhaps, that can be fairly conceded to the advocates of sanitary reform is, that cases of unnecessary sickness bear the same proportion to unnecessary deaths which all cases of sickness do to all deaths; though in making this concession, we are obliged to disregard the well-established fact, that the relation between sickness and death is very far from uniform. But if we are allowed to proceed on this assumption, we shall be able to test Dr. Lyon Playfair's ratio by the result of inquiries drawn from a very different source, and more worthy of confidence. A calculation founded by Mr. Neison (Contributions to Vital Statistics, p. 108) on the returns of a large number of Benefit Societies, shows that

1000 persons, from 21 to 70 years of age, would experience 274 attacks of sickness in the year, and lose 16 of their number. This gives us 17 attacks of sickness to one death. Had this calculation been extended to persons of all ages and both sexes, and made to embrace the periods of infancy, childhood, and youth, it is not at all probable that the proportion would be raised in any material degree; for though the period of infancy and early childhood are liable to frequent attacks of sickness, youth enjoys an unusual immunity from disease. Taking all these considerations into account, it is not very probable that the proportion of attacks of sickness to deaths will exceed 1 to 20. Here, then, the estimates of the sanitary reformer require to be reduced; but not to such an extent as to justify a charge of gross exaggeration, much less of intentional misrepresentation. The attacks of unnecessary sickness occurring every year in England and Wales, will have to be reduced from 980,000 (35,000 × 28) to 600,000 (30,000 × 20); and in the United Kingdom from 1,708,000 (61,000 × 28) to 1,020,000 (51,000 × 20). Even though these smaller numbers may be exaggerations, there is little room for doubt, that here, as in their estimates of the waste of life, the friends of sanitary reform have unintentionally overstated a very strong case.

The conclusions to which our examination of sanitary tests and estimates has led us, are the following:

1. That the advocates of sanitary reform are justified in assuming *two per cent.* as the rate to which the mortality of all towns, and *a fortiori* of the country at large, may, by proper sanitary measures, be reduced.
2. That there are fair grounds for assuming for the whole of the population a still more favorable rate of mortality.
3. That the estimated annual sacrifice of 35,000 lives in England and Wales, and of upwards of 60,000 in the United Kingdom, is not greatly exaggerated; and that a more moderate estimate of 30,000 for England and Wales, and 51,000 for the United Kingdom, may be very safely assumed.
4. That in measuring the waste of life in counties or towns, the standard of *two per cent.* is to be preferred, as more moderate and less open to objection, to that of the healthiest registration district.
5. That the average age at death, as well as the number out of which one will die annually, are fallacious tests of the sanitary condition of a population, and cannot be employed for that purpose without leading to serious errors.
6. That the only accurate tests are those founded jointly on the ages of the living and the ages at death, whether they take the shape of life-tables, or assume the transfer of one population with its peculiar distribution of ages to other localities, to be there subject to the mortality at each age proper to those localities.
7. That the estimates of the years of life lost by the inhabitants of towns, and by the community at large, founded on the average age at death, are greatly exaggerated; but that, when measured by the more correct standard which embraces the *ages at death*, and *the ages of the living*, this waste is still of appalling magnitude.
8. That the alleged coincidence of an excessive reproduction and a high mortality, is not borne out to the full extent of the calculations of the advocates of sanitary reform, when the ages of the reproductive population

are taken into account; but that, in consequence partly of reproduction and partly of immigration, the worst districts maintain their population, and sometimes show a very rapid rate of increase.

9. That there is no sufficient evidence that working men are subject to a higher rate of mortality than other members of society; but that, on the contrary, there is the best reason to believe that, in spite of the unfavorable influences by which they are surrounded, they are longer lived even than the gentry.

10. That this circumstance, however, does not furnish any valid argument against the injurious effects attributed to those influences; inasmuch as the causes which injure the health and shorten the lives of the two classes respectively are altogether distinct from each other.

11. That there is reason to believe that the lives of the higher classes are shortened in those towns and districts in which the mortality of the general population is high.

12. That the estimated amount of sickness, like the estimated waste of life expressed in years, has been somewhat exaggerated by the advocates of sanitary reform; that 20 cases of unnecessary sickness to one unnecessary death, is a safer proportion to assume than 28 to 1; and that the total cases of unnecessary sickness will have to be reduced accordingly.

We have thus, to the best of our ability, and with a sincere desire to ascertain the truth, even at the expense of weakening the arguments advanced in favour of the most important reform of modern times, passed in review the leading tests and estimates of the advocates of Sanitary Improvement; and we think that our readers will agree with us that in removing some of their unsound materials, and in narrowing here and there the foundations on which they have based their popular appeals, we have done good service to a good cause.

If, in consequence of the strict investigation into which we have entered, the medical profession should be induced to turn their attention to the Sanitary question, and to admit that the great truths which form the core of the matter entitle it to attentive consideration, and its advocates to encouragement and support, our object will have been answered; for we are bound to confess, that we entered upon this subject with a conviction that it would bear examination, and that, on the whole, the verdict would be decidedly in its favour.

As a further evidence of our desire to deal impartially with this question, we would return for a short time, even at the risk of wearying our readers by repetition, to the broad question opened by Mr. Neison, as to whether our towns are or are not to be considered as owing their unhealthiness to the causes assigned by the advocates of sanitary reform,—namely, deficient water-supply, defective cleansing and drainage, emanations from decaying animal and vegetable matter, the smoke nuisance, overcrowding, a neglect of ventilation, and, in a word, aerial impurity in whatever manner brought about. Mr. Neison is, as far as we are aware, the only author who has evinced a decided scepticism as to the sufficiency of these conditions to bring about the high mortality of town populations. This scepticism is not the less worthy of respectful attention, nor do the grounds of it less challenge strict examination, that Mr. Neison is not a member of the medical profession. Like Mr. Chadwick, with whom he has been at issue on this subject, he has been drawn out of his proper sphere of study and occupation to this most attractive inquiry, on certain parts of which he has succeeded in throwing much valuable light. His labours have already been noticed with approval, and his opinions canvassed with freedom, in the pages of one of our predecessors;* but we feel that we should leave unfinished the task we have set ourselves, if we did not return to opinions at once so peculiar, and so worthy of attention.

A few passages from Mr. Neison's work will serve to display the nature and extent of his scepticism.†

> "If," he says, "in any public inquiry, it should be attempted to ascribe the increased amount of sickness in the town districts to the less healthy nature of the districts, or their peculiar local influence on health, the conclusion would certainly be fallacious." And again; "Whatever sanitary regulations may be carried out for promoting the health of towns, the wide distinction between the rates of sickness and mortality in particular districts will still not disappear. The cause of that difference is beyond the reach of any sanitary measure; and, unless a change were to take place in the character and machinery of the manufactures of a town, by which the workmen would be habituated to less restrained, but more natural and complete, physical exercises, no improvement in the state of health is to be hoped for. The evils, so far as relates to health, represented to exist by some writers to so frightful an extent, and to connect themselves with inferior sewerage, filthy streets, and ill-planned houses, are certainly overstated by them." Once more: "It is not to be expected that any arrangements whatever as to the drainage and planning of streets are likely to add to the longevity of a tailor; but, if it were possible to give his frame the physical exercises of a ploughman, 20 per cent. would be added to the duration of his life. Neither is it to be thought that the plumber, painter, and glazier is to be relieved from the poison of the metallic combinations to which he is subject; nor that the clerk can inhale the fresh air, and indulge in those exercises necessary to develope his physical constitution, while he follows the drudgeries of the counting-house. It is an aggregation of these and other employments similarly conditioned, which makes up the excessive mortality of our large towns; and since it has been shown that this class of lives is also less healthy even in the country districts, and that the town populations are chiefly made up of persons following such occupations, the legitimate result to be expected is a shorter duration of life in towns, independent of any local influence on health. If improvements and changes are to be effected in the sanitary regulations of our large towns and cities, let them be at once carried out—not upon the necessity of such municipal innovations to avert a pestilential havoc in human life, but on the true merits of the question—the comforts, conveniences, and elevation of taste and moral purity thence arising." (pp. 109-10.)

If we were to be guided solely by these quotations, we should be led to suppose that Mr. Neison denies the influence of filthy houses and streets altogether; but, on turning to an earlier part of his work (p. 60), we meet with an admission, that, although "the peculiar sanitary condition of large towns has not the remarkable effect which many have supposed in shortening the duration of life, still it has some effect." It would appear, therefore, that our author attributes the unhealthy condition of large towns chiefly to the aggregation of unwholesome occupations, and in a very insignificant degree to the wretched condition of the places in which the people live.

In forming an opinion so strangely at variance with the views of the advocates of sanitary reform, and with the remarkable experience of our ships and prisons, we think that Mr. Neison has been misled, by confining his attention too exclusively to the adult male population of our towns, on the one hand, and to the members of benefit societies on the other. Had he included females and children in his inquiries, and the entire adult labouring population, instead of its more respectable and careful members, he would have arrived at very different results.

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* British and Foreign Medical Review, July, 1844, p. 199; and Vol. XXI, p. 1.

† We quote from the second edition of his Contributions to Vital Statistics, published in 1846.

We cannot better demonstrate the unsoundness of Mr. Neison's position, than by comparing the metropolis with Liverpool, by the aid of the tables already so often referred to.* The means of refutation will be found on the same page. The proportion of deaths occurring under 5 years of age to the living at the same age is, in the metropolis, 88 in the thousand, while in Liverpool it is 113 in the thousand. From 5 to 10 years of age, the numbers are 12 and 15 respectively; and from 10 to 15, 4 and 5 respectively. The total deaths under 15, in the metropolis, are 104 in the thousand; and in Liverpool, 133 in the thousand. Here then we have eliminated the influence of occupation altogether, at least in reference to children under 5 and 10 years of age; and yet we have a marked excess of mortality in Liverpool, an excess which can only be fairly attributed to the unwholesome condition of the dwellings of the labouring class, for which that city is so notorious. In other respects, there is no reason to believe that the circumstances of the Liverpool children are less favourable than those of the same class in the metropolis.

The same result is obtained, if we apply Mr. Neison's own test to the female population of the metropolis. That population, which amounted in 1841 to 966,720, lost, in the year 1840, 22,490 females of all ages; but if they had suffered the mortality of the females of Liverpool at the same ages, the deaths would have amounted to 27,412, being an excess of nearly 5000 deaths, or about 21 per cent. Here, again, the influence of occupation is eliminated; for the females of the two commercial cities do not differ materially in this respect; or if they differ at all, it is in this, that the metropolis has an excess of females following sedentary occupations.

A comparison of the mortality of adult males, in London and Liverpool respectively, furnishes, as it seems to us, a still more conclusive reply to Mr. Neison's objections. The deaths in London among the male population between 20 and 50 years of age, amounted, in the year 1840, to 5515; but had the same population been exposed to the mortality of Liverpool, it would have lost 5642, or 127 more. Now, we contend that the occupations of the male inhabitants of Liverpool are more healthy than those of the male inhabitants of London. In Liverpool, there is a great demand for out-door labour, but in the metropolis there is certainly a greater proportionate number of persons employed within doors. The first class of occupations is acknowledgedly the more healthy of the two; and, therefore, if it were not for the inferior house-accommodation of Liverpool, the mortality would certainly be lower there than in London. Even after making due allowance for the greater liability to accident of the dock-labourers of Liverpool than the average of the working classes of London, there will still be a higher mortality among the former class;

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* Statistical Journal, vol. vii, p. 57.

which higher mortality we can attribute to no other cause than the bad sanitary state of their dwellings. It is true that Mr. Neison asserts that "the class of labourers in large cities is subject to a very high rate of mortality;" but we cannot find any evidence in his own tables of the correctness of that assertion; for the agricultural labourer who works in the open air of the country stands at the top, and the clerk at the bottom of the sanitary scale; and assuredly the work of the dock-labourer of Liverpool is as nearly allied to that of the agricultural labourer, as the employment of the clerk is to a very considerable proportion of the occupations of the metropolis.

The comparison which we have been instituting between the metropolis and Liverpool, for the sake of testing the value of Mr. Neison's objections, seems to us to be in every respect as fair as it is conclusive. The method we have employed is the very one suggested by him, and used in his controversy with Mr. Chadwick; in applying it, we have first evaded the difficulty founded upon different occupations of the inhabitants of the two cities, by comparing the children and females; and we have then grappled with that difficulty, and, as we think, surmounted it. As some time has now elapsed since the publication of the 'Contributions to Vital Statistics,' it is possible that Mr. Neison may have somewhat modified his extreme views. It ought, therefore, to be understood, that we deal with those views as we find them expressed in the work in question; as being the most decided exponent of a creed, if not diametrically opposed to that of the sanitary reformer, at least differing from it in a very decided manner. The great industry and ability, and acute powers of analysis displayed in that work, have raised Mr. Neison to the rank of a first-rate authority, and have given unusual weight to his objections and doubts. These considerations have induced us to submit them to a careful, though brief examination; and the same motives will, we trust, influence Mr. Neison in reconsidering his opinions. He himself must be in possession of materials which would serve to test the soundness of his own theory. If, instead of comparing the expectation of the members of benefit societies in Liverpool with those of city districts (p. 59), and attributing the loss of from two to three years of life in that notoriously unhealthy city to labour in the docks, on the assumption that such labour is peculiarly unhealthy, he would contrast the expectation of the dock-labourer of Liverpool with that of a class following the same, or a similar occupation in the metropolis, or in other large cities, we shall be much surprised if the result do not prove the greater unhealthiness of that classic ground of cellars and courts. But be this as it may, the difference in the rate of mortality of women and children remains an insuperable objection to the theory, which would trace the unhealthiness of large towns exclusively to the aggregation of unwholesome occupations.

Our limited space does not admit of our discussing the soundness of other parts of Mr. Neison's reasoning. But we must protest against two assumptions which appear to enter into his reasonings throughout. 1st. That because the higher classes of society, who live in wide and well-drained streets, and in houses well supplied with water and the means of cleanliness, who have at least a sufficiency of wholesome food, wear clean linen, are no strangers to the bath, and are not sparing of soap and water, are short lived, therefore the working classes, who exist in filth and discomfort, are not sufferers by so unnatural a state of things; and 2d, that the members of benefit societies, who exhibit the virtue of prudence, which is never a solitary one, and can afford to pay a weekly contribution towards a provision for the future, occupy, in common with the reckless and improvident, the worst parts of our large towns. The first assumption appears to us to offend as much against the rules of logic, as the last does against all the probabilities of the case.

There is yet another objection which may be urged against the opinions of the advocates of sanitary reform; namely, that they attribute too much to the condition of the towns, streets, and houses in which the people live, and too little to their habits of life. This is an objection very likely to be advanced by those who, under a deep conviction of the ruinous effects of habits of intemperance, are disposed to attribute to an indulgence in those habits almost every form of physical and moral evil. We, too, believe that intemperance is a most fruitful cause of disease and premature decay, and that it claims every year some thousands of victims; but, at the same time, the same strain of argument which serves to disprove the justice of Mr. Neison's objections, seems to us to furnish conclusive evidence that the portion of the mortality of our large towns, which the advocates of sanitary reform attribute to unwholesome streets and houses, is really due to that cause. If, as before, we compare London with Liverpool, we discover an excessive mortality in the latter city among young children, who are not directly affected by intemperance, and among women, who are not largely addicted to the use of the bottle. Or, if we are in error in exempting women and children from the effect of spirituous liquors, it is obvious that, as with them so with men, the same degree of intemperance which prevails in London is likely to reign at Liverpool. The intemperance is probably nearly equal in the two cases; the condition of the city is the *vera causa* of the loss of four years of life to each inhabitant. Banish intemperance from London, and allow it to continue unchecked in Liverpool, and you would have a difference of four years still due to the inferior structural arrangement of the metropolis, *plus* an addition not easy to estimate, arising from the improved personal habits of the London population.

A last objection still remains to be disposed of. The want of food is alleged to be the real cause of that high mortality of towns, which has been attributed to impure air. This objection is as easily answered as the rest. As a general rule, the inhabitants of large towns do not want food. They have as much food, of as good quality, and as well cooked, as the poorer classes inhabiting the comparatively healthy rural districts. Between the diet-roll of London and Liverpool there is probably as little difference, as between the wages of labour or the relative quantity of alcohol consumed; and in years of scarcity, the towns have more ready access to supplies of food, and are less liable to be stinted in their allowance, than the rural districts; and yet even in those seasons which are free from epidemics, the towns are less healthy than the rural districts, and Liverpool less healthy than London. It is true that famine and a high mortality, especially from pestilential diseases, go together. When the price of food rises beyond a certain point, so as not merely to impose on the poor a necessity for economy in items of expenditure not absolutely necessary, and a disuse of the few luxuries they are able to command, but the consumption of a small quantity of food, and the substitution of a less wholesome diet for that to which they are accustomed, disease and death are always busy among them, and pestilence, in some of its many shapes, ravages the land. Nevertheless, even in this extreme case, the exact part which is played by want of food in this national tragedy is fairly open to discussion. Can starvation alone generate typhus fever? If our towns and villages were in the condition in which the advocates of sanitary reform wish to see them, would fever follow privation, and would it spread from person to person, from village to village, from town to town? Place a number of starving people in a well-ventilated building, keep them and everything about them scrupulously clean, let them have the prison allowance of air to breathe, and give them only as much food as charity has doled out to the fever-stricken inhabitants of Ireland, would typhus fever originate and spread among them? That it does arise and spread from person to person, and from the poor to the rich in our large towns, in circumstances the very reverse of these, we all know; that it may originate and spread where filth and impure air are not reinforced by want of the necessaries of life, is equally notorious; that it will sometimes invade the dwellings of the better classes, where both these assumed causes are, to all appearance, absent, every medical practitioner can testify; but, nevertheless, we have not any satisfactory evidence that privation alone can give rise to fever—no evidence to be compared to that which is held to demonstrate the possibility of fever arising *de novo* in extreme overcrowding and impurity of the air, and spreading rapidly from person to person under the same unwholesome influences. It is no part of our present plan to discuss this very difficult question of the exciting and predisposing causes of fever; our space will only permit of our expressing an opinion that the wretched structural arrangements of our towns and villages are always part and parcel of those causes. The subject is one which challenges an early and searching examination.

We must now leave the physical part of the sanitary question, and briefly advert to its economical and moral bearings. As a physical question, it appeals to us as medical men; as an economical and moral question, it concerns us as citizens and philanthropists.

The economical bearings of the sanitary question are scarcely less important than those which we have been considering. The waste or misapplication of money on a large scale is in itself a very serious evil, pressing heavily on the classes who are already the principal sufferers in their health and comforts. In this branch of the subject, too, the figures put forward are large and startling. The annual waste of life and sacrifice of health reduced to equivalents in pounds, shillings, and pence, under the heads of sickness, funerals, and labour lost, is represented by a grand total for England and Wales of £14,873,931, or little less than £15,000,000 sterling. Of this enormous total the metropolis contributes very nearly two millions, and Lancashire upwards of four millions.* The standards of comparison employed in these calculations, are the rate of mortality and average age at death in the most healthy registration district of each county; the ages of the living being disregarded, and the ratio of sickness to death being taken at 1 to 28. If this essential element of age had been taken into account, if the more moderate standard of two per cent. had been substituted for the perhaps too favorable mortality of the most healthy district, and if the ratio of 1 to 28, between deaths and cases of sickness, had been made to suffer some abatement, it is not impossible that these fifteen millions might be reduced to considerably less than half. Possibly the total waste of money might not exceed the sum annually raised in the shape of poor rates.

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* Tables of the Vital Statistics of England and Wales.

As we have already intimated, this economical branch of the sanitary question does not so directly concern the members of the medical profession, as that to which we have already devoted so much space. Still we must not dismiss it with so short a notice. There are yet some economical considerations, which no member of society can view with indifference. The calculations published in the tables of the Health of Towns Association embrace only three heads;—funerals, sickness, and labour wasted. Orphanage and widowhood, which impose a perpetual burden on the Poor Laws of about 50,000 women and children, and an annual burden, which though not yet ascertained, cannot but be considerable, are not taken into the account.

Then there is another enormous item of waste or misappropriation of money, not contained in these tables; namely, the sums squandered in the shape of defective and costly structural arrangements above and below ground;—in sewers, which are little better than elongated cesspools, put down in wrong places, built of wrong materials, faulty in shape, with insufficient fall;—in cisterns and water-butts, with their paraphernalia of pipes and ball-cocks, adapted to a limited and intermittent supply of water;—in shops and workshops, destitute of all means of ventilation;—in houses and hovels, furnished with the costly and barbarous cesspool, expensive alike to landlord and tenant. Add to these the enormous expenditure incurred by the use of hard water for soft; by the smoke nuisance, with its double waste of fuel and soap; and by the discharge of the refuse of towns into the sea. What all these barbarisms have cost and are costing us, it would be difficult to say; but that they amount to several millions a year, no reasonable man can doubt. We refer our readers to the Reports of the Health of Towns Commission, and the publications of the Health of Towns Association, for particulars. If they appear exaggerated, let them halve or quarter every item, and there will still remain the most remarkable *exposé* ever yet made of municipal and national extravagance.

One broad principle may be safely enunciated in respect of sanitary economics;—that it costs more money to create disease than to prevent it; and that there is not a single structural arrangement chargeable with the production of disease, which is not also in itself an extravagance.

But perhaps the *moral* bearings of the sanitary question are, after all, the most important, as well as the most indisputable. If it could be proved that filth and overcrowding, short supplies of water, and aerial impurity, are innocent of unnecessary sickness and premature death; if it could be shown that the structural arrangements of our towns, and the nuisances with which they abound, do not involve a sixpence of unnecessary expense, even then the sanitary question would hold its high ground among the great moral questions of the day. You cannot degrade the physical man by a life-long familiarity with scenes of filth and indecency, without debasing his whole moral nature; and no medical man needs to be told that the mass of the people of this country are born and bred, and live and die, amid such scenes. Everywhere the habitations of the people are more like the encampments of savages than the abodes of civilized men. The scenes which you encounter in the most crowded parts of the metropolis are reproduced with unerring exactness in the rural environs of Hampstead; the recently completed survey of Sheffield is but the disgusting counterpart of the description of Liverpool, Manchester, or Nottingham, given in the Report of the Health of Towns Commission. Some of the descriptions of the poorer districts of Sheffield are enough to turn the stomach of an Esquimaux. The offensive refuse which even animals will bury out of sight, is brought into perpetual contact with human beings. It stagnates in the courts and alleys, flows into the cellars, and is sucked up into the walls. Men, women, and children eat, drink, and sleep surrounded by its disgusting effluvia. The pig in its stye is not more familiar with its own ordure, than is the wretched immortal in the dwelling which ignorant carelessness has built for him, and municipal and legislative indifference have suffered him to inhabit. Such a state of things is beginning to be recognised by all orders of men as a strange anomaly. It has made some impression even on the stolid stupidity of parish authorities; town-councillors are rubbing their eyes, and slowly awaking to a consciousness of the true state of things; the idea is dawning on the rate-payers that the farthing-in-the-pound economy may, after all, be a form of extravagance; medical men are recollecting that it was amid such scenes that cholera first made its appearance, and that typhus fever takes up its abode in them; and clergymen, who are generally, naturally, and perhaps properly, to be found in the rear of popular movements, are discovering that the scavenger and the architect are among their best allies. One member of that sacred profession saw at a glance that science had marked out for him a new sphere of usefulness; and almost before the Reports of the Health of Towns Commission were dry, took their appalling revelations for the text of an appeal on behalf of that helpless class committed by his heavenly master to the peculiar care of the church.* This was but the first of a series of labours directed to the improvement of the physical condition of the poor. From his most recent literary effort,† which has the one rare fault of brevity, we extract a few short passages, which will conclude all we purpose to say on the moral bearings of the sanitary question.

"First, I would ask you just to contemplate for a moment in your minds the outward universe, so orderly, so beautiful, so richly replenished and adorned: the fields decked with flowers as well as laden with fruits, the heavens glittering with countless stars. Remember how these things are spoken of in Scripture. 'Consider the lilies of the field, how they grow!' and can you doubt that much more would God have man, the noblest of his creatures here below, fed, clothed, and lodged in comfort, to its own satisfaction, and to the glory of his Maker?" Our author then conducts us to "some close tenement, some narrow lodging, perhaps a single chamber for a whole family, dark, dirty, noisome, pestilential, the occupiers in rags, and faint for want of food," and he asks us "whether the beauties of the Christian character are likely to flourish in such an atmosphere as this?" whether "modesty can bloom where common decency is impracticable?" whether such circumstances are not most adverse to a holy life?* There can be but one answer to these questions. Such circumstances *are* most adverse.

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* Letters on the Unhealthy Condition of the Lower Class of Dwellings, especially in Large Towns. By the Rev. C. Girdlestone, M.A.

† The Cause and Cure of Abject Poverty. By the Rev. C. Girdlestone, M.A., Rector of Kingswinford, Staffordshire.

The members of our own profession who have already embarked in this most righteous crusade against physical corruption, cannot but feel themselves encouraged and supported by the sympathy and co-operation of the clergy; and those who have not yet taken any part in furtherance of the sanitary cause, may perhaps find a motive to exertion in the growing interest with which it is regarded by the members of other professions, and by society at large. But a sense of duty, far more than the mere force of example, ought to enlist the medical man in this holy warfare. No member of society is so cognizant as he is of the facts of the case, or better prepared to interpret and enforce them; no one is less open to the suspicion of mean or unworthy motives; and no one has such frequent opportunities of converse with men of every rank and degree. If he, who knows so much, should appear indifferent, or, what is worse,—from the bad habit of looking at the routine practice of his profession as the only honorable occupation of a medical man, and the work of palliation as his only duty,—should speak slightingly of this higher work of prevention, and carp at the efforts of others on the pretence that they are given to exaggeration, society would soon catch his tone of thought and feeling; and a cause which, on serious reflection and careful examination, he would be constrained to support, must suffer irreparable injury. If, on the other hand, he could be induced to exert himself heartily, but discreetly, in favour of sanitary measures, and to bring his influence to bear on those with whom his professional avocations place him in communication, it is impossible to over-estimate the good he may be the means of effecting. He is also favorably circumstanced for combating that petty spirit of parochialism, which embodies its ignorance and selfishness in the cry of local self-government, and the transparent pretence of a zeal for liberty. Unlike the clergy, who are often too much mixed up with parish vestries to act with spirit and independence, and who have, in one or two melancholy instances, shamefully truckled to the narrow spirit which actuates them, medical men have little to expect from the local authorities but the injustice and indignity of inadequate remuneration. A large majority, at least, of the medical profession are in a position to take, if it so please them, an attitude of remonstrance against local mismanagement, and to administer deserved reproof to parochial incapacity; and we trust that they will avail themselves of every opportunity of discharging this very necessary duty.

We have purposely abstained from addressing to our medical brethren any selfish motives to exertion in the cause of sanitary reform. But there is one consideration of this nature so natural and allowable, that we shall scarcely be blamed for suggesting it; we mean the motive of self-preservation. The practice of our profession, at all times and in all places a service of danger, is peculiarly hazardous when it takes us into those scenes of filth and overcrowding to which we have just adverted. The history of the Irish fever has served to show the extent and reality of this danger—a danger, be it recollected, rarely counterbalanced by any reasonable hope of reward. Fever cases in the worst localities of our large towns do not pay. Where they spare the life of the patient, they overwhelm him with embarrassment, and rob him of the means of discharging his debts. The medical man knows this to his cost. A regard to his own safety, therefore, should reinforce his other motives to exertion in behalf of those wholesome measures, which, if they do not banish typhus fever from the land,. will at least greatly lessen the danger of infection.

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* See the whole of this beautiful passage in the Journal of Public Health, No. I.

But the medical man will soon be called upon to take a more direct and practical part in the prevention of disease. Having faithfully discharged his duty by exerting his influence in favour of the promised measure of sanitary reform, he may hope to become an honoured instrument in carrying its benevolent plans into effect. Whatever the precise shape which Lord Morpeth's new bill may assume, the appointment of officers of health in our principal towns, and in all our town districts, must be one of its provisions. The duties attaching to the new office, cannot fail of being as grateful to those to whom they shall be intrusted, as they will be important to the community at large. The highest aim of good government being the prevention of social evils, the best and most welcome occupation of a citizen must be the furtherance of that aim. We anticipate, therefore, that the post of officer of health for its own sake, and irrespective of the emolument which may attach to it, will prove attractive to men of large views and scientific training. But at the same time, we must express a hope that the Government will not propose, nor the Legislature sanction, so low a scale of remuneration as that which was shadowed forth in one of the early Sanitary Reports; and that the disgraceful example of the Poor-law authorities will not be held up to imitation. Without liberal remuneration, it is unreasonable to expect, and impossible to obtain, efficient public services. The value of the work to be done will be measured by the salary received; and nothing will tend so much to secure respect for the new functionaries, as a stipend which will enable them to maintain the position of gentlemen. We are moved to allude particularly to this subject, by the notorious readiness of the authorities to offer, and of medical men to accept, remuneration utterly disproportioned to the value of their services, and derogatory to their own position in society. One step in the right direction would do much to remedy this crying abuse.

It is essential that the officer of health should be independent of local influence. A liberal salary will do much to ensure this desideratum; but his nomination by the government, and not by the local authorities, is, if possible, still more essential. It was with equal grief and indignation that we heard of the abandonment by Lord Morpeth of that very wise provision of his bill, by which the appointment of this officer was vested in the crown. Nothing but a sudden panic, or an equally mischievous impulse of good nature, can account for so unwise a yielding to the pressure from without. Give the local authorities the nomination of officers of health, and it will become the direct interest of medical men to possess themselves of nuisances. We speak advisedly, when we say that in two instances this concession of Lord Morpeth's was received with unusual favour; and it was well understood that medical men, who owned some of the worst property in the towns in which they lived, were looked upon as peculiarly eligible to an office that threatened serious annoyance to all the owners of low and neglected tenements.

We trust that the profession will keep their eyes upon the movements of the Government, and submit Lord Morpeth's promised bill to a close scrutiny. An admirable opportunity will be afforded by the new appointments for vindicating the right of the profession to a more liberal treatment than that which they have hitherto received in any branch of the public service. Such an opportunity must not be lost; but in the meantime we would earnestly entreat all those members of our profession, whom we may have been so fortunate as to convince of the general soundness of the sanitary cause, to use their utmost influence to support the Government in their battle with the refractory local authorities, from whom all the opposition to sanitary reform has hitherto proceeded, and may still be expected. A better cause—a cause more strongly fenced round with facts and arguments, medical, economical, and moral—has never yet been brought forward; and certainly none in which every member of the medical profession may engage with more propriety, or with more effect. As a question of science, it belongs peculiarly to them; but, as a great economical and moral movement, it is the most pressing business of the state, and the most promising boon yet offered to the public.

ART. II.

A Practical Treatise on the Causes, Symptoms, and Treatment of Spermatorrhœa. By M. LALLEMAND. Translated and Edited by HENRY J. M'DOUGAL.—*London*, 1847. 8vo, pp. 334.

THE translation of a work that has been so long before the profession as M. Lallemand's treatise 'Des Pertes Séminales Involontaires,' might seem to call for a mere bibliographical notice at our hands; but we have adopted the present course towards it, in the conviction that the subject upon which it treats has insufficiently engaged the attention of practitioners in this country. For, notwithstanding the reputation as an accurate observer achieved by its author in another field of inquiry, the manifest care he has bestowed upon the present investigation, the novelty and importance of some of the views he has put forth, and their partial confirmation by several able surgeons, we doubt whether the class of diseases he has brought under notice, is much more accurately recognised, or more successfully treated by the great body of the profession, than heretofore. Whether this has arisen from the exaggerated character of some of his statements, his piecemeal mode of publication, the incomplete manner in which his views have been laid before the English reader, or the somewhat repulsive character of the subject he has chosen for illustration, we cannot take upon ourselves to decide; but the publication of Mr. M'Dougal's condensed translation, in which the substance of three thick octavo volumes, written at different periods, is compressed into one of moderate size, some of the defects of the original obviated, and its unquestionable merits brought more into relief, must lead to a reconsideration of the matter; whence it is to be hoped will result a more satisfactory mode of dealing with these cases than prevails at present. That M. Lallemand should have been the first to institute an accurate appreciation of their true character, says much for his practical acumen and freedom from the dominion of preconceived ideas; and should tend to inspire confidence in his conclusions, inasmuch as these have been arrived at in spite of circumstances well calculated to mislead. For it was in consequence of the publication of his work on the 'Encephalon and its Dependencies,' that patients were brought to him, apparently labouring under chronic disease of the brain and its membranes, as indicated by the presence of the symptoms of mental alienation, hallucination, hypochondriasis &c. &c. His favourite studies would naturally have led him to view these cases in the same light as the practitioners who consulted him respecting them; but accident having at first directed his attention to their true nature, further investigation obliged him to yield to evidence, when he found "the symptoms increase in the same proportion as the seminal discharges, and diminish, cease, or reappear with them."

Although we do not believe that disordered conditions of the male genital organs prevail in this country to the same degree as upon the Continent, and especially the southern portions of it where sexual abuses are carried to a frightful extent, yet we have been long painfully convinced that medical men are little aware of the actual amount of the evil existing among ourselves. Not only has this arisen from their having failed to recognise the actual cause of the symptoms indicative of a disordered state of the nervous system in the class of cases alluded to by M. Lallemand; but from their neglecting in the ordinary course of practice to make any inquiry whatever into the condition of a function which excites so marked an influence over the well-being of the economy; while, when this has been pressed upon their notice, by patients or their friends, they have seldom seemed alive to its importance, or they have hazarded unreflecting and inapplicable advice. Indeed, when we find so judicious a practitioner as Dr. Golding Bird, stating, in a passage quoted and properly animadverted upon by Mr. M'Dougal, that "it certainly is not very consistent with our national character to dilate so freely upon a subject which, in the great majority of cases, can be treated of only as the effect of a most degrading vice," we at once see (supposing, as we are inclined to suspect is the case, that such fastidiousness is somewhat general,) what a confusion of ideas prevails upon the subject, and can understand why patients suffering from the class of ailments, of which spermatorrhœa constitutes one of the eventful consequences when treatment has been neglected, often prefer submitting their affections to the quack and the impostor, rather than call in the regular practitioner. The one holds out every encouragement by means of deceptive promises and strong assurances, which are not without their beneficial effect upon persons in whose condition despondency is so marked a feature; while the other examines into the case carelessly, and gets rid of it as speedily as possible, as one of a nasty description, unworthy of occupying his attention.

Our primary duty, if not our sole one, is the relief of the ailments of the human organism, no matter what the origin of these may be. If we are to eliminate all the affections which originate in moral perversion, our sphere of action will be found to be somewhat inconveniently limited. But without establishing a censorship of this kind, the occasions are not rare, in which, having duly ministered to our patient's necessities, we may have so gained his confidence as to be in a most favorable position for offering to him advice, admonition, or remonstrance, calculated to induce the abandonment, or prevent the recurrence, of habits and practices alike adverse to religion, morals, and health. We are here indeed called upon to appeal to every motive and invoke every aid, and are encouraged to do so by every motive of humanity and some hopes of success; but to determine that a case shall not receive all the aid that science has placed at our disposal, on the ground that it is self-inflicted, is to wofully mistake our mission. An apology would be due to our readers for formally stating so elementary an ethical principle, did we not know that some such repugnance as that adverted to is not infrequently entertained.

A more legitimate reason, which has induced practitioners to avoid these cases as far as possible, is the fear they have entertained of being confounded with the herd of disreputable persons who especially affect them. They are aware that some men, who have even received a professional education, lay themselves out with all the schemes, artifices, and iniquitous procedures of the undisguised charlatan, for the purpose of entrapping a portion of the lucrative spoils extorted from the fears of the timid and the credulity of the ignorant. That any ground of suspicion of entering into such fellowship should be strongly eschewed by every upright mind, is indeed to be expected; but, in order to avoid any such imputation, the closure of their doors against this class of applicants for aid is neither the necessary or the most proper course. The only manner in which this description of quackery can be discountenanced effectually, is by the destruction of its speciality; and this cannot be accomplished unless medical practitioners in general render themselves competent, and hold themselves ready, to treat any ailment brought under their notice, no matter what its origin or in whom occurring. This, it is true, will not eradicate the evil; for it is too often the characteristic of the unfortunate victims of this class of diseases, to require bolder promises than legitimate medicine can furnish, and to submit to less restriction than its demands; but at all events it will alleviate the mischief, and prevent the better informed of those patients resorting to impure quacks, because they cannot find legitimate practitioners able and willing to do justice to their cases. The benefit which would accrue from such a course may be judged of, when we consider that a very large proportion of the persons who consult these quacks are very young men, alarmed at the consequences of a first indiscretion, or very often only suffering under imaginary evils dependent upon the perturbation of the economy by the full establishment of puberty. Their fears, augmented by the perusal of the alarming statements so widely circulated in the obscene pamphlets of these charlatans, would speedily be allayed, could they have recourse to the aid of conscientious practitioners, who would pay what they consider due attention to their cases, instead of treating lightly and flippantly those disorders which are to the sufferers the occasion of the keenest anguish.

M. Lallemand judiciously commences his treatise with a chapter upon the *Pathological Anatomy* of the genito-urinary organs; a correct appreciation of this being as important as it is rare. Practitioners seeing patients exhibiting every sign of prostration of vital powers, and oftentimes manifesting little severe local pain, have been misled by the notion that in these cases a primary constitutional debility is the condition to be coped with, and have prescribed tonics, stimulants, &c., to the neglect and aggravation of the local chronic inflammatory action upon which the general symptoms in the great majority of cases depend, or at least without the relief of which they cannot be removed. When it is recollected that the bulk of these cases, as we shall presently see, originate in sexual excesses of some description, we are prepared to find the various portions of the over-stimulated apparatus falling into an inflammatory condition, which will be followed by the ordinary consequences observed in analogous textures. The appearances after death have been seldom recorded, for these cases are of rare occurrence in hospital practice, and great obstacles usually present themselves to the making an efficient examination in private; and indeed the directions furnished by M. Lallemand for making such an examination, involving the division of the rami of the pubes and ischia, so as to retain the soft parts in situ, can obviously be followed nowhere but in the dissecting-room. We believe, however, that much important information may be obtained by a little dexterity and patience at an ordinary autopsy; and it is certainly to be regretted that practitioners so seldom examine, even thus imperfectly, the condition of these organs. Under these circumstances, M. Lallemand's detailed accounts of the post-mortem examination of several patients, who died worn out with seminal discharges, become invested with additional importance. From these it is evident that there is no portion of the entire track of the mucous membrane of the genito-urinary organs, that may not participate in the inflammatory action originally excited by excess of action, gonorrheal inflammation, and the like, in the vicinity of the extremities of the ejaculatory ducts. The amount of mischief thus induced will of course depend upon the individual susceptibility, duration of the disease, and other circumstances, all to be taken into account in estimating the curability of a given case. Thus we may have every change induced in the prostate gland, from mere engorgement, to its complete conversion into a soft tubercular mass, held together only by its fibrous envelope. One or both testicles may become inflamed, according as the seminal canals of one or both sides are affected; and M. Lallemand instances cases, in which inflammatory action was propagated from the vesiculæ seminales to the contiguous peritoneum. The orifices of the ejaculatory ducts become enlarged and distorted from their circular form; and M. Lallemand suggests that possibly in some cases the ulceration or dilatation of the sphincters closing these may prove the only lesion producing the spermatorrhœa. In other cases, this affection may depend on the loss of elasticity and contractile power sustained by the canals themselves. The function of the vesiculæ seminales, as receptacles of semen, becomes materially interfered with when their parietes are indurated or thickened, or when their cavities are loaded with pus, which, owing to the difficulty it finds in effecting an exit, undergoes inspissation. The same condition of purulent matter within the vasa deferentia, or the tumefaction of their parietes, may produce a more or less complete obliteration at certain points of their course; while, if an abscess of the epididymis open externally, a spermatic fistula may result. Such obliteration may in some cases induce atrophy of the testis. The extension of inflammatory action, through the urethra to the kidneys, may induce also a diseased condition of those organs. M. Lallemand remarks at some length upon the analogical mode of action of the urinary and genital organs, and their mutual influence upon each other; and although this last point is generally admitted, we may extract a passage or two tending to illustrate it:

"Diurnal pollutions are too little understood, to have been generally noticed in these cases; they are always obscure, and the attention is fixed usually upon another object; but I have so often satisfied myself of their presence as a sequel to strictures, that I regard spermatorrhœa as the true cause of all the cases of hypochondriasis, ischuria, and debility, which are attributed to affections of the urinary organs. This position is, I think, proved by the weakness and rare occurrence of erection, the rapidity of ejaculation, and the increased fluidity of the semen observed in most of these patients.

"Cases of diurnal pollution, uncomplicated with catarrh of the bladder, are somewhat rare; and this often renders diagnosis difficult, not only on account of the symptoms of catarrh being present, but also on account of the mucus secreted by the bladder and prostate; on this account when I see the urine cloudy, I always inquire respecting diurnal pollutions, so that I may not confound mucus with semen. It is very remarkable, also, that those who give themselves up to venereal excesses or masturbation, frequently experience a desire to micturate; this fact gives rise to the saying of the ancients, '*raro mingitur castus.*' I have ever been struck with the truth of this axiom; and the fact proves how easily the urinary organs share the excitement of the spermatic........

"Most patients remark also, that when they are threatened with a relapse, it is preceded by a more frequent and very sudden desire to micturate, whether this increased sensibility of the bladder arise from cold, or from an excess either of drink or of coitus.

"Persons affected by diurnal pollutions, experience generally very injurious effects from the use of diuretics. Nearly all those who have taken squills, nitrate of potass, digitalis &c., have noticed, during their use, a remarkable increase of the seminal evacuations; and a few, after having been cured during a longer or shorter period, have experienced relapses which could not be attributed to any other cause, and which have also spontaneously passed off as soon as they have relinquished the use of these medicines. It is also worthy of notice that children subject to incontinence of urine, are particularly liable to nocturnal pollutions at the age of puberty; and at a later period, to diurnal pollutions." (p. 31.)

The greater portion of M. Lallemand's work is taken up with the illustration, by means of detailed cases and remarks upon them, of the various *causes* of spermatorrhœa. These involve much repetition, but certainly form a most valuable clinical history of the disease. The following are those which are thus separately commented upon: Blenorrhagia; the metastasis of old cutaneous affections; irritation of the rectum by obstacles to defecation, hemorrhoids, and especially by ascarides; the abuse of the organs of generation by masturbation; excessive venery; the action of certain medicinal and dietetic substances, as astringents, drastics, tobacco, cantharides, large doses of camphor, diuretics, and the immoderate use of tea or coffee; the influence of the cerebellum; and congenital predisposition, as exhibited in a proneness to sebaceous accumulation under the prepuce, too great length or a phymotic state of this part, a natural debility of the generative organs, and hereditary transmission. In this varied list, excessive, but most especially unnatural, employment of the organs of generation occupies the foremost place; and indeed most of the other causes enumerated must be regarded as only cooperating or additional causes, which would not, save in some very susceptible subjects, have proved competent to the production of the affection, unless a morbid condition of the seminal receptacles and ducts had been previously induced. Although M. Lallemand frequently notices the conjoined operation of two or more causes in the cases he relates, we think he has in his *seriatim* enumeration too often not sufficiently distinguished the essential from the occasional element; magnifying the etiological, though perhaps not the practical importance of the latter. Even blenorrhagia itself, which, as far as relates to site, has everything in favour of its potency, is of insignificant effects, save in the circumstances adverted to; while ascarides should perhaps be rather regarded as an exciting cause of masturbation, especially in children, than as a direct cause of spermatorrhœa.

In defining what he implies by venereal excess, M. Lallemand very truly remarks that the apparent *wants* of the genital system may be *factitious*.

> "A violent attachment may, in this respect, give rise to great illusions; the direct irritation, brought on by *herpes preputialis*, or by the presence of ascarides in the rectum, may excite morbid excretions which have no connexion with the real wants of the system. Irritation of the cerebellum, the spinal cord, or the nerves supplying the genital organs, may produce the same effects; so that the frequency and duration of the erections will not always show the amount of the true powers. In many persons, the desires are greater than the power of fulfilment; the imagination of such is constantly occupied with erotic ideas, whilst their physical powers are very feeble. The impulse in these persons is purely derived from the brain; and their immoderate desires cannot, therefore, furnish a measure of their real wants." (p. 165.)

We are enabled, to some extent, to estimate by the feeling left after coitus, whether it has been employed in excess. In such a case, in place of the normal energy and activity which it should impart to every portion of the system, we find lassitude, satiety, sleepiness, and disinclination to mental or bodily exertion; and a repetition of the erection under such circumstances, is to be regarded rather as resulting from morbid irritation than from unsatisfied natural desire.

Upon the ill effects of the *nitrate of potass* and other diuretics, so indiscriminately prescribed in affections of the genito-urinary organs, M. Lallemand remarks:

> "Saline mixtures, containing nitrate of potass, are prescribed every day for the relief of the inflammatory symptoms in the first stage of blenorrhagia. There cannot be a greater error. It is also regarded as a diuretic, because ordinarily it increases the flow of urine; and this is why its sedative powers should be doubted. The quantity of urine can only be increased by stimulating the functions of the kidneys, or, in other words, by acting on them as an excitant. When administered in too large a dose, it produces hæmaturia, pain, &c. But it is not on the kidneys alone that nitre produces this stimulating effect: it increases all inflammations of the bladder, whether acute or chronic; it is even contraindicated in the most simple cases of vesical irritation I have seen; it produces the same effects in diseases of the prostate; it increases the stabbing and pricking pains, and the sense of weight which patients always feel in that region. . . . . . .
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> "It appears, then, that nitre acts as a stimulant on the whole urinary apparatus, and it is at least probable that it produces the same effects upon the spermatic organs. I am led to this opinion, partly by analogy, but chiefly because more than forty of the patients whom I have treated for involuntary seminal discharges, had taken it in some form or other, and all, without exception, found themselves worse afterwards. Many of them also observed the same effects from preparations of squill, and in fact from all diuretics." (p.186.)

M. Lallemand, in our opinion, attributes far too great an importance to prolonged *continence* as a direct cause of spermatorrhœa. That a moderate exercise of the genital organs is the condition most favorable to the maintenance of their healthy condition, and the general well-being of the economy, is a fact which requires no proof; but we conceive that M. Lallemand greatly exaggerates the ill-consequences which ensue upon the withholding such exercise. In the first place, the condition of spermatic plethora, or distension of the vesiculæ, is not such a necessary consequence as he states it to be; and may be said to be in great degree under the control of the individual. Even if the secretion of semen is of constant occurrence, the amount is much influenced by the mental state operating through the nervous system, and by the demand made upon the gland. Thus, if an individual has frequent recourse to coitus, or without this indulges in erotic thoughts, libidinous reading and conversation, or analogous practices, a large quantity of semen will be secreted; and in the latter case, not finding a natural ejection, it may lay the foundation of obstinate nocturnal pollution. This, indeed, may occur in any person without prior excitement of sensual ideas; but then it is usually both rare and moderate. And, indeed, the testicles and their secretion seem to possess a far greater power of accommodation to the exigencies of the economy, than M. Lallemand is disposed to allow; and we imagine the instances of atrophy from mere disuse, unaccompanied by prior disease, to be of very rare occurrence. Certain it is, that many men, who have maintained a complete continence long after full manhood, have yet proved effective progenitors of children; while the number of cases of premature impotence, brought on by too early or excessive intercourse, would lead us to suspect that this is the more formidable error of the two. He underrates the influence of education and hygienic measures, as also of original susceptibility; and jumps to the conclusion, that if continence can be maintained, it is because there is inability or physical indisposition to break through it, and this, for the most part, engendered by daily pollutions which have escaped attention. That persons of originally strong passions, who have taken little pains to subdue them, may suffer all the inconveniences he supposes, we can allow: but that in all individuals there is continually a large secretion of semen, which by fair or foul means must be expelled, we do not admit. We prefer the explanation offered by M. Gosselin, in a paper upon "Obliteration of the spermatic passages," published in the 'Archives Générales' of the present year. Judging from the results of his late anatomical inspections, he is disposed to regard this as of far more frequent occurrence than is generally supposed. He found examples of such obliteration occurring in the vas deferens at either extremity of the epididymis, and in the tubuli themselves. Upon the two former cases he makes some interesting observations. 1. Although the semen was thus prevented reaching its vesiculæ, and was confined within the vas deferens and epididymis, no alteration in the seminiferous tubes of the testes took place, though sometimes a marked dilatation of the epididymis, producing a true spermatocele, was observed. 2. In all these cases no atrophy of the testis occurred; a fact capable of demonstration by comparison with that of the opposite side. 3. The testicle secreted normal semen, as shown by the presence of the spermatozoa, molecular granules, and epithelial cells in the fluid of the epididymis, while that of the vesiculæ contained only the cells—M. Lallemand, as we have seen, insists that semen once secreted must be expelled; but M. Gosselin agrees with those who believe that it may be reabsorbed. He says:

"All physiologists acknowledge that, of all the secreted fluids, semen most easily admits of absorption. In fact, in the best organized subjects, its excretion is neither continuous or regular. It is true that the vesiculæ are organs of reception; but their capacity is small, and their power of distension slight. Now, if these organs do not admit of a large accumulation of the fluid, and yet, on the other hand, nothing arrests or suspends its secretion, it is by means of a gradual and proportionate absorption that nature supplies this apparent deficiency in the anatomical arrangements.

"Need I allude to the fact that such absorption of semen is regarded by all physiologists as of utility for the regular maintenance of our organs and the exercise of almost all our functions? Observe what takes place when the testis is absent, disappears, or is imperfectly developed. The semen is then absorbed in insufficient quantity, and all the organism suffers from this, the constitution continues feeble, while the body does not take on, or loses a portion of, its masculine characters. Nature endeavours to provide for the regular conservation of the individual, by means of the absorption of the same products whose excretion serves for the preservation of the species. When an obliteration takes place, the latter and most capital of these uses is suspended, but the other persists. The absorption of the semen throughout the spermatic passages prevents a distension of parts which might end in local mischief, while the beneficial influence of the secreted semen is imparted to the entire economy."

We pass on now to the consideration of the *general symptoms* of this disease; and among these are none of a pathognomonic character, though several may lead us to suspect the existence of the affection. It is one of M. Lallemand's principal merits to have indicated to the profession the occasional, or, as he would say, the frequent, dependence of a perverted condition of the most important functions upon the presence of spermatorrhœa; under the influence of which, a simulation of aggravated disease, of the nervous and digestive systems in particular, capable of deceiving the ablest practitioners, takes place, and may give rise to treatment doubly mischievous, by leading to the resort of unnecessary and injurious measures and omitting those alone adapted to the true pathological condition. Every practitioner has had, from time to time, cases come before him in which the furtive and timid glance, the absence of memory, incoherent statements, anomalous symptoms, and a general and inexplicable deterioration of physical and mental power, have led him to suspect (especially in youthful patients), and sometimes to discover, the true cause of these in some injurious practice or consequence of it; but it is to M. Lallemand, that we are indebted for the best instructions for obtaining, through an efficient local examination, the confirmation or contradiction of any suspicions we may entertain; the more welcome, as in this description of cases it is exceedingly difficult to get the truth out of patients, while, in many instances, they may not be aware that bad habits, long since discontinued, are still visiting them with their retributive effects. Then, again, although M. Lallemand seems to us, with the natural fondness of a discoverer, disposed to generalize too hastily in attributing hypochondriasis, certain forms of insane delusion, and other examples of severe disorder of the nervous system, to unperceived seminal emissions, yet that many of these depend upon such causes, no reasonable doubt can be entertained; and no practitioner is justified in continuing a prolonged treatment in such harassing cases, without satisfying himself on this point. We need not follow the author through his able analysis of the various symptoms; for, in truth, it may be stated in general terms, that there is scarce a function in the economy which, in different cases, and in different degrees, does not become perverted, an irresistible debility being the common cause and accompaniment of such lesions.

We think, however, that M. Lallemand attaches too much importance to the mere amount of discharges as the enervating agent; and we believe that the intensely exclusive occupation of the brain by erotic ideas, and the violent perturbation to which it is subjected by repeated emissions unnaturally produced, have far more to do with the production of nervous exhaustion than the losses themselves. These, it is true, may—as do all the affections of the genito-urinary apparatus—react also on the brain, and induce aggravation of existing mischief; and great temporary relief may be obtained by the removal of such supplemental cause of irritation. We know the case of an individual who, by a constant indulgence in erotic ideas, brought himself to as enfeebled a condition as could well be, although no emission, nocturnal or diurnal, attended the erections thus induced. Then, again, masturbation in children and women is attended with little, and sometimes no discharge; and yet in the one we have emaciation and spasmodic affections; and in the other, all the symptoms of confirmed hysteria and exhaustion, far more difficult of relief than analogous conditions of the male, inasmuch as the cause may be suspected, but can seldom be detected.

When, from the history of the case, the anomalous character of the general symptoms, or the obstinacy with which these persist, we are led to a suspicion of its nature, we should at once resort to an investigation of the *local symptoms* derivable from an examination into the condition of the generative organs. The patient has become impotent, and even an aversion to the sex becomes a characteristic feature of the disease. Nocturnal pollutions, at first attended with pleasurable sensations, increase in frequency, with gradually decreasing orgasm, until at last they pass into the condition of a passive flux. In estimating the importance of pollutions, we must be guided, not so much by the amount of discharge, as by the effects produced on the system; for they may, and eventually will, decrease in quantity, and finally disappear, the patient's condition notwithstanding deteriorating more and more, inasmuch as the nocturnal pollutions, unknown to her or his attendant, have become exchanged for diurnal ones. When taught how to search for such discharges, he finds them terminating his frequent micturitions, or passing from the urethra owing to the pressure occurring during stool.

The most certain and satisfactory method of determining the presence of spermatozoa in the urine, is microscopic examination. This test cannot, however, be well applied, unless a certain familiarity with the objects and with the various aspects which they present has been first acquired. M. Lallemand give us the following instructions on this head:

> "After coitus there always remains a sufficient quantity of seminal fluid in the urethra, to serve for precise and complete microscopical examination. This may be obtained by pressing the canal shortly after the act, and receiving the drop of fluid from the orifice of the glans on a slip of glass. In this drop of fluid thousands of animalcules may be seen, agitating themselves like so many tadpoles in a pool of stagnant water, only that the tails of the spermatozoa are relatively

> longer and thinner, and that the head presents a brilliant point near its insertion. Generally the number of these animalcules prevents them from being easily examined, and it becomes necessary to spread them out by introducing a small quantity of water and pressing firmly down the thin glass that covers them; they are found most separated on the edges of the fluid. If the water added be of the temperature of the body, their motions become free and lively, and continue so until cooling and evaporation affect them. By avoiding these two causes of disturbance, the motions of the spermatozoa may be kept up during several hours.
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> "However long a time may have elapsed after coitus, there are always spermatozoa in the urethra, provided they have not been washed away by the passage of urine. Although the point of the glans may be dry, and pressure along the whole length of the canal may not produce the least dampness, still on passing urine living animalcules may be obtained from the first drop which escapes. This may be received on the glass, and is perhaps the easiest and most natural mode of obtaining spermatozoa for microscopic examination.
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> "It is evident that the same experiments may be applied in the case of nocturnal pollutions, as well as in other seminal discharges in whatever manner they may occur. But many errors may arise from commencing with cases of disease; for it is during perfect health that the spermatozoa are most active and their development most complete, and they live longer after coitus than after any other kind of seminal discharge." (p. 254.)

There are certain precautions, however, to be kept in view in the search for spermatozoa in the urine, in case of suspected spermatorrhœa. The number of spermatozoa may be very small, especially in the urine passed after diurnal pollutions; and it is, therefore, necessary to search very carefully for them. These bodies, moreover, are often of unusual transparency in cases of disease; and a peculiar management of the light becomes necessary for their detection, a considerable part of it being cut off by a diaphragm, or the cone of rays being sent up from the mirror obliquely, instead of perpendicularly. Variation of the density of the fluid under examination, also, either by adding water, or by permitting evaporation, is frequently useful, by producing a difference between the refractive power of the liquid and that of the bodies of the spermatozoa, when these were so nearly identical as to render the latter invisible or nearly so. "I have frequently," says M. Lallemand, "in cases of spermatorrhœa, failed to perceive anything in the fluid under examination for half an hour, an hour, or more; then suddenly an animalcule has made its appearance; then a dozen; and then perhaps a hundred in the space of a few minutes. The following morning when desiccation has become complete, there are no longer any traces of these animalcules, or, at all events, I have only been able to distinguish their tails, the other parts of them being fixed in the dried-up mucus. The absorption of a drop of water has restored the phenomena observed the night before." (p. 253.)

Directions are given, however, by M. Lallemand for the detection of the seminal fluid in the urine by the unassisted eye; and we shall quote the most essential parts of them, although we consider that the microscopic test should never be dispensed with, when the least doubt exists as to the nature of the case. We here quote from the original, as Mr. M'Dougal has considerably abridged the whole of this portion of M. Lallemand's work.

> "I have desired some of these patients to observe what took place when they passed urine in a bath, and I have in this way obtained some very curious infor-

mation. The attention which these unfortunate persons concentrate upon the exclusive object of their thoughts, gives them great patience and perspicacity for everything which relates to it. Here is what I learned from them, having, in most instances, verified their statements myself.

"In simple and incipient cases, it is thus very easy to observe the semen mingling with the last drops of the urine, as it has as yet much opacity, and contains a number of flocculi and granules which disperse in the urine in every direction. In the most serious and obscure cases, its presence may, by a little care, still be detected by the sudden increase of density which the urine acquires. A kind of riband, like a very thick syrup, descends on quitting the urethra, and even casts a shadow upon the thigh when illuminated by a sufficiently bright light. . . . . . .

"In recent cases we see fall to the bottom of the vessel a number of small granules of a variable size, semi-transparent, irregularly spherical, and pretty much resembling seeds of semolina. We cannot confound them with any urinary salt, because they are seen before the urine cools, and are distinguished by their soft texture, and their non-adherence to the walls of the vessel. On the other hand, neither the prostate, urethra, bladder, or kidneys furnish such granules, especially when the urine is transparent. They proceed from the vesiculæ seminales, as I shall afterwards show, in a direct manner, and may be regarded as certain signs of diurnal pollution. . . . . . .

. . . . . . "When the disease has made more progress, the urine no longer deposits sufficiently large granules to sink to the bottom of the vessel, but it contains a thick, whitish, homogeneous cloud, dotted with little brilliant points, which reach the lower layers, and which have with some truth been compared to the deposit formed by a somewhat concentrated decoction of rice or barley. Hitherto there have never been assigned to this seminal cloud sufficiently precise and constant characters, to allow of its being distinguished from the various deposits which the vesical mucus and the prostatic fluid may furnish; so that practitioners have remained in great uncertainty or complete incredulity respecting it.

. . . . . . "I think the brilliant granules of which I have spoken, should leave no uncertainty concerning the nature of the cloud in which they are observed. Repeated miscroscopic researches leave me under no doubt that such clouds are [illegible]e, for the most part, to the presence of semen in a very altered condition; and I shall show that the brilliant points come from the vesiculæ seminales." (Tom. ii, pp. 352-4-7.)

In the *treatment* of this affection, M. Lallemand observes, it is more important that our attention should be directed to the present condition of the spermatic organs, than to the original cause producing this; and there can be no question that, in many cases, until this great source of irritation be removed, all the efforts of the practitioner and resolutions of the patient will be in vain. To the chapter which directs the suitable treatment, when this diseased condition arises from irritation occurring in the vicinity, we need not advert; seeing that the remedies employed against ascarides, eruptions or fissures at the margin of the anus, diseased conditions of the prepuce, stricture of the urethra, constipation, &c., are those in ordinary use.

Although it is a common but pernicious error to suppose that spermatorrhœa generally arises from an original debility or atonic condition of the genital organs, yet such cases are occasionally met with, "occurring especially in such as have suffered during infancy from an incontinence of urine, in those whose genital organs are not perfectly developed, or whose temperaments are markedly lymphatic." In such subjects we find the pollutions increasing during mild and damp weather, and diminishing under the influence of bracing winds. Tonics are obviously here indicated; and M. Lallemand has derived much advantage from the transmission of galvanic shocks through the penis and perineum; while he has seen nothing but mischief result from the use of cantharides and phosphorus. He thinks well of the ergot of rye, given in doses of from 4 to 20 grains night and morning. Cold bathing has been far too indiscriminately resorted too in spermatorrhœa. It is more especially useful in masturbation and venereal excess, but always proves injurious when, as is usual in the case of diurnal pollution, the great debility prevents the production of a sufficient reaction. Warm, aromatic baths are in such cases very useful; especially when incontinence of urine existed during childhood. Cold applied in a more local manner, as by lotions or the douche, and followed by active friction, is of good service. In well-marked cases, the ferruginous waters, due attention being paid to the condition of the digestive organs, are useful; and although all such medicinal substances, as are given for the especial purpose of exciting the genital organs in impotence from debility, are generally injurious in their operation, yet, when there is an abnormal sensibility of the genito-urinary apparatus, the balsamic remedies, as copaiba, turpentine, &c., given in small doses, sometimes prove of service in allaying it.

Occasionally, seminal discharges arise from the abnormal nervous susceptibility of the genital organs, the least touch or friction inducing peculiar sensations or emission. Great irritation may likewise prevail in the urethra, without local inflammation at the orifice, in the course of the cord, and about the neck of the bladder. These cases occur in irritable subjects, and are best met by narcotics and sedatives, given cautiously however at first. In such instances, five or six-grain doses of camphor often diminish the irritative erections. The introduction every few days of a moderate-sized gum-elastic catheter, which is to be allowed to remain in the urethra for an hour or more, although at first productive of increased suffering, eventually gives much relief by blunting the morbid sensibility of the canal. To produce the full effect of this means, however, more time is required than many patients are willing to devote to it, and in such cases acupuncture of the perineum may be advantageously substituted; this being a means of treatment which M. Lallemand considers has fallen unjustly into disuse. Pollutions maintained by the influence of acquired habit are advantageously treated by it and catheterism.

As we have already seen, the great bulk of cases of spermatorrhœa are connected with a state of irritation of the spermatic organs, varying in degree from simple excitement to actual inflammation. In the treatment of such, M. Lallemand thinks that we have injudiciously departed from the advice of Hippocrates, by substituting cold for tepid baths. His remarks, too, upon the advantages of a milk diet and light regimen, are excellent;—the stomach, however, owing to its usually irritable condition, requires a variety of modifications of diet. Wine, forbidden by Hippocrates, is usually abandoned voluntarily (as is generally beer also) by these patients, owing to their experience of its ill-effects in the aggravation of all their sufferings. They are unanimous in their statements upon this point. All exercise of the organs by coitus when possible, or by the excitement of erotic ideas, must be abandoned, until after convalescence has become firmly established. In the chronic inflammatory state of the orifices of the ducts and the adjacent portion of the urethra, which is the essential characteristic of the form of the disease we are now considering, and the relaxed condition of the parts consequent upon its prevalence, *cauterization* with the nitrate of silver is especially indicated; and to M. Lallemand are we indebted, if not for its first actual application to these cases, (Sir Everard Home having previously so employed it,) at any rate for the far more important service of having perseveringly investigated its claims, and established its right to be considered as the means *par excellence* best calculated to dissipate lingering morbid action, and restore lost tone. He reasonably complains of the conduct of those who have chosen to adopt this part of his practice, without accompanying it with the precautions he deems requisite; and especially enforces the allowing a sufficient interval to elapse after the first application, before again having recourse to it—three or four weeks at least. In the majority of cases a single cauterization will indeed suffice, and at most two; though in some few instances more may be required. We need not enter upon a description of the mode of performing the operation, or of the instrument employed by M. Lallemand (of which Mr. M'Dougal proposes a modification), these having been sufficiently made known in the writings of Mr. Curling, Mr. Phillips, and others.

M. Brâchet related to the Academy of Medicine last year (Bulletin, tom. x, p. 650) some cases of aggravated spermatorrhœa, which he cured by means of a bandage applied to the perineum for several weeks. We do not know that this success has been confirmed by other practitioners; but in certain cases,—as where the patient refuses to submit to cauterization, or the canal is in too irritable a state to admit of its performance, or the practitioner not confident in his dexterity,—the plan would certainly seem worthy of a further trial.

Convalescence, in all but simple and recent cases, is of a slow growth, relapse being, by the force of habit, of such easy occurrence. Hygienic precautions require to be rigidly enforced long after their apparent necessity has ceased. When the health has become sufficiently reestablished and natural desire rekindled, it is highly desirable that the sexual organs should be moderately exercised, both for the prevention of the reacquisition of depraved habits, and for the imparting additional vigour and tone to them. But advice of this kind must not be lightly given, inasmuch as we are not justified in prematurely recommending a matrimonial union for the mere chance of benefiting the patient. Any such experimental proceeding of this kind would be a gross injustice to the wife, and might entail despair and disappointment upon the husband. Renovated powers, on the other hand, are apt to be used incautiously; and indeed what would be abstemiousness in the robust becomes an excess in those whose genital organs have been disordered; and this may easily lead to the reproduction of the spermatorrhœa.

Before concluding the present article, we wish to make a few observations upon the *prevention* of this disease. When once established, from all we have seen and read we are reluctantly obliged to come to the conclusion, that it is by no means so easily eradicated as M. Lallemand represents it to be; and we fear that some of the very rapid cures of long-standing cases he relates were only temporary ones. It is very true that the relieving the morbid state of the urethra by means of cauterization, often exerts at first even a marvellous effect upon the condition of the patient; but in respect to persons whose nervous system has been shattered by years of perverted action, and who, with some returning strength, are so apt to fall under the dominion of old habits and associations, this is only a part and a small part of the cure; the rest much depending upon the possession of a resolution, in which these unfortunates are especially deficient. The less effective our curative treatment, however, the more important the preventive; and this, to be of service, should be commenced at a very early period of life, and form a part indeed of an improved educational management. This is a fertile topic for dissertation, but the few words we have to say shall be confined to the special subject before us: and we touch upon it at all only to suggest to practitioners the propriety of drawing to the matter the attention of those with whom they come into contact, as by their aid alone can it be brought into notice. There can be no doubt that children are more addicted to onanism than is usually supposed; and several of the anomalies that occasionally present themselves in their condition would be cleared up, could we obtain precise information concerning bad habits acquired by themselves or taught them by schoolfellows and servants. It is useful always to bear the possibility in mind; although, of course, to suggest investigation respecting it requires tact and caution. M. Lallemand repeatedly calls attention to the important fact that the genital instinct of children is developed long before the period of puberty; and justly reprehends the carelessness of parents in leaving young girls and boys too much together, and of females in acting before the latter as though sexual considerations were out of the question at this age. It were certainly pleasanter to rely upon the idea of childish innocence at this period of life; but any one who has watched the conduct of even very young children of opposite sexes when together, must feel persuaded that the caution is not given in vain. M. Lallemand also mentions cases of impressions thus early imbibed abiding with and influencing the individual for the rest of life; and we are in possession of the particulars of an instance, in which the fact of a boy some seven or eight years of age assisting at the toilet of a lady, produced so profound an impression, as to induce a most extraordinary train of thought and action for the next thirty years. They are too long for present insertion, but as a true psychical curiosity may be brought under notice at some other opportunity. We also agree with M. Lallemand, that immense mischief is done to the moral purity of lads entering upon puberty by assembling them together in large numbers in schools, where, escaping supervision, and left much to themselves, the most vicious habits are acquired and propagated. Nine out of ten cases of onanism thus originate.

We are strongly of opinion, likewise, that much of the mischief which afterwards occurs to young men arises from the imperfect character of their school education, which has been too superficial to call into employ the higher faculties of the mind, or to lay the seeds for that love of knowledge which might hereafter, in moments of leisure, ripen into attempts at its pursuit. Destitute of the high incentive which this supplies, they abandon themselves to mere sensual enjoyments, or engage in that description of light and discursive reading, destructive of all fixity and energy of thought, which is so much in vogue at the present day, and to which its highest praise of being an innocent pastime cannot be always accorded.

If consulted respecting a case of onanism or nocturnal pollution, the practitioner too often contents himself with the recommendation of sexual intercourse, without taking into consideration the various other means of obviating the inconvenience. These are chiefly to be found in whatever discourages indolence, whether of body or mind; and must be adjusted with discrimination. To the youth of high moral principle and cultivated mind, we should open out the inducements offered for the exertion of the powers of self-control, and for the engaging in some investigations which may occupy his thoughts, or some employment of his leisure which may prove useful to mankind and interesting to himself. To minds of lower calibre, and it is among such persons that masturbation is chiefly found to prevail, we recommend active bodily exercise, carried to the verge of fatigue; and we think that the discontinuance of many of our hardy games and sports is a matter for the sincerest regret. For all, early rising, hard beds, cold bathing or ablution, active exercise, the occasional use of aperients, spare diet, and the free enjoyment of respectable society, especially when females form part of this, are of great importance. In a great number of instances, these and analogous means will be attended with success; but there are, doubtless, other cases in which the establishment of the natural action of the genital organs would be the most effectual remedy; but we maintain that this is by no means so often the case as is generally supposed; and that it is far too indiscriminately recommended by medical men, is very certain. Thus, it is not uncommon to find it quietly advised for patients suffering under advanced spermatorrhœa, who are quite incompetent to act; in whom, even if they could accomplish it, it would only produce additional irritation of the already too excited organs. Moreover, in several cases it is not curativ[illegible] for excesses, even in consequence of marriage, have not unfrequently brought on slight spermatorrhœa or analogous disorders.

In concluding our notice of this subject, we may again express our opinion that Mr. M'Dougal's translation of so useful a work will prove of great service to the profession in this country, by recalling attention to a too-neglected subject. We have carefully compared it with the original, and find the author's meaning very satisfactorily rendered; but, although the curtailments are, upon the whole, judicious, we think they have been made somewhat in the extreme, and have been accomplished sometimes by the amalgamation of portions of separate paragraphs, which constitute an act of injustice towards the author, if done without his approval. We do not either understand in what sense Mr. M'Dougal employs the word "edited," the signification of which seems to be taking a great extension at the present day. In the present instance, the word "abridged" precisely expresses what has been effected.

ART. III.

1. *Correspondence on the Subject of the "Eclair," and of the Epidemy which broke out in the said Vessel.* Presented to the House of Commons by Command of Her Majesty, in pursuance of their Address, on the 23d January, 1846.—*London.* Folio, pp. 94.
2. *Report on the Fever of Boà Vista.* By Dr. M'WILLIAM. Presented to the House of Commons, in pursuance of their Address of the 16th March, 1847.—*London.* Folio, pp. 112.
3. *Letter addressed by Sir William Pym to the Lords of the Council, relative to a report on the Fever at Boà Vista, by Dr. M'William.* Presented to the House of Commons, in pursuance of their Address of May 14th, 1847.—*London.* pp. 16.
4. *Report of a Special Committee of the House of Assembly of the State of New York, on the present Quarantine Laws.*—*Albany*, 1846. 8vo, pp. 313.
5. *Report on the Climate and Principal Diseases of the African Station, compiled from Documents in the Office of the Director-General of the Department, and from other Sources, in compliance with the Direction of the Right Honorable the Lords Commissioners of the Admiralty, under the immediate Direction of Sir Wm. Burnett,* M.D., K.C.H., F.R.S. By ALEXANDER BRYSON, M.D.—*London*, 1847. 8vo, pp. 266.
6. *A Dictionary of Practical Medicine.* By JAMES COPLAND, M.D, F.R.S. Parts X and XI.—*London*, 1847.

THE events which have called forth some of the works before us are of a very interesting and important kind. We are now put into possession of some knowledge both of the terrible mortality prevalent almost every year among our squadron on the West Coast of Africa, and of the causes which produce it. In the lamentable story of the "Eclair," and in the unhappy fate of her devoted crew, there is much to excite deep interest and commiseration, and the history of the fatal disease which succeeded her presence at the island of Boà Vista, is a most important element in the great controversy which has so long been carried on as to the origin and propagation of yellow fever.

The case of the Eclair has indeed revived all the slumbering animosity of the two great sections of contagionists and non-contagionists. With her arrival at the Motherbank immediately recommenced an ancient feud; the adverse parties loudly proclaimed their differences, and vaunted their own peculiar enlightenment. By one side the arrival of the Eclair was considered as a national calamity; the tragedy played within the limits of her narrow decks was to be repeated, with the broad fields of England for its scene, in place of her Majesty's steamer, and London in the 19th century was hardly to be saved from the fate which befel Athens more than 2000 years ago. The other party regarded these fears as puerile, and laughed at the precautions as absurd. They saw nothing remarkable in a fever derived from the pestilential rivers of Africa, lasting for more than two months after departure from the scene of its origin. They considered as unworthy of refutation the opinion that a steamer from the African coast, even without carrying the ominous plague-flag, might chance to introduce among us, during certain months of the year, one of the most fatal diseases known at the present day; and they would hardly condescend to justify their disbelief of an opinion so startling.

What then are the reasons of this extraordinary difference of opinions in men who have witnessed the same disease, and who have had abundant opportunities for studying all its forms? How comes it, that during the last thirty years there has been no subject in medicine so violently debated as the causes and nature of yellow fever, and with such small results? It appears to us, on looking back on this discussion, that it is impossible to avoid the conclusion that it has been premature. It may be suspected to be even at the present time premature. For the settlement of a great question numerous facts are wanted, and without these, induction degenerates into hypothesis, and discussion into controversy. Without a sufficient number of facts the most comprehensive mind wanders, the most subtle intellect is at fault, and conclusions, if such are arrived at, must necessarily be lame and impotent. And throughout the whole great subject of epidemics and contagious diseases, we find a continual want of more accurate observations, of a more extended knowledge of phenomena, of the assistance of an advanced and subtle chemistry. And when, by the aid of advancing science, our descendants shall be able to recognise those mysterious agents of pestilences and plagues which, unindicated by the most delicate manipulations of art, yet testify their presence and activity by effects totally disproportioned to their apparent power, it may be that they will look back with astonishment on our futile guesses, our blind errors, our equally blind truths.

In this remark, that the discussion regarding yellow fever has been premature, it must not be supposed that we do not estimate the advantages of debate. Differences of opinion necessarily imply debate and controversy, but by the word "premature," we mean only that the attempt to arrive at a final decision is as yet impossible, and has therefore when attempted always failed. This consideration should teach us caution and humility, and a proper respect for opinions differing from our own. It is with a full belief that the subject of tropical fevers is yet too little known to warrant a decided opinion on many points, that we proceed to the consideration of the works before us; and we shall avoid to the utmost of our power taking any share in the keen and bitter quarrels which the subject has engendered.

The first point which arrests the attention in entering on this subject is the absolute and remarkable difference between the fevers of hot and cold climates. Contrast the "fièvre typhoide" of Paris with the West Indian yellow fever, and it may well be questioned, how affections so dissimilar can by any number of intermediate phases be made to confess alliance or connexion. Or with the common English typhus compare the malignant remittent fever of Mysore or Batavia, and the propriety of the common appellation "fever" to both diseases may be doubted. Between the different fevers of cold and of hot climates, respectively, there exists a general uniformity or similarity, but between the two great classes there is scarcely any. Each seems to have its geographical limits, beyond which it is unknown, or if an extraordinary combination of circumstances should bestow upon it a temporary existence external to its own domain, a return to the natural order of events never fails to arrest and to annihilate it. To explain this remarkable difference there is but one way—the human frame is the same under both circumstances, and climate bestows upon it no new power of developing from the same cause a new reaction; the causes must have changed; the agents of the two classes of fevers must be radically distinct. There is this resemblance, however, between them, that nearly the same kind of controversy has in either case been carried on: equally in cold as in hot climates, it has been debated whether the different fevers are essentially distinct, or to be referred to a common type; whether the agent is contagious; whether it is so always, or only under certain circumstances; whether other additional causes are to be admitted or not. Transfer these questions to the tropical fevers, and we have the gist of the controversies which have been carried on for the last fifty years. And in either case it is possible that the conclusion may be the same, and that in both instances all the effects may be traced to the action of a single proteiform and capricious agent, acting in accordance with certain definite laws which permit considerable modifications of development. But our present purpose forbids our entering into the parallel between the fevers of hot and cold countries.

The severe fevers of hot climates have been referred to several causes; these may, however, be all reduced into two great classes:

1. The first class of observers attribute these fevers to a particular agent, derived from various sources, according to the peculiar views of each individual; some consider that this is merely marsh miasma; others that it is a distinct and specific agent, contagious or not;* others believe that both these causes are in operation, and produce distinct and distinguishable fevers.

2. The second class of observers reject all definite and peculiar agencies, and contend that certain atmospheric conditions, such as great heat, humidity, &c., acting on a predisposed frame will produce all the symptoms of the most malignant fever. This is the opinion of Tommassini, as developed in his work on the yellow fever of Leghorn in 1804. According to him, the intensest form of yellow fever is but the developed degree of the common bilious derangements common in hot and rainy seasons.† The same opinion has been advocated since 1804 by several writers on West Indian diseases. It was started anew by M. Lassis in the discussion before the French Academy in 1829.

It is to be inquired, first of all, whether it is right to admit the agency of these several causes in the production of the severe fevers of hot climates; and, secondly, whether the resulting fevers are of the same or of a dissimilar nature. These two questions exhaust the subject; it is to be wished that it was as easy to answer as it is to propose them.

1.—AS TO THE AGENCIES PRODUCTIVE OF YELLOW FEVER.

We shall consider this subject under two heads. We shall first debate the point whether a contagious virus or poison, multiplying itself by its passage through the human system, can be proved to produce true yellow fever; secondly, we shall consider whether an agent evolved from marshy and malarious places, with or without the assistance of a high temperature, can produce a yellow fever.

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* The opinion that a specific non-contagious agent produces a fever called "yellow," but totally different from the real yellow fever, is strongly maintained by Rochoux. He gives many differences between the "typhus amaril," as he calls his specific affection, and the "fièvre jaune." (Dissertation sur le Typhus Amaril ou Maladie de Barcelone, improprement appelée Fièvre Jaune. Par S. A. Rochoux, D.M.P. Paris, 1822.)

† Recherches Pathologiques sur la Fièvre de Livourne de 1804. Par J. Tommassini, Professeur de Phys. Parme. Paris, 1812. pp. 73-5.

1. *Is Yellow Fever ever produced by a Contagious Virus?* If this question had been asked two years ago, we should have given an answer to it only at the expense of a long and tedious argument. But the case of the Eclair steamer has thrown a most important light on the subject, and it will be impossible, henceforth, ever again to doubt the possibility of a yellow fever being propagated by contagion. We shall make no apology for entering into a full analysis of the Reports on this particular fever now before the public.

The Eclair steamer was commissioned in August 1844, and sailed for the west coast of Africa in November of the same year. From December 8th to July 4th, 1845, she was employed in watching for slavers off Sherbro and Seabar, and also visited Sierra Leone. Up to March 2d the health of the crew continued good; after this time she sent her boats up the Sherbro and Seabar rivers, and remained at anchor from three to six miles off the shore. From the 3d of April to the 10th of June she had 13 cases of fever, of which 10 were fatal. With two exceptions, these all occurred in men who had been employed in the boats. On July 4th, the Eclair arrived at Sierra Leone, and at this time the health of the crew was improving. The men had limited leave, but several of them slept on shore; they were also employed in cleaning out the Albert, which had remained untouched since the Niger expedition. On the 23d of July she left Sierra Leone, and anchored off the coast till August 9th. During this time there were 15 cases of fever and 6 deaths; the fever was distinctly remittent, and attended with unequivocal black vomit.* On the 9th of August the steamer arrived at Gambia, and left on the 15th; on the 16th she arrived at Goree, and was refused pratique. On the 21st of August she arrived at the island of Boà Vista, one of the Cape de Verde, and at this time there were only 5 cases of fever on board. The numbers increased, however, so rapidly, that on the 31st of August the crew were landed on a small island two miles from Porto Sal Rey, the capital of Boà Vista; the mortality being increased instead of diminished by this measure, the crew re-embarked on the 13th of September, and steamed for England, where the ship arrived on the 28th of September, and was put in quarantine. During this run there were 41 new cases and 12 deaths. After the arrival at the Motherbank there were 9 fresh cases and 5 deaths.

The analysis of all the circumstances connected with the Eclair resolves itself into two distinct portions:

1. What was the consequence of the landing of the crew at Boà Vista, as far as the inhabitants of the island were concerned?

2. What conclusions are to be drawn from the history of the fever in the Eclair herself?

We shall take these questions in order. Boà Vista, the easternmost of the Cape de Verde islands, lies in lat. 16° 5′ N., and long. 22° 55′ W. Its soil is composed of sandstone, on a bed of basalt. The principal towns are Porto Sal Rey, the capital; Rabil, four miles to the southward; Estacia, or the Old Town; and several villages to the north and east. It appears to have been a healthy place for many years. Dr. Almeida, a Portuguese surgeon, states that no disease has been prevalent for thirty-seven years, and Senor Carvahal, a resident on the island for more than fifty years, speaks with equal confidence on this point. Remittent and intermittent fevers are almost unknown at Porto Sal Rey; they are sometimes prevalent at Rabil, on account of a ravine which exists there; in 1821 twenty-one persons died at Rabil from this cause, thirteen in 1827, and fifteen in 1833.

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* Report on the Climate and Diseases of the African Station, p. 185.

Lind, in his 'Essay on Diseases of Hot Climates' (p. 151), speaks of the white sand of Boà Vista as injurious to health, and at page 84 he says that St. Antonio and St. Nicholas are the only two islands of the Cape de Verde "where strangers are exempted from a general sickness during the rains," but there is no mention of the inhabitants of these islands being unhealthy. For some time before the arrival of the Eclair, it is certain that the island of Boà Vista was perfectly healthy, and Dr. M'William states that this was true also of all the other islands of the group. We have already said that the Eclair arrived on the 21st of August, 1845. On the 30th of August the crew was landed on a small island two miles from Porto Sal Rey; the steamer was boarded by labourers from this town and from Rabil, and was cleaned, and on the 13th of September the crew re-embarked. During this time the crew were in a kind of quarantine, and avoided intercourse as much as possible. So great was the dread of the disease among the inhabitants, that Dr. M'William mentions (p. 82) that the consul had great difficulty in procuring labourers. It would appear, however, according to Sir William Burnett, that the crew managed to smuggle vast quantities of spirits, and, of course, it is possible that more secret intercourse went on than can be gathered from the Reports. Certain of the inhabitants were, however, brought more or less in contact with the people of the Eclair. These were—1st, the military guard at the Fort; 2d, labourers from Porto Sal Rey, Rabil, and Estacia employed on board the Eclair, 41 in number; 3d, labourers employed in the launches, or at a coal-heap on the small island, 46 in number; 4th, washerwomen who washed the officers' clothes, 17 in number. In addition, Captain Estcourt, the commander of the steamer, lived in Porto Sal Rey, at the consul's house; the gun-room and ward-room officers and midshipmen occupied a house in Porto Sal Rey; and leave was given to the warrant officers and a few of the men, and one man stopped in Porto Sal Rey for two nights. We shall allude to these cases in the order in which they stand.

1. Military guard at the Fort on the small island.—During the time the people of the Eclair remained on the island, a guard composed of a corporal and two or three men, were on duty at the Fort. This guard was relieved three times, and 10 men were thus brought into personal contact with the crew. The corporal of the 1st guard went on duty, on the 30th of August; on the 31st he felt indisposed, but did his duty, and returned to his barracks in Porto Sal Rey; he was then relieved off duty and went to his own house, where he was ill for a month with general pains, severe headache, and vomiting. It is very doubtful what this case was, and we shall therefore put it aside altogether. The privates of the 1st and 2d guard remained healthy. On the 13th of September the

Eclair left; on the 14th or the 17th of September the corporal of the 3d guard was taken ill. Dr. M'William gives the former, and Mr. Consul Rendall the latter date. On the 15th or the 18th, a private of the 3d guard was taken ill; in both cases the symptoms were "fever, wildness, and constant black vomiting." (Report, p. 23.) The corporal died on the 17th or the 20th of September, and the private on the following day. A man named Alves, who had belonged to the 2d guard, and who assisted in burying these two men, was taken ill a day and a half afterwards, and was removed to barracks in Porto Sal Rey. On the 21st or 24th of September, four days after the death of the corporal, the two remaining privates of the 3d guard were taken ill, and they were removed to Porto Sal Rey, but for the sake of precaution were not sent to barracks, but lodged in a portion of the town called Pao de Varella; at some period of their illness they had delirium and black vomit. The names of these men were Barbosa and Manoel, and the introduction of the fever into Porto Sal Rey is attributed to them. A 4th and 5th guard occupied the Fort after this, but as this was subsequent to the departure of the Eclair, we shall not dwell on the details; three out of four men composing these guards being attacked with fever, the Fort was abandoned towards the middle or latter end of October.

2. Labourers on board the Eclair.—Forty-one were employed; of these some were on board when the sick were landed, and also when they were reshipped. Sixteen went into the hold, pumping water and stowing provisions, 10 did not go below the lower deck; the rest are not mentioned. Of these 41, one man, named Luis Pathi, was taken ill on the 17th or 18th of September. None of the others were attacked at this time, but many had fever in November, December, and January. Luis Pathi was a labourer of Rabil; after the Eclair left he went to a festa at Moradinha, where he was taken ill; he remained there eight days, and was then carried to his own house at Rabil. To this case we shall have to return immediately.

No mention is made in any of the examinations of any unpleasant smell or odour being perceived in the hold of the Eclair. It would appear, however, that she was not thoroughly cleaned at Boà Vista. Dr. Bryson states that, when she was recommissioned, a large collection of mud, fully three inches in depth, was found upon that portion of her bottom occupied by the boilers and machinery. (Climate and Diseases of Africa, p. 223.)

3. Labourers employed at the coal-heap on the small island and in the launches, 46 in number, of whom 23 were brought into personal contact with the crew of the Eclair, who were not sick. None of these men were taken sick at this time.

4. Seventeen washerwomen washed the officers' clothes; of these none were taken sick till November and December. It is, however, expressly stated (Correspondence, p. 28) that the clothes and bedding of the diseased persons were thrown overboard, so that the exemption of the washerwomen is not material to the argument.

5. It is stated that the owner of the grog-shop to which the men who had leave resorted was indisposed after their visit, but it is not clear from what cause. Two prostitutes visited by the Eclair's people stated that they had slight fever four or five days afterwards. This is probably quite immaterial evidence, and we shall not further allude to it.

It appears from this statement of occurrences that 64 men living at Boà Vista had greater or less intercourse with the people of the Eclair; that, in addition, some of the officers and crew of the Eclair were for several days in Porto Sal Rey, but that bad consequences resulted only in a few instances. Allowance must be made for the dread of the disease which undoubtedly prevailed in the minds of the majority of the labourers, and which must have deterred many from free communication with the crew. It must also be remembered that Captain Estcourt adopted, as much as possible, measures of seclusion and quarantine.

It also appears:

1. That the men who were chiefly in contact with the crew and with the sick men, and who were in the sick men's apartments (Report, pp. 20-4) (that is to say, the soldiers in the Fort), suffered much more severely than any other class: thus of 10 men who were on duty between the 30th of August and the 13th of September, 5 had fever, and another was ill of some complaint or other for a month.

2. The labourers on board the Eclair, many of whom were also in the hold, suffered only in the proportion of 1 to 41, although these men were all susceptible of fever as after events proved. This is the more remarkable, as it is stated that so malignant were the exhalations in the Eclair, that the clerk, purser, and lieutenant of the Growler steamer, who formed a board on the purser's stores of the Eclair, were all attacked with fever immediately afterwards. (Correspondence, p. 74.) There had, however, been fever on board the Growler, and we doubt, with Dr. Stewart (Correspondence, p. 89), whether these individuals really derived the disease from this temporary visit to the Eclair.

The result of this examination is—1st, that the development of the fever appears to have been strictly in proportion to the amount of intercourse; and, 2d, that within a reasonable time after the departure of the Eclair there were 3 persons ill with fever at Boà Vista, and 2 were already dead. These 5 persons had all been in contact with the crew of the steamer.

Is there then any evidence of these 3 persons having any active share in the production of the fever which shortly afterwards desolated the island? If it can be shown that there is a strong probability that these men communicated fever to their attendants, then we may apply the argument to themselves, and contend that there is strong reason to believe that they derived their disease from an analogous source.

The period of incubation in the Boà Vista fever must be first determined. If we consider the incubative period as occupying the time from the death of one person to the attack of the next in the same house, we find evidence that the time varied from 2 to 8 days. As an instance of the first period we will cite the case of Antonio Perica (Report, p. 30), and for the second, the case of his wife, who was taken ill 7 days after the death of her husband, or the case of the third child of Luis Pathi (Report, p. 43), who was attacked 8 days after the death of his sister. There are many other corroborative instances, proving the period to have been often as long as this. It is true that these persons may have been exposed to some other source of infection, but it is impossible to avoid all uncertainty, and it is absolutely necessary to determine in some way the incubative period, and to judge the question of contagion by rules furnished by itself.

It has been said that 3 cases of fever did, on a certain date, occur at

Boà Vista; these were the cases of the two soldiers, Manoel and Barbosa, who were taken ill at the Fort, but were removed to a house in the district of Porto Sal Rey, called Pao de Varella, and the case of Luis Pathi a labourer, who had been on board the Eclair, and who lived not at Porto Sal Rey, but at Rabil. There is therefore a double argument to be carried out, and if it should appear that the persons in contact with these men suffered first, then there is a double reason for attributing the disease to the intercourse allowed.

The following tables show at a glance the results of the inquiry.

1. As to the two soldiers from the Fort.

TABLE, No. I. *Names of persons living in the houses in the row Breira, in Pao de Varella, adjacent to that in which the soldiers were living. There are only four houses in the row.* (Report, pp. 26-31, and p. 85.)

Theresa Maria Jezus—next door above.
Anna Gallinha } —next door below.
Anna Texeira }
Joseph Lisboa—next door to Anna Gallinha.
Manoel Affonso—twenty yards away.
Gertrude Bent—next door to Manoel Affonso.

TABLE, No. II. *Names of those taken ill first in Porto Sal Rey, with the dates as far as they can be ascertained. Those marked thus * fatal.*

*Anna Gallinha—October 12th.
Anna Texeira—October 19th.
*Manoel Affonso—October 17th.
*Maria Nazarinha—between October 20th and 25th.
Theresa Maria Jezus—between 19th and 24th October.
*Gertrude Bent—21st October.
*Antonio Perica—20th or 21st October.
*Lisboa—21st October.

The following table carries the evidence a step farther, and proves more conclusively the intercourse between the different persons attacked with fever, and whose names are included in Table, No. 2. We must, first of all, recall the dates to our readers' memory. The Eclair sailed the 13th of September; the two soldiers died in the Fort on the 17th or 18th, or on the 20th or 21st September. The two soldiers, Manoel and Barbosa, were removed to Pao de Varella on the 24th or 26th September, the dates are not perfectly clear; they remained in the house till the 2d or 4th of October, and were then removed to the barracks. Anna Gallinha was taken ill on the 12th October.

TABLE, No. III. *The names marked thus * are those of persons attacked with fever after the intercourse referred to, within the incubative period.*

1. Persons most in contact with Manoel and Barbosa.	*Anna Gallinha, who cooked for them.	
	*Sylvester Romess, whose wife also washed for them.	
	*Anna Texeira, who visited them often.	
2. Persons most in contact with Anna Gallinha.	*Anna Texeira, who nursed her.	
	*Manoel Affonso	often in the house.
	*Gertrude Bent	
	*Maria Nazarinha	
	*Lisboa, lived in the next room.	
	Piedad Angelica.	
3. Persons most in contact with Anna Texeira	Dr. Kenny—not taken ill for some time afterwards.	
	*Her son, who nursed her.	
	*Theresa Maria Jezus.	

4. Persons most in contact with Manoel Affonso.	*Luis Ignes, visited him often. *Antonio Perica, who also carried the corpse to Rabil. *Gertrude Bent. *The wife of Joachim des Neves—she had also visited Anna Gallinha.
5. Persons in contact with Sylvester Romess	*His child, niece, and wife. The wife also had washed the clothes of the soldiers, but she was taken ill last of the family.
6. Persons in contact with Antonio Perica	*His wife. *Eusebio da Luz, who nursed him. *A girl in the house.
7. Persons in contact with Lisboa	*A son of Senor Carvahal, who was two nights with him.

There is not mentioned in the Report the name of a single person who lived in Pao de Varella, or who was in personal communication with the sick soldiers who was not attacked with the fever at the time; and of course it is understood that there was no other fever at this time prevalent in the town.

If our readers will now glance over the names contained in these three tables, they will find that each is almost a copy of the others, and they will find that the proof is complete that certain persons living nearest and most in contact with the two soldiers were first attacked. This is a fact, and is independent of all explanation or hypothesis of contagion.

After this time (the 21st to the 25th October) the fever appeared in other parts of Porto Sal Rey, and no attempt is made to trace communication further. It did not reach its height, however, till the latter end of November, or more than two months after the departure of the Eclair. It disappeared altogether by the end of April.

2. As to Luis Pathi, the labourer of Rabil. At the same time that fever was thus spreading, as from a centre in Porto Sal Rey, a course of events, almost the counterpart to these, was taking place at Rabil. Luis Pathi, a labourer on board the Eclair, returned to his house in Rabil on the 13th or 14th September. He had been employed on the lower deck, and was on board when the sick were reshipped. On the 15th September, he went to a festa at the neighbouring hamlet of Moradinha, and while there, on the 16th, 17th, or 18th, he was attacked with illness; he remained 8 days in the hamlet, and was then carried to his own house—his symptoms were headache, general pains, and fever. Ten or eleven days after his return to Rabil, that is to say, on the 3d, 4th, or 5th of October, his daughter was taken ill, and died in 3 days, with suppression of urine and black vomit. It will be remembered that the first death in Porto Sal Rey, that of Anna Gallinha, did not occur till the 16th October. Four days after the death of the daughter, another daughter was taken ill, and died in 4 days, with the same symptoms. Eight days after this second death, his son was attacked, and died in 5 days. On the day of this last death the wife was attacked, and died in 15 days, with black vomit. Previous to this, however, several cases had occurred among the neighbours.

The following are the names of the owners of the houses immediately adjacent to Luis Pathi: Manoel Fachina, Joaquim Marques, Joaquim Pathi, Manoel Rosa, Luis Delgado Nazario.

The following are the names of those first attacked in Rabil, with the dates, as far as they can be ascertained:

1. Luis Pathi—September 18th.
2. Wife of Manoel Fachina—soon after the return of Luis Pathi to his own house, viz. the 26th or 28th September, she had been a good deal in the house of Pathi. (Report, p. 64.)
3. Child of Manoel Fachina—very soon after the mother. This child died about the same time as Luis Pathi's child, or, according to some evidence, before.
4. Two children of Luis Pathi—4th and 11th October.
5. Manoel Marques—very early in October.
6. Joaquim Marques—between the 8th and 12th October.
7. Joaquim Pathi—about this time.
8. Family of Joaquim Pathi—soon after the father.
9. Manoel Rosa—immediately after Joaquim Pathi.
10. Child of Manoel Rosa—two days after her father's attack.
11. Luis Nazario—two or three days afterwards.

It will be seen at once that the names in the two lists correspond, and, in addition, it appears clearly that all these persons visited each other, but it would be waste of space to tabulate this. After this period the disease spread through the whole of that district of Rabil called Cabeçada, in which Luis Pathi lived; it attained its maximum of intensity in November and December.

In reading over the above history, it is impossible to avoid being struck with the great value of the independent testimony derived from Porto Sal Rey and Rabil. In both cases a centre or focus of the disease is proved, and in both cases, the first persons sick had been connected with the Eclair. If Luis Pathi had been a resident in Porto Sal Rey, it might possibly have obscured the deductions from the other cases; but separated as he was by several miles from the two soldiers, it is confirmatory evidence of the highest value.

The disease did not spread for some weeks from Porto Sal Rey and Rabil into the surrounding country. In every case Dr. M'William succeeded in tracing communication, and it is remarkable how clear the evidence is as to a single person being first ill in each village, and as to the radiation of the disease to all appearance from this individual. But as we have so fully examined the cases at the two principal towns, we will now merely extract Dr. M'William's remarks on the introduction into the eastern villages. The names of the eastern villages are Joao Gallego, Fundo das Figuiras, and Cabeça dos Tharafes.

> "Cabeça dos Tharafes was the first of the three villages in which fever appeared. (It appears to have been late in October, p. 76.) A young mulatto girl, daughter of a Captain Aniceto, arrived at the village from Porto Sal Rey, where she had been living in the house of Antonio Joaquim Lima where there was fever; the next day she was taken ill. The mothers and sisters were in succession attacked, and a woman who lived in the house immediately behind Aniceto was about the same time laid up; she had, however, just come from Rabil. The next person attacked was an old woman named Domingos, she soon died.
>
> "Joao Gallego was in a state of perfect health on the 1st of November, when Pedro Moraes and Ignacio Silva, soldiers, came from the barracks in Porto Sal Rey, where the fever was raging, to the houses of their relations in the former town. They were both laid up immediately after they arrived, and in a day or two afterwards the mother of Ignacio Silva was attacked with fever and died. Three others of the same family were about this time taken ill, and two of them

died. A woman named Rufina Mends, who lived close to the soldiers, was next attacked, and she also died. Then the family of Nicolo das Neves, but they all recovered. From this neighbourhood the disease seemed to radiate in all directions through the town.

"One man was said to have been ill with fever at Fundo das Figuiras in October, but it seems more probable that the first sick person there was a man called Joao da Curz, who came from Rabil about the 19th November. This man had made a boast that he was proof against fever, and accordingly slept in a bed at Rabil in which two persons had died. Dr. Almeida placed him and some others who were sick in the house by themselves, and also prevented communication between the villages. To whatever cause it was due, there seems no doubt that in Fundo das Figuiras there was no further extension of the fever, until the middle of December, when a man named Antonio Jose came to the place from Joao Gallego, where he had left his sister on her deathbed from fever. He was soon laid up, and the fever from that time spread, as Dr. Almeida thinks, by those who nursed Jose becoming infected." (Report, pp. 92–3.)

It will be observed, from this examination, how singularly the fever spread in Porto Sal Rey and Rabil from two particular localities. If this circumstance, and if the presence of the two sick soldiers in one place, and of Luis Pathi in the other, be merely accidental, then the origin of the disease is perfectly inexplicable.

In fact, under any circumstances, only three hypotheses can be devised to account for this malignant and fatal fever.

1. That it arose from some peculiar unhealthiness of the climate of Boà Vista, arising from local causes.

2. That it was generated by unusual and peculiar atmospheric conditions, in which case Boà Vista must have been only equally exposed with the other islands of the Cape de Verde.

3. That it was propagated by means of multiplication of its poison by the action of living systems.

The first hypothesis may be at once set aside, as there is evidence that for many years Boà Vista had not been subject to fever beyond the slight remittents arising from the ravine at Rabil. Admitting this local taint, it must also have simultaneously arisen in two particular localities to which sick men had been accidentally taken, forming an almost incredible coincidence of events. Moreover, it is a remarkable circumstance, that the part of Rabil most exposed to the influence of that portion of the ravine chiefly productive of malaria was most exempt from the ravages of the epidemic.

The second supposition, that the fever was generated by unusual atmospheric phenomena, is the one adopted by Sir W. Burnett. It appears from the letter of Mr. Consul Rendall that, in the early part of October, there was an unprecedented degree of heat and heavy falls of rain, so that stagnant water appears to have collected in considerable quantity at the back of Porto Sal Rey. Dr. M'William doubts apparently the prevalence of these circumstances, but we think they ought to be admitted. There is, however, great uncertainty as to the power of such circumstances to produce, in a healthy climate, a fever so peculiar and severe; there is a strong probability also that if they did so produce it, the attack would have been more general, and distributed in several places at once over the whole town, or the whole country, as always happened in the case of the Asiatic Cholera, which never could be traced from foci. It is obviously improbable that such general abnormal atmospheric phenomena should have limited their baneful effects to Boà Vista, and should not have extended to the islands immediately adjoining. But, in addition, how could we account for the undoubted cases of yellow fever which occurred in the middle of September, before the prevalence of any unusual atmospheric states, for instance, the four soldiers of the guard and Luis Pathi? If these were produced by general vitiation of the atmosphere, how is it that the general attack did not take place for nearly a month afterwards in Porto Sal Rey and Rabil (having, in the interval, been slowly extending from person to person), or for nearly two months in the northern and eastern villages?

Suppose, on the other hand, the disease to be derived from the Eclair, and the march of the epidemic is readily explained. According to the ordinary laws of contagion, the greater number of those exposed to the morbific influence escaped, but some of those chiefly in contact suffered. And, as if to clear up all doubts, and afford most striking corroborative evidence, two distinct foci of contagion are introduced into the two chief towns on the island. Gradually all those nearest to the sick men in either locality, and who had been chiefly about their persons, fall sick; the time of such attacks is found to accord with the incubative period adduced from evidence. The presumed contagion is traced from individual to individual, until the sick persons are too numerous to allow of analysis. Every village, then, gradually receives the disease, and in every instance its introduction is traced to an individual, and its propagation from this individual as a centre. (See pp. 107–8.)

If we had wished to have tried a great experiment on this subject, with all available precautions, and with all the aids against fallacy that imperfect human reasoning can furnish, it would have been difficult to have devised any more complete and conclusive than that with which accident, or, we should say, if it be not profane to judge of the arrangements of the universe, a providential accident has furnished us in the case of Boà Vista.

We have adopted the evidence adduced by Dr. M'William, because we have felt bound to give evidence to what is honestly and candidly stated by an observer who must have felt the immense responsibility that rested upon him in drawing up his Report. If he has not taken all possible care to sift his testimony, and to lend an unbiassed ear to conflicting evidence, then the errors he may introduce into medicine are incalculable. If the premises with which he has furnished us are wrong, then we shall henceforth doubt whether any evidence on this subject can be made available. But if, as we believe (and upon which belief we have acted), his evidence has been collected with the necessary impartiality and care, then we cannot but believe that as strong a proof of propagation of a disease from individual to individual has been made out as can be found in the whole history of contagion.

The question of the identity in nature of the fever of the Eclair and that prevalent at Boà Vista need not detain us long. Fortunately, or unfortunately, the symptoms are too peculiar to be mistaken even by the uneducated Negro; pyrexia, delirium, suppression of urine, black vomit, are the leading symptoms universally given in reply to the inquiries of Dr. M'William, and, in addition, that gentleman had an opportunity of witnessing some straggling cases of the disease so late as the 30th of May, and of recording the well-known symptoms of yellow fever.

We shall now dismiss the subject of Boà Vista, and return to the subject of the fever on board the Eclair; we have to inquire what arguments can be drawn for or against contagion from the history of the fever among her crew.

The men on board the Eclair, from the time she left Sierra Leone to the 11th October, may be divided into three classes:

1. The original crew, who had been exposed to the causes of disease prevalent on the African coast.

2. Additional hands from the Growler joining at Boà Vista, who had also been exposed to the same morbific influences, viz. Drs. M'Clure and Coffey, and 7 seamen.

3. Individuals who had not been on the coast of Africa, viz. Mr. Burnard and 2 sailors who joined at Madeira; the pilot, Dr. Rogers, Dr. Stewart, and two or three other medical officers who joined in England.

Dr. M'Clure, Mr. Burnard, the pilot, Mr. Coffey, and Dr. Rogers, were all attacked with the fever, and the three former died.

All these individuals agree in the fact of an equal exposure to the exhalations from the timbers or the bilge-water of the Eclair. It is necessary therefore to determine, if possible, the effect these circumstances had upon them.

It is of course undeniable that exhalations from the timbers of a ship, or from a foul hold and bilge-water, do occasionally seem to produce fever of a bad type. We shall have occasion to refer to this subject in an after part of this article in connexion with the opinions of Dr. Wilson. We shall merely here refer to one additional fact mentioned by Sir W. Burnett. "When the holds of the Growler were opened at Woolwich (1845), after her return from Africa, two men, who slept directly over the hatchway, were seized with fever possessing all the characteristics of yellow fever, and in the course of a few days they both had black vomit, and died in the marine infirmary." (Climate and Diseases of Africa, p. 224; see also Correspondence, p. 71.)

Is there any proof that such cause or causes were active on board the Eclair?

It is impossible to decide definitely this question; but it appears to us that the negative is the most probable answer, for the following reasons:

1. Because only 1 out of 41 of the Boà Vista labourers who were on board the Eclair were attacked.

2. Because the Eclair was fumigated and watered at Boà Vista, and, although not thoroughly cleaned, must have been materially altered for the better, if there were any exhalation going on in the hold, whereas the fever continued to spread with great virulence after this date till her arrival in England. We cannot but suppose that a temporary check must have been given to the fever by these measures, even if, by the gradual reproduction of the poison, it had afterwards returned. It is true that three officers of the Growler holding a board on the purser's stores at Boà Vista were attacked with slight fever (Capt. Buckle's Letter, Correspondence, p. 44); but, in addition to the uncertainty respecting the nature of this fever, there was at the time sickness of a similar kind in the Growler.

But if it be considered doubtful whether there was any morbific exhalation from the unclean hold or bilge-water of the Eclair, how are we to account for the illness of several of the individuals belonging to the third class, who had not been exposed to an African climate? Clearly only by intercourse with the sick. It would, however, be impossible to assert that this was the true cause, from the limited number and from the uncertainty of the fact before us, if it were not that we are able to fall back on our previous conclusions, and to call in aid the decided proofs of contagion furnished by the evidence taken at Boà Vista. If the disease had decided contagious properties at Boà Vista, *a fartiori* it must have possessed the same property on board the sloop.

Moreover, we find, as a general rule, that when a ship attacked with non-contagious remittent leaves her unhealthy locality, and goes out to sea, either into a colder latitude, or even within the tropics, that the fever ceases after a certain time. To the exceptions to this statement we shall presently recur. But the Eclair adds another case to those on record of the increase of the disease after removal from the presumed cause. Thus, after arrival at Boà Vista, from the 21st of August to the 30th, the day when the sick were landed, there was a considerable increase in the number of cases, and this cannot be attributed to the influence of the land, or to the abuse of ardent spirits by the crew. In fact, the sick men were placed, during these nine days, in more favorable circumstances than they had been at sea, being moved into the captain's cabin, &c., and yet the mortality increased. Therefore, there must have been a propagation of poison on board the vessel, and it is by far the most probable that this was by the living systems, and not by vegetable decomposition of any kind. But to this subject we shall return in discussing the origin and nature of the fever on board the Eclair.

It is not a little singular how closely this case of the Eclair tallies with the well-known case of the fever on board H.M.S. Bann, reported by Sir William Burnett.* The Bann left Sierra Leone on the 27th of March, 1823, with fever on board, of a malignant type. How she obtained this is of no consequence to us at present. She arrived at Ascension on the 25th of April, by this time the fever had travelled completely through the ship's company, beginning forward, and gradually coming aft; only 16 escaped out of a complement of 114. The garrison at Ascension consisted at this time of 35 officers and men, and several women and children, all in good health; the sick of the Bann were landed, and no restriction was placed on intercourse (p. 14). On the 28th of April, one of the garrison was attacked with fever, resembling that prevalent on board the Bann, but recovered, and was convalescent on the 7th of May. On the 11th of May a boy was attacked and died, and soon afterwards 6 men, 2 women, and 7 children were attacked. At this time the fever had nearly ceased in the Bann, after causing a mortality of 34 out of 98; it went on, however, daily increasing in the island, and attacked 28 officers and men, of whom 15 died; 6 women, of whom 4 died; 2 boys, who both died; and 7 children, who all recovered. While the Bann was suffering from fever, H.M.S. Driver arrived from the African coast in perfect health; 4 of her crew visited the Bann, and 3 of these were immediately attacked; 2 were sent on shore and 1 died on board. The Driver immediately went to sea, and the disease was arrested. The Bann soon after this went to Bahia, on the coast of Brazil, to recruit; and while there, there is some further evidence of contagion, but as it is rather indefinite we may disregard it.

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* Official Report on the Fever which appeared on board H.M.S. Bann, and on the Island of Ascension in 1823 By Wm. Burnett, M.D. London, 1824.

The above case seems, as in the instance of the Eclair, to fulfil all the necessary conditions of the inquiry. The origin of this fever on the island of Ascension is in the highest degree improbable; the island was healthy, and although, as Sir William Burnett remarks, a "bilious remittent fever" had been known to prevail after the turtle season, this was of mild type, and very different from the fatal disease which appeared after the arrival of the Bann. The only other sources are the exhalations from the hold of the Bann or contagious influence. The effect on the four individuals from the Driver might be ascribed to exhalations, although it is stated that the hold was minutely examined at Bahia, and found to be clean (Report, p. 17); but how could such exhalations affect the garrison? We are absolutely driven to the opinion expressed by Sir William Burnett, "that there is just reason to believe that the disease was introduced into the island by the Bann." (Report, p. 53.)

There are some curious similarities between this case and that of the Eclair. In both instances the fever became increased in violence from the influence of the shore. Thus, at Ascension, the mortality in the garrison was over 50 per cent. of those attacked, while it was only 16 per cent. in the crew of the Bann, although they were under the most unfavorable circumstances of ventilation, cleanliness, &c. So also at Boà Vista, it will be remembered how rapidly the sick list became augmented when the sick were landed. In both cases also the coloured people on board escaped, or nearly so. A few of the Kroomen of the Eclair were attacked in England, but till after the departure from Boà Vista they had enjoyed good health. The negroes of the Bann equally escaped. (Report, p. 23.) It is, however, perfectly well understood that the absence of predisposition does not constitute any argument against contagion.

If these cases of the Eclair and the Bann be taken together, agreeing as they do in all their main features, it is logically impossible to doubt the existence of a multiplying or contagious virus. There are, however, some other cases nearly equally strong, to which we shall now briefly allude.

On the 16th of May, 1795, the Hussar frigate captured the French ship Raison. The Raison had been employed at Guadaloupe as a prison ship, and when captured her crew were suffering from yellow fever. In shifting the prisoners, great care was taken to receive on board the Hussar only those in perfect health;* notwithstanding this precaution, the same fever soon afterwards broke out among the crew of the Hussar. The immense value of this evidence, if correct, is at once apparent, because there are no modifying or doubtful circumstances. The disease was imported at sea, far from any land; the hold of the Hussar was clean, as proved by the previous health of her crew, and in fact there is only one supposition to account for the origin of the fever, viz. that it was introduced by the crew of the prize. But Dr. Bancroft, examining the books at the Navy Office, found that the whole of the prisoners' names were entered on the muster-rolls of the Hussar, and that prize head-money had been paid for all of

* Select Dissertations. By Sir Gilbert Blane, Bart. London, 1822. p. 323.

them; he inferred, therefore, that no selection of healthy from sick had been made, that no fever existed on board the Raison at the time of her capture, and that consequently the whole story was a fable, invented by the surgeon of the Hussar. It is a melancholy circumstance, that the ardour of controversy should have led Dr. Bancroft to make such aspersions on the character and honour of the surgeon, as it appears that he was wrong in the interpretation of the fact; it is stated by Sir Gilbert Blane that prisoners of war, whether wounded, sick, or well, whether remaining on board of their own ships or transferred to the capturing ship, are entered on the books of the capturing ship.[*] There is, therefore, no reason to doubt from this reason the evidence of the surgeon of the Hussar; and as there is evidence, that when the Hussar arrived at Halifax, on the 28th of May, she was put into quarantine because she was affected with a malignant fever, it is difficult to conceive that this should have arisen in any other way than that stated by that gentleman. Fourteen men sent from the Hussar to navigate the Raison lost nine of their number in 12 days; but as these men were of course exposed to the exhalations from the timbers of this ship, this fact is useless for our present purpose.

A case of an opposite kind is related by Caillot.[†] A French ship of war affected with yellow fever captured a merchantman, and sent on board some of her crew to navigate her; soon after the French sailors came on board yellow fever broke out in the prize, and nearly all the crew died of it. We will not multiply quotations, these are sufficient for our purpose; but the well-known case of the Scout sloop of war may be referred to, as the contagious nature of the fever on board has been admitted by Sir W. Burnett (Report, p. 34), and we may also refer to the cases of La Pique and the Majestic, recorded by Gillespie.[‡]

We have selected these cases because, from the small number of modifying circumstances, they appear to us peculiarly definite and satisfactory; each case is separately strong, united they form evidence, which is incontrovertible. One of the most eminent opposers of the doctrine of the contagion of yellow fever thus expressed himself in 1827:

> "It has been said justly that the negative facts of contagion, were they ten thousand, would fall before a single positive fact. Truly, if it can be proved by one authentic example that the yellow fever has been communicated from a diseased to a sound individual by the transmission, mediate or immediate, of a virus derived from the diseased person, there will be no possibility of denying the existence of this virus, and all the conditions of contagion will be fulfilled."[§]

If this be true, and we presume it will be admitted by all, the few decided and definite instances we have selected have advanced us some way in the inquiry into the causes of yellow fever. It is certain that a contagious virus, an agent multiplying itself in the human system, is present in certain cases of yellow fever. But from these facts are we entitled to generalize and to assert that in all cases of yellow fever a contagious virus is present?

[*] See letter from the Chairman of the Victualling Office, Select Dissert., p. 324.

[†] Traité de la Fièvre Jaune, par Louis Caillot. Paris, 1815. p. 202

[‡] Observations on the Diseases of His Majesty's Squadron on Leeward Island Station, between November 1794 and April 1796. By Leonard Gillespie, M.D. London, 1800. p. 53.

[§] Discours prononcé par M. Sedillot sur la Rapport de la Commission chargée d'examiner quelle est la valeur des documens recueilli par M. Chervin relativement à la solution de la contagion ou de la non-contagion de la Fièvre Jaune. Paris, 1827. pp. 12–13.

In order to answer this question we may adopt one of two ways,—we may examine separately each epidemic of yellow fever, and decide from recorded evidence as to the probability of its contagious nature. There are great objections however to this course. Our readers would scarcely credit the immense quantity of controversial matter which a single epidemic like that of the West Indies, or Philadelphia in 1793, or those in Spain in 1803, 1811, 1819, or 1828, have elicited. And in proportion to the extent has been the bitterness of the dispute. Men, whose only object has been the pursuit of truth, and who have, to the best of their abilities, honestly followed the path which seemed to lead towards her, have allowed themselves to betray the utmost rancour and hostility towards those who, with equal honesty of purpose, adopted views different from their own. It would be invidious to mention names, although many of the most active partisans on either side have left the scene of their conflicts, and have abandoned the field to those whom a deeper conviction of the valuelessness of intemperate controversy has taught humility and caution. And, moreover, as in all cases which have been much debated, so many statements and counter-statements, so many proofs and so many refutations, have to be balanced, that one hesitates to decide, and to fix on the truth hidden beneath this cloud of words. As an instance in point, let us refer to the Report of the French Commissioners in Spain, Messrs. Bally, François, and Pariset,—to the debates on the subject before the Academy, —to M. Chervin's critical examination of the Report of the Commission—to the Report of the Committee of the Academy upon M. Chervin's paper—to the various documents and speeches which this examination originated,—the whole forming a library in itself, and being encompassed with all manner of contradictions, doubts, and fallacies, and incapable of satisfactory elucidation. Or, as another example, let us take the presumed introduction into Grenada by the Hankey, and the subsequent epidemic in the United States. We feel an unconquerable reluctance to rekindle the ashes of an extinct dispute, and we shall as much as possible leave equally undisturbed the stubborn advocacy of Chisholm, the keen pleading of Bancroft, the philosophical hesitations of Rush, as well as the eloquent appeals of M. Pariset, and the bitter criticism of Chervin.

Moreover, this examination of evidence, difficult as it is, and doubtful from its very nature, has been perplexed by an insufficient understanding of the grounds of debate. If the action of the specific virus of yellow fever were so peculiar and well marked, as to render the diagnosis as easy as that of smallpox or measles, the question would be much simplified—but this is not the case; it is admitted on all sides, that severe marsh fevers, in certain geographical limits, have a close resemblance in symptoms to the contagious yellow fever. So also, it is said, have fevers arising simply from a high temperature acting on an unseasoned system. Is it possible that there could, under any circumstances, be a generation of the specific poison? or that from a neglected "ardent fever" a self-generating or reproducing virus could spring? Till this question be settled, we do not see how fallacies can be avoided, and a single example of its occurrence would be sufficient to overthrow much of the reasoning which has been adopted on both sides. For it is singular enough, that those who deny altogether the contagious nature of yellow fever, and those who admit a true contagious yellow fever or "hæmagastric pestilence," adopt on this occasion the same line of argument.[*] We observe, however, that on this point Dr. Copland expresses himself with great caution; thus speaking of the possibility of "remittent or periodic fevers changing their characters, so as to become the hæmagastric pestilence," he says, "the evidence bearing upon the question is insufficient to prove the negative, but most certainly we have no satisfactory proof of the affirmative. . . . I cannot deny the possibility of this conversion; the conversion may be possible, I, however, believe it to be very improbable." (Op. cit. 174-5.) In the next page he states that a "putro-adynamic fever" may by certain circumstances become infectious; but, it is added, "this is not the pestilence under consideration, and I cannot find any evidence that this pestilence has ever really originated in this source." (p. 176.) We are of opinion, however, that, in making this admission, Dr. Copland is not altogether consistent with himself, the whole course of his description, the whole force of his reasoning, is rendered doubtful by an admission of the possibility of such a conversion; if he is not certain on this point, he has been unnecessarily severe on those who believe in the production of a contagious virus from marsh yellow fever. Sir W. Pym is more consistent; he asserts unconditionally the specific nature of the hæmagastric pestilence, and declares "that it is in no way connected with malaria or unhealthy districts."[†]

In order therefore to examine into the question of the contagious nature of all the epidemics of yellow fever, we may, instead of burying ourselves in a mass of contradictory evidence, commence in another direction, and inquire into the possibility of this conversion. If a severe marsh fever can develop a contagious virus, then the alliance of the two diseases must be admitted, or, in other words, we should say that the yellow fever was both contagious and non-contagious.

But before entering upon this important point, it is necessary to consider the remaining causes, the diagnosis of yellow fever, and the possibility of distinguishing the different diseases which are said to have been included under that term.

II. *In addition to a Contagious Virus, which has been proved to be an active agent in the production of Yellow Fever in certain cases, are we to admit as causes the influence of external Heat and of Exhalations from the Soil?*

The opinions of almost all the best writers are so far agreed, that they admit the existence of severe fevers arising from the causes above specified. This uniformity of opinion renders it unnecessary for us to consider the evidence on this point, we shall for the present assume it as a fact.[‡] The interpretations put upon this fact are, however, singularly discordant.

1. Thus it has been supposed that each of these three alleged causes of yellow fever, viz. heat, marsh miasma, and a specific virus, contagious or not, produces a fever radically distinct from that resulting from either of

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[*] The term "hæmagastric pestilence" is used by Dr. Copland for the epidemic yellow fever. We have occasionally employed it in speaking of the views of Sir Wm. Pym or Dr. Copland, as not involving the assumption which the terms "Bulam fever," or contagious yellow fever, render necessary.

[†] Letter to the Lords of the Council, p. 5.

[‡] We assume this opinion at the present time, because it would lead us too far from our subject to debate the exact influence of heat alone in the production of yellow fever. We reserve, however, our own opinion, as we believe that Dickinson, Boott, and others have attached too much importance to this agent; the severe "ardent yellow fever," or "inflammatory endemic," or "seasoning fever of new-comers," is unknown in many countries much hotter than the West Indies.

the other causes; that these fevers so far agree in some of the leading symptoms as to be entitled to the common appellation of "yellow fever," but that when examined with proper care the characteristic differences cannot be overlooked. According to the supporters of this opinion, the discordant and contradictory evidence of the nature and treatment of these fevers arises from inaccurate observation, and from a confusion of the three diseases.

2. A second opinion admits the influence of two of the three causes, viz. heat and marsh miasmata, but considers the specific virus to be only a product of the resulting diseases, developed by peculiar and unprecedented circumstances.

3. A third opinion denies altogether the specific virus, either as originally distinct from marsh miasma, or as a product of this, and asserts that the yellow fever is produced solely by terrestrial exhalations in certain geographical limits. A modification of this opinion admits also the occasional production of a yellow fever from external heat, with or without the additional agency of marsh miasma.

A consideration of these several views brings us to our second question.

II.—CERTAIN AGENCIES BEING ADMITTED AS PRODUCTIVE OF SEVERE YELLOW FEVERS, ARE THESE FEVERS OF THE SAME OR OF A DISSIMILAR NATURE?

What do we mean by true or pestilential yellow fever, and what are the diagnostic symptoms which distinguish it from the diseases which resemble it, but which are said to be really dissimilar? Louis, in his work on the Yellow Fever of Gibraltar* in 1828, gives the following symptoms as sufficient to establish the diagnosis (pp. 287–8): Headache, red and suffused eyes, pains in the limbs and febrile symptoms; then nausea, vomiting after 12 or 15 hours, restlessness, yellowness of eyes and skin on the third and fourth days, blackish stools;—in fatal cases the vomiting continues, the vomited matters become more or less blackish, or completely black, the yellowness increases, the heat forsakes the limbs, and in 20 to 36 hours after the supervention of this state of things, and four or six days from that of attack, the patient dies.

"Should a patient then," continues Louis, "exhibit the symptoms which have just been enumerated, should he inhabit a country in which the yellow fever has been observed, or only a city, the latitude of which is comprised between the limits in which the yellow fever has been observed, we might be almost certain that he was attacked by that disease, for these are the symptoms which have been observed by us in a great majority of cases of yellow fever, and *they do not beloug to any other disease*, as we shall soon clearly see." (p. 288.)

And again he writes—

"The diagnosis can present no real difficulties during the life of the patients in the greater number of cases, on account of the rapid course of the disease, of the black vomit and dejection, of the yellowness, and extremely uncomfortable feelings." (Op. cit. p. 310.)

Were this true there would be no difficulty about the question; but, unfortunately, M. Louis' diagnosis is too limited; the symptoms of black vomit, yellow skin, and rapid course are common to the severe fevers arising from all the admitted causes of yellow fever. Of this we shall immediately proceed to cite evidence, but we may remark, that it is admitted by Dr. Copland, who may be considered as the most prominent supporter of the distinctive nature of one of the varieties of yellow fever, which he describes under the term of "hæmagastric pestilence."

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* Researches on the Yellow Fever of Gibraltar of 1828. By P. Ch. A. Louis; and translated by G. C. Shattuck, LL., M.D. Boston, 1839.

> "It will be observed," he says, "that sporadic cases of this distemper and the earliest of those occurring, when it assumes the form of a devastating pestilence, will often be recognised with great difficulty, and be liable to be mistaken for the more malignant cases of remittent fever, or even for the inflammatory seasoning fever. The difficulty is chiefly owing to the mildness of the symptoms in some instances, and to the circumstance of *yellowness of the skin, and vomiting of a dark brown or black fluid* being observed in many of those cases, as well as in the true hæmagastric disease."

Louis either overlooked the fact of the reputed existence of these fevers, or he considered them to be examples of true yellow fever, as he does not allude to any difficulty of diagnosis arising from this source, but proceeds simply to draw the distinction between yellow fever, on the one hand, and the fièvre typhoide and gastritis, on the other; although in uncomplicated cases there seems to us no possibility of confusion.

We may assert indeed unconditionally, that the significance of these symptoms of yellowness of the skin and black vomit is very small indeed as diagnostic marks. We find yellowness of different shades described, not only in the epidemics of yellow fever,[*] but by all writers on the fevers of hot countries, no matter whether they refer to the form arising, according to the common notion, from heat, or to that from marsh miasmata, or to that from a specific poison. Thus it is stated by the believers in the existence of a true yellow fever, or "hæmagastric pestilence," that Gillespie, in his 'Causus,' Mosely in his 'Endemial Causus,' Veitch, in his 'Ardent Continued Fever,' Dickinson, in his 'Inflammatory Endemic,' and a host of writers, under the various terms, "synocha," "seasoning fever," "inflammatory remittent fever," have included, as indeed many of these writers have expressly stated, a fever arising from the action of change of climate and a high range of temperature upon a plethoric and unseasoned habit. Yet we find all these writers describing yellowness of the skin as a prominent symptom, and many of them expressly state that this assumes various shades. Thus, Mr. Dickinson says,[†] "the yellow fever is constantly witnessed among new comers from temperate climates, when the most minute investigation is unable to detect either the agency of contagion or of febrific effluvia evolved from soil." "The skin," he says afterwards, "is often variously tinged from the pale colour of a lime to the deep tint of a ripe orange" (p. 29). The yellowness of the skin in remittent fevers, arising from malaria, has been noticed in all climates, although it is certainly most common in those of the western hemisphere. Cleghorn observed it at Minorca;[‡] Irvine,[§] in Sicily, in the autumnal fevers; Burnett,[||] in the Mediterranean fevers, of all depths of colour, &c. Sir G. Blane observes, that "there is no such symptom in the bad fevers of the East Indies (op. cit. p. 288); but this is erroneous. In the fatal fevers of the Mysore country, yellowness, during some years, has been almost universal.[*] Dr. Johnson describes it in Batavia. It occurred in the fever of Rangoon in 1824-5. A fever, attended with yellowness of the skin, raged like a pestilence in Rohilcund, from 1836 to 1840; at the same time that a fever, with symptoms of plague, was prevalent in Marwar and Meywar, and common remittents and intermittents prevailed between these districts.[†] A fatal remittent fever attacked H.M.'s 29th regiment in 1844 at Ghazepore; in many cases there was "deep jaundice," and in one case a symptom occurred which has been often witnessed in the West Indies, viz. sloughing of penis and scrotum. Mr. Christie noticed yellow skin in Ceylon.[‡] We shall have occasion presently to allude to the malignant fevers of the Straits of Malacca, and of the countries and islands northward of this settlement, in which yellowness of the skin is a usual accompaniment.

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[*] Dr. Gilchrest says: "Regarding yellowness, it is quite inconceivable how any writer laying claims to the smallest knowledge of this disease could have placed a very light or lemon yellow as the true diagnostic colour, for nothing is better known than that the skin may assume a very intense yellow." (Cyclopædia of Medicine, vol. ii, p. 273.)

[†] Observations on the Inflammatory Endemic of the West Indies. By Nodes Dickinson. London, 1819. p. 1.

[‡] On the Epidemical Diseases of Minorca. 4th edition. London, 1779.

[§] Observations upon Diseases as they occur in Sicily. By Wm. Irvine, M.D. London, 1810. p. 43.

[||] On the Mediterranean Fever. 2d edition. London, 1816. p. 425.

The occurrence of "black vomit" has been stated by several writers to be a distinctive feature of the yellow fever arising from a specific virus. Sir William Pym has lately asserted this unconditionally in his 'Letter to the Lords of the Council on the Fever at Boa Vista.' "This disease is unknown in the East Indies, in Egypt, or in Turkey, and is a native of and peculiar to the west coast of Africa; it is attended with the peculiar and fatal symptom of black vomit, a symptom which rarely if ever appears in the marsh or remittent fever." (p. 5.) We are somewhat surprised to meet with this assertion again, after it has been contradicted by so many observers. Even Dr. Copland, whose opinions of yellow fever are quite as defined as Sir William Pym's, admits the occurrence of black vomit in fevers obviously of marshy origin. In fact, there is incontestable evidence that in the latter stages, both of the seasoning fever or causes and of endemic marsh fevers, the black or coffeegrounds-like vomit is a common symptom. As an example of black vomit occurring in the latter stages of seasoning fever, we may refer to Mr. Dickinson. (Op. cit. pp. 124-40.) This writer also states that he has seen black vomiting in gastric affections of a purely tropical nature, in *coup de soleil*, in injuries of the brain, &c. The frequent occurrence of black vomit in African remittent fever is amply attested, were evidence really required, by the admirable 'Report on the Climates and Diseases of the African Station.' Thus, in the remittent fever of the Conflict (p. 96), the Waterwitch (p. 127), the Buzzard (p. 137), black vomit is noticed as a usual symptom. So also in the yellow fevers of America, which are incontestably of marshy origin, black vomit is a usual symptom. For abundant proofs of this we may refer to Dr. Boott's excellent account of the fevers of the southern and midland states of North America.

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[*] On the Seringapatam Fever. By Dr. Nicoll. Edin Med. and Surg. Journal, vol. xi, p. 280.

[†] See Mr. Ranken's admirable Report on the Pali Plague (Calcutta, 1838), where it is stated that this fever was indistinguishable from West India yellow fever.

The following description of the fever at Moradabad is given by Dr. Shirreff (Ranken's Report, p. 215): "Severe headache, great prostration, burning skin, tension and pain at epigastrium, tormenting thirst, high-coloured urine, at times tinged with bile, occasionally suppressed, yellow suffusion on third or fourth day, hemorrhage from nose and fauces without alleviation of symptoms. This disease appeared decidedly infectious, but the infectious property is stated to have been completely lost by ventilation. There were evident remissions. Other accounts mention dark stools and oozing of blood from the tongue."

Mr. Guthrie, writing from Bareilly, mentions the inflamed eyes, the sight being lost in a single night by disorganization of the cornea. Hiccup also was a common symptom; there were evident remissions. (Op. cit., p. 209.)

Severe dysentery and cutaneous abscesses were sequelæ of the fever. (Mr. Spencer. op. cit., pp. 201-4.)

The analogy of this fever to West Indian fever, except in the absence of black vomit, cannot be contested. It is singular, also, that several reporters state that quinine was hurtful, although the immense power of this remedy in the common remittent fevers of India has been fully demonstrated.

[‡] Sir James M'Grigor, op. cit., p. 165.

As an exception to the frequent occurrence of black vomit in severe remittent fever, we should notice its remarkable absence in the fever of the Niger expedition. This is noticed both by M'William and Pritchett.* There is also no doubt that black vomit is much more common in the epidemic than in the endemic fever; but this is equally in accordance with the opinion which supposes the former to be only the latter in an intense degree, and of course with symptoms of unusual severity.

But if the black vomit is not seen exclusively in the epidemic yellow fever, it is, on the other hand, undoubtedly often absent in cases of true hæmagastric pestilence. At Gibraltar, in 1814, black vomit was rarely seen. In an epidemic in 1827, in Jamaica, it was not witnessed in one regiment, although the fever was so severe that 140 men died in six weeks, and the surgeon considered it to be contagious; and in cases of recovery it is comparatively seldom seen, as we shall have occasion to notice more particularly afterwards.

Yellowness of the skin and black vomit being admitted as insufficient diagnostic marks, the supporters of the distinctive nature of hæmagastric pestilence adduce the following additional grounds of difference: 1. That this pestilence is of continued, not of remittent, type. 2. That it occurs only once during life. 3. That it is infectious. This latter point is the one under discussion, so that we shall postpone our decision upon it, and inquire into the other two.

Is the type of the epidemic or presumed contagious yellow fever always continued? There seems no doubt that in the most fatal forms remissions are not observed. In other cases, however, there is no doubt of their presence. Thus it can hardly be doubted that Rush was speaking of the true "hæmagastric pestilence," when he writes of the memorable fever of the year 1793.† "In every instance of this disorder which came under my notice, there were evident remissions or intermissions. The tertian type discovered itself in some people after the more violent symptoms of the fever had been subdued." Dr. Lining, of Charleston, noticed occasionally a quartan type. Dr. Pinckard states that a quotidian, a remittent, and a malignant yellow fever run indiscriminately into each other. Gillespie gives numerous instances of these transitions. "In 1796," he says, "the ardent or yellow fever reappeared; in the first instance, it was in the form of a double tertian, with remissions and even intermissions. It very soon changed, however, and took the character of the acute yellow fever, apparently communicated by infection, as the persons who attended on the sick were first attacked and successively all the crew." (Op. cit., p. 130.) The symptoms of this yellow fever are described in the next page; the chief are great prostration, black vomiting, black stools, yellow eyes and skin, pulse slow and at times intermittent. (p. 132.) He says the sick were landed, and then the black vomiting and purging were absolutely mortal symptoms; yet he adds, "the disease evidently partook of the tertian remittent type, as fevers of this kind both ushered in and closed the constitution; even in the middle of the constitution cases occurred in which remissions and intermissions took place." (Op. cit., p. 146.)

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* Medical History of the Expedition to the Niger. By J. O. M'William, M.D. London, 1843. p. 138. Some Account of the African Remittent Fever. By M. Pritchett, M.D. London. 1843. p. 162.

† An Account of the Bilious Remitting Yellow Fever in the City of Philadelphia in the year 1793. By Benjamin Rush, M.D. Edinburgh, 1796. p. 61.

The tertian type of this disease proves that Gillespie was not speaking of the seasoning fever, and it can scarcely be doubted, from the constant occurrence of black vomiting, and from the remark he makes of the infectious nature, that he had to deal with the true pestilence. This is corroborated by its prevalence during those years in the West Indies. Dr. John Hunter, whose description, in our opinion, can only apply to true yellow fever, mentions remissions of the quotidian type.[*] Jackson says (History and Cure of Fever, p. 62) "at Mirebalais the disease was frequently of one type, most frequently of the double tertian form, with bilious vomiting and purging; the shades of yellowness were various, from a slight tinge to that of a Seville orange, but though the more common form was remitting, there occurred on many occasions instances of the purer form of yellow fever, of rapid course, terminating with black vomiting, and hemorrhages from different parts of the body;" and he adds, that the study of this epidemic convinced him, contrary to a preconceived opinion, that the concentrated yellow fever was merely the common endemic fever in an aggravated form.[†] Sir James M'Grigor,[‡] who describes most accurately the yellow fever of Grenada of 1795, mentions the frequent occurrence of remissions; he says also, that some cases terminated in intermittents. In Spain, Dr. Arejula,[§] a great name among the contagionists, noticed both remissions and intermissions. Dr. Gilchrest observed them in Gibraltar in 1828, and quotes the following names "in support of the fact, that remissions not unfrequently take place in this disease in the Gibraltar yellow fever epidemics: Drs. M'Mullin and Brown, Messrs. Sproule, Wild, Martindale, Auriel, Daw, Donnett, Humphries, Lee, and Hugh Fraser."[‖]

We have selected these few instances from a much greater number, because there can be no cavil about the disease referred to being the hæmagastric pestilence. If, however, we were to take doubtful examples, that is, epidemics which possibly were connected with terrestrial exhalations, we should find a constant association of the symptoms of black vomit and yellow skin with remissions. In proof of this, we refer to that most laborious and admirable description of yellow fever by Dr. Boott, contained in the life of Dr. Armstrong.[¶]

It is, of course, possible to explain the occurrence of remissions in true hæmagastric pestilence, by supposing that the specific poison was acting at the same time with malaria. There is no doubt of the possibility of the union of two poisons in this way; it has been supported by Sir John Pringle and others, in the case of typhus and marsh fevers; bilious remittents have prevailed with smallpox in the West Indies, forming, as an old writer remarks, "the most infernal combination that ever affected the human frame."[*] In 1777 Closset observed the yellow fever associated with a putrid typhus.

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[*] On Diseases of Jamaica. By John Hunter, M.D. London, 1796.

[†] Sir W. Pym affirms that the above description is sufficient to prove that "there really existed two diseases" at Mirebalais, which Jackson confounded (On Bulam Fever, p. 150); but with this we find it difficult to agree. It is, in the first place, begging the question; and, secondly, it is surely an unfair aspersion on Jackson, who distinguished with such remarkable tact and minutiæ the several forms of tropical fevers, that he should have confounded diseases which, according to Sir W. Pym's own argument, are readily distinguished by an experienced eye.

[‡] Medical Sketches, &c. By Sir James M'Grigor. pp. 226-38.

[§] Brieve Descripcion de la Fiebre Amarilla de Cadiz, quoted by Gilchrest.

[‖] Cyclopædia of Medicine, vol. ii, p. 965.

[¶] Life of John Armstrong, M.D.: with an Inquiry into Facts connected with Marsh Fever. By R. Boott, M.D. London, 1833.

What, however, is the real value of remissions or intermissions as denoting variations in cause? We suspect very little. Most writers allow the plague to be a continued disease, yet remissions have been noticed by Russel, Sir James M'Grigor, and Rush. Intermissions and irregular remissions are noticed by Diemerbroeck, Bertrand, Veney, &c. &c.[†] The malarious origin of plague has been even argued from this circumstance, an error arising, it appears to us, from an over-estimation of this symptom. Our limits, however, will not allow us to enter into this question, we shall conclude, first, that remissions are not unknown in epidemic yellow fever or the so-called hæmagastric pestilence; and, secondly, that they are symptoms of little diagnostic value.

The non-recurrence of yellow fever in the same person is an old opinion which has lately been revived. Dr. Lining, of Charleston, in 1748, noticed that those who had formerly had the disease escaped. Dr. Clarke says "those who recovered from this fever were never attacked a second time."[‡] Sauvage asserted this positively, in the 'Methodical Nosology.' Sir W. Pym has lately, in his 'Letter to the Lords of the Council' (pp. 9-16), reproduced the evidence he conceives decisive of the question. Thus, in the epidemic at Gibraltar in 1804, he states that he took the nominal list of the officers of the Royal Engineers, the Royal Artillery, and the 2d, 10th, 13th, and 54th regiments, and found that every officer had been attacked except those who had passed the disease in the West Indies. In his work on the 'Bulam Fever,' he is less explicit on this point, merely saying that he "requested a parade of all the officers and men who had been in the West Indies, at which, to my astonishment and gratification, I found 122; many of them had been exposed to the contagion in a variety of ways, but all escaping it; not one of them had had even the slightest headache."[§] It is an extraordinary fact, that he does not state that these 122 men had had the disease previously, but merely that they had been in the West Indies. It is obvious that the effects of climate and of seasoning might be active here, and in the next page but one, we actually find Sir W. Pym referring to the effects of residence in warm climates as giving partial immunity. Thus, he says—

> "When the disease first made its appearance at Grenada, Dr. Chisholm particularly noticed its attacking constitutions of natives, or of Europeans assimilated to a warm climate, in a comparatively mild form. He mentions that of 56 men belonging to the ordnance department (who had been three years in the West

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[*] Practical Remarks on West Indian Diseases. London, 1776. p. 124.

[†] In the malignant fever with glandular swellings, which prevailed in Kutch and Kuttywar in 1815 and 1819, there were remissions. (Bombay Reports, vol. i, p. 337.) So also in the "Pali Plague" there were remissions in the slighter forms. (See Ranken's Report; also Dr. Forbes on the Nature and History of Plague in India, p. 7. Edinburgh, 1840.)

[‡] A Treatise on the Yellow Fever in the Island of Dominica in 1793 4-5-6. By James Clark, M.D., F.R.S.E. London, 1797. p. 19.

[§] On the Bulam Fever. By Wm. Pym, Esq. London, 1815. p. 25.

Indies) attacked with the disease, only five died, and that about the same time 26 recruits lately arrived from England were attacked with the fever, of which number 21 died."

The next paragraph is a very important one. In the Letter to the Lords of the Council, the following remark is make of the 10th Regiment: "In the 10th regiment, every officer was attacked except Captain Carpenter, who had had it in the West Indies." (p. 10.) In the work on the 'Bulam Fever,' Sir William continues, after the paragraph we have last cited, in the following words:

> "Of this last peculiarity of the disease we had at Gibraltar a most convincing proof in the 10th regiment of infantry, which had been quartered several years in the *East Indies;* eight officers (who had been in India) belonging to this regiment were attacked with the fever, and all recovered. Seven officers, who had not been in India, *had the disease in so different a form* that five of them died. Four hundred of the men who had been in India were attacked with the disease, of which number only *four* died (!); and of forty-eight who had not been in India, sixteen died. This circumstance is so strong and of such consequence as, in my opinion, to merit the consideration of his Majesty's government as to the propriety of trying the experiment of sending troops from the East to the West Indies, after having been seasoned to the climate by a residence of about three or four years: they will, by this, I am confident, be rendered proof against a violent attack of Bulam fever."[*] (Op. cit. p. 30.)

The fact which Sir W. Pym has insisted upon, that residence in warm climates in either hemisphere lessens the susceptibility to yellow fever, has been noticed by all practical observers. But, if true, what becomes of the evidence of the protective influence conferred by former attacks? It is of course liable, from this cause alone, to endless fallacies. An attack of yellow fever is not the only cause of safety, and even if we could rely on the mere statement of the 122 officers and men that they had suffered from true yellow fever, supposing they had made it, which they do not appear to have done, still we should be uncertain whether they owed their exemption to this fever or to mere acclimatization. Sir James Fellowes, in speaking of second attacks in the epidemic of Cadiz in 1804, states that those who had had the disease in 1800 were exempt; the same remark was made of "those who had been ill with it in South America, as well as of the natives of that country who had resided there many years *without ever having had the fever, or any disease like it.*"[†]

The next argument Sir W. Pym adduces in the Letter below referred to, is the inquiry which, at his suggestion, the Anglo-French Commission made at Gibraltar in 1828, and of which MM. Louis, Trousseau, and Chervin were members. The commission called upon 33 medical men to state how many cases of yellow fever they had witnessed in all epidemics in Europe and America, and how many cases of presumed second attacks. The number of observed cases was 27,000, the number of presumed second attacks 13; of these 13, one case of double attack was declared evident by the majority of the board; three cases were declared probable; one case had an equal number of votes for its being probable, and for its being doubtful; eight were declared inadmissible. Of the 27,000 patients, the board supposed that 9000 had been possibly exposed to the influence of two epidemics, and if the whole number of presumed second attacks are admitted, still the proportion is considerably under the average number of second attacks in smallpox, which, from a communication to the Royal Academy of Medicine of Marseilles in 1825, is supposed to be about one per cent.

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[*] We may remark here incidentally that, as the best observers uniformly agree that in the epidemic disease few recover who have the black vomit, Sir W. Pym, in assigning so small a mortality as one per cent. in the case of the 10th regiment, must have diagnosed the disease from other symptoms. We may also fairly conclude that an account of the epidemic, compiled from these 400 cases, would have given the black vomit as one of the rarest symptoms.

[†] Reports of the Pestilential Disorder of Andalusia. By Sir J. Fellowes. London, 1815.

Louis, whose philosophic caution is so well known and highly estimated, thought this investigation so conclusive, that he does not hesitate to say, "From the facts already given, the reader can scarcely have a doubt of the preservative influence of a first attack of yellow fever" (Op. cit. p. 319); and again, "these facts, though few, are conclusive, and in this point of view Sir W. Pym has rendered a great service to science and humanity," (p. 322.) Louis also remarks that the slightest as well as the severest attacks bestow equal immunity, and that the preservative influence is not destroyed by time, as those who had suffered in 1804 were still protected by it in 1828.

There cannot be a dissenting opinion as to the importance of this evidence, although we are disposed to think that the number of second attacks will be found hereafter to be above the allowance of the French commission. We may remark, however, that two of the commission, Chervin and Fraser, dissented from the Report. And it would not be difficult to object to the mode of investigation pursued on the occasion, if we were not determined to abstain from controversy as much as possible. It is a remarkable fact, too, that Dr. Gilchrest, who was in Gibraltar at the time, and who acted as interpreter for the French Commission, takes little notice of the Board, or its investigations, and indeed passes over without comment the subject of the preservative influence of first attacks of yellow fever.

As opposed to the doctrine of the non-recurrence of true yellow fever, we may make the following observations:

1. There is evidence on the other side of considerable weight. Thus, in 1816, Drs. Gray and Tate, physicians to the fleet, examined the surgeons' journals of several of the ships serving in the West Indies, and found 100 instances of second attacks which they considered authenticated. They do not state that any of these individuals had had black vomit, but they argue that, as in many of the fatal cases this symptom was present, "there is a fair inference that, although the symptoms of many of those who had suffered from a second attack might have been of a mild nature, yet the disease was the same with that generally denominated Bulam, or ardent yellow fever.* The opinions of 24 chief naval medical officers were also called for by the government; of these, 11 declared the doctrine of non-recurrence to be totally erroneous; 2 remained doubtful; and 11 abstained from any opinion, because they had not had sufficient experience of the disease, (See also a Sequel to an Essay on Yellow Fever, by E. N. Bancroft, M. D. London, 1817, p. 51.)

The late deputy-inspector Ferguson expressed himself on this point with his characteristic decision: "Another piece of doctrine has been propagated from the writings of the authors above alluded to (Pym and Fellowes), that the yellow fever cannot be received by the same subject more than once. We, again, who live among yellow fever, not only know nothing about this, but are used to see it contradicted by the daily experience of our lives."

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* Correspondence, p. 7.

Sir W. Burnett also, in his work on the Mediterranean Fever, gives lists of "50 authenticated instances of second attacks" in the Gibraltar epidemic of 1814. Sir James M'Grigor states (Medical Sketches) that in 1796, on board the Betsy transport, "almost every person was attacked once, and a great many twice with this fever." And it must not be argued that these were relapses, as Sir W. Pym considers relapses almost as unfrequent as second attacks. Dr. Curry* states that he "has seen several instances of this fever occurring a second time under unequivocal circumstances." Dr. Rush, in 1797, attended six persons whom he had attended in previous epidemics. Dr. Pinckard, who is generally considered to have seen the hæmagastric pestilence, says it may attack the same person many times. But we need not multiply quotations.

It may be objected to this positive evidence, that the observers confounded the "remittent" with the "contagious" yellow fever. To this it is difficult to reply; but if the accusation be possibly or even probably true, it leaves the question in as great uncertainty as ever, as no one with the above statements before him could consider the evidence on the contrary side conclusive, until these statements had been refuted by testimony of undeniable accuracy.

2. Admitting that all these presumed cases of second attacks are falsified by the mistake above referred to, it is perfectly evident that the argument may be turned the other way, and it may be questioned how many of the supposed preservative first attacks were really cases of true yellow fever, and not of remittent, or seasoning fever; and when it is remembered how difficult the diagnosis confessedly is in many cases, and how constantly even medical men have, according to the believers of a specific contagion, confounded these several diseases, it must be conceded that the assertion of a non-professional person, that he has had true yellow fever a certain number of years before, must be received with great limitation. But if the bare assertion of an individual is to be received as evidence, it would not be difficult to bring forward counter-assertions of the same kind. Thus we have ourselves questioned the officers of a regiment which had served 11 years in Jamaica, and had suffered two decided and undoubted attacks of epidemic yellow fever, as well as lost men every year from sporadic attacks, yet there were two or three of these officers who stated that they had had the yellow fever two and three times, and one officer had had it seven times; when questioned more minutely, this gentleman stated that his disease was considered by every one true "yellow Jack." We concede at once that this gentleman was mistaken; we entertain little doubt of this, because a first attack of yellow fever does certainly afford some protection from a second; but we contend that if the loose statements of non-professional persons are to be received at all, one side is entitled to as much credit as the other.

There is no unequivocal symptom of yellow fever which, like the eruption of smallpox, can render the diagnosis certain; even black vomit, which Sir Wm. Pym regards as pathognomonic, is seldom witnessed in those who recover from epidemic yellow fever. This is broadly affirmed by Dr. Copland:

"Of the cases which I had an opportunity of treating many years ago, only

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* On the Synochus Icteroides of Philadelphia, p. 15. Philadelphia, 1794.

> one recovered after black vomit had fully and unequivocally manifested itself. It should, however, be remarked, that vomiting of a dark grumous fluid, occurring with or after yellowness of the skin, not unfrequently occurs in the last stage of malignant bilious or remittent fevers, and that recovery occasionally takes place in that fever even after these symptoms have appeared. But the case is very different in the true hæmagastric pestilence. Most of the cases of recovery which we hear of from the black vomit are recoveries from these states of yellow or remittent fever, which have been confounded with this pestilence." (Op. cit. p. 149.)

Now we put it to any one, whether, when the great characteristic symptom assigned to the disease by Sir W. Pym is absent in so many cases of recovery, the proofs of a first attack are possessed of that definite and unequivocal character which we have a right to demand in an important inquiry like the present?

3. But, in spite of the deficiency and looseness of the arguments commonly urged, we have already stated that, judging from the evidence adduced at Gibraltar, and by other observers in various quarters, a first attack of yellow fever does give a certain degree of immunity from a second. But, then, what is the extent of this immunity, and what is its value as a diagnostic mark? Questions by no means easy to answer, because we find, as already stated, that residence in a hot climate, even when this climate is not subject to yellow fever, also renders the constitution in some degree insusceptible of the action of the poison.[*] And it is a remarkable fact, attested by Humboldt, that this insusceptibility is diminished by change of residence. This point is so important that we shall quote his own words:

> "Individuals born and brought up at Vera Cruz are not subject to the vomito, and it is the same with the inhabitants of the Havannah who do not quit their country; but it happens that merchants born in the island of Cuba, and who have inhabited it for a great number of years, are attacked with the vomito when their affairs call them to Vera Cruz during the months of August or September, when the epidemic is raging. In the same manner Spanish Mexicans, natives of Vera Cruz, have been seen to fall victim to the vomito at the Havannah, Jamaica, or the United States. Notwithstanding the analogy which the climate of Vera Cruz bears to that of the island of Cuba, the inhabitant of the Mexican coast, insensible to the miasmata of the air of his native country, falls under the exciting and pathogenetical causes which act on him at Jamaica and the Havannah. It is probable that, under the same parallel, the gaseous emanations which produce the same diseases are almost the same; but that a slight difference is sufficient to throw disorder into the vital functions, and to determine that particular succession of phenomena by which the yellow fever is characterized."[†]

Afterwards Humboldt qualifies his position by saying, that it is only "sometimes", that the seasoned inhabitant of another locality runs the same risk as persons unseasoned altogether. "I say sometimes, for in general, the examples are as rare of persons born in the West Indies being attacked with the yellow fever at Vera Cruz, the United States, or Cadiz, as of Negroes falling victims to this disease." It is obvious that Humboldt's observation tends to prove that, while the resident of a yellow-fever district is nearly secure from the vomito while he remains in that district, his system becomes susceptible when he moves to another district, but not nearly to the same extent as the new comer from colder climates.

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[*] Jackson says: "It has been long supposed, and it has lately been pretended to be proved, that persons who have sustained one attack of yellow fever are exempted from all apprehensions of a second. The exemption contended for, I think I am warranted to say, is not absolute; but I willingly admit what I know to be true, viz. that there is something in residence and in changes produced by concentrated fever which renders the habit comparatively less susceptible of the form of disease which terminates by black vomiting, and which in pre-eminence is called yellow fever." (Op. cit. vol. i, p. 43.)

[†] Political Essay on the Kingdom of New Spain, vol. iv. London, 1822.

4. It is a singular circumstance that the "inflammatory endemic," or "inflammatory seasoning fever" of the West Indies, the "causus" or "ardent fever" of some writers, a disease which is maintained by Dr. Copland, and those who agree in his opinions, to be altogether a distinct disease from true yellow fever, also possesses the power of securing the constitution from a second attack. Mr. Dickinson, who has most carefully described this form of disease, states this positively: "The inflammatory endemic only attacks once, unless the inflammatory diathesis be reacquired from change of climate."* And Dr. Copland has obviously observed the same fact, for he remarks that "even ardent fever will occur a second time, if the person who has once been affected by it has returned to Europe, resided long in it, and afterwards gone to a warm country."† The ardent fever is then supposed to exhaust the susceptibility of the constitution, and yet no one has assumed that there was in its cause anything specific or peculiar.

But without enlarging on this topic we may sum up our argument in the two following propositions:

1. That it is probable the yellow fever does give a certain immunity from future attacks, but that the degree is not determined. And we object strongly to much of the evidence which has been adduced in proof of this immunity, as in the highest degree loose and inaccurate.

2. That, even admitting the exhaustion of constitutional susceptibility from an attack of yellow fever, we are by no means certain how far this points to a specific and peculiar disease, as we find the same exhaustion or abolition of susceptibility produced by the gradual influence of climate; moreover, we find strong reasons for believing this susceptibility to be restored by change of climate. It appears, also, that ardent or seasoning fever, which, according to the supporters of a peculiar contagious yellow fever, is a totally different disease, is also non-recurrent, although it has nothing specific in its nature.

We cannot then rely upon this presumed character any more than upon the yellowness, black vomit, or continued type, as proving the existence of a formal and specific yellow fever.

Although, when the symptoms are separately examined in this way, the contagious yellow fever, supposing it to exist, is found to possess no peculiar diagnostic and pathognomonic mark, it does not follow that it may not, in the general assemblage and collocation of its symptoms, present peculiarities which may be easily discernible by an experienced eye. This, indeed, is affirmed by some observers who have seen it, as Dr. Copland and Sir William Pym, and it is supported by the fact that several writers have stated that, although accustomed to the endemic fever, they at once recognised a distinct disease in the epidemic fever.‡ This has been strenuously denied by writers on the opposite side, and it is somewhat singular, if the distinction be so simple, that men of very great consideration should have fallen into such errors of diagnosis as have been attributed to them. Thus Dr. Rush, who probably saw more yellow fever than all the American physicians put together, is said by Dr. Copland to "have confounded bilious, remittent, and pestilential fevers with each other, and most illogically to have contended that what is true of the first and second is also true of the third." (Op. cit. vol. ii, p. 187.) And, according to Dr. Copland, Chervin, who traversed the world in pursuit of yellow fever, and witnessed it in thirty-seven different degrees of latitude, did not diagnose these diseases said to be so easily distinguishable. "This zealous apostle of non-infection* confounds remittent fever with this pestilence, and either suppresses or does not admit the established fact of protection caused by a former attack. Hence most of his pleadings are baseless and irrelevant." (Op. cit. p. 188.) Moreover, Chisholm, the great advocate for the specific contagious disease, found it no such easy matter to distinguish it from the remittent fever—at any rate, in description; for he says, in his letter to Haygarth, "It is a disease defined with difficulty, and is distinguished by shades, which require the industry, the discernment, and the fidelity of a Claude Lorraine to delineate."

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* Op. cit. p. 68.

† Op. cit. art. Fever Remittent, p. 951.

‡ For example, Dr. Barry, in the Epidemic of Sierra Leone in 1829. See Boyle's Account of the Western Coast of Africa, p. 270. Boyle himself says that the epidemic of 1829 resembled the endemic fever in an aggravated form. (p. 202.) Also Sir J. Gilpin.

It is to be remembered also, that very considerable differences in type do not always disprove the existence of a single and common cause. We should have some difficulty in believing that the mild scarlatina simplex had any alliance with the malignant sore throat with little or no eruption, if repeated observation had not demonstrated the transition stages. What more unlike than a mild quotidian and the pernicious intermittent which kills in a single paroxysm; or a slight case of true cholera and the fatal and uncontrollable disease which devastates India! And if any one had seen only the milder forms of the remittent fever, and had had no opportunity of tracing up the several grades, he might well believe, when he saw suddenly the severest variety, that he had before him a distinct affection. A cause is not unalterable; a morbid poison has not always the same concentration; it is ever varying in its intensity, and in the action it exerts on the system, more or less susceptible of it. And this has long been insisted on by writers on the endemic fevers of the tropics. No one doubts, or rather, we should say, that no one ought to doubt, considering the evidence on the subject, that the cause of all intermittent and remittent fevers is the same, although modified by an infinity of circumstances. And yet, in warm climates, the resulting diseases appear at first sight various and changeable in the highest degree. As an example of this, let us recall the observations of Jackson on the fevers of the West Indies.

> "The diseases which came under notice," says he, "were such as are called endemic. The symptoms, as viewed superficially, were different in form and degree; so different in many cases, as if the cause were essentially and radically different. The fluctuating variety of appearances was a cause of embarrassment to such an extent, as at one time to make an impression that the attainment of knowledge in the medical art was almost a hopeless expectation. The cases which were taken down in the manner stated amounted in a short time to a considerable number; they were exceedingly unlike in superficial appearance, but when carefully analysed, they produced, when compared and closely considered in all their relations with each other, such marks of radical resemblance as clearly proved identity. This discovery threw a ray of light upon the author's view. It animated his pursuit, and served in some degree as a guide to his future researches."†

Our readers will remember the care with which Jackson traced the variations produced by temperament and habit of body in the action of the cause of fevers, and how, after witnessing the different forms on an extended scale, he retracted the opinion he had formed in early life of the distinct nature of the yellow fever, and declared that it was possible by connecting links to prove it to be only a variety or an intense degree of the endemic remittent fever. It is also a fact that the vast majority of army surgeons who have witnessed all the forms of endemic and epidemic remittent yellow fever have accorded with this opinion of Jackson.*

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* It should be non-contagion; for Chervin, to use his own language, was an "infectionist."

† A Sketch of Febrile Diseases, &c., &c. By Robert Jackson, M.D. Second Edition. London, 1820. Introduction, p. viii.

On the other hand, we find, as we might have anticipated, instances of considerable variation of symptoms in undoubted hæmagastric pestilence; modifications are even presented during the same epidemic, and are well described by Dr. Copland. (Op. cit. p. 138-9.) The French Commissioners (François, Pariset, and Bally) thus expressed themselves regarding the Andalusian epidemic: "The Barcelona fever is the yellow fever of America —the same that we have in the Antilles and at Cadiz;" but they add, "it is a Proteus clothed with different forms, and which offers such strange anomalies, both in the slowness and the rapidity of its march, in the combination, the succession, and the degree of its phenomena, that it is impossible to assign to it a fixed and invariable course."†

Our space will not permit an extension of this analysis, but we think we are justified in concluding that the presumed "hæmagastric pestilence" has no pathognomonic signs, and presents no peculiarities which may not be explained by supposing it to be the result of the most concentrated action of a particular cause. In other words, *the severe fevers resulting from all the admitted causes are not distinguishable by symptoms from each other.*

But we have already proved that in certain cases yellow fever has decidedly manifested a contagious property. It is admitted that endemic or malarious yellow fever has not this property. Now, if it could be made certain that the concentrated and pestilential form of yellow fever always manifested a contagious character, this would be a marked distinction between it and the endemic fever, and we could then have no hesitation in admitting its specific nature.

Previous, however, to the discussion on this question, a preliminary inquiry is necessary, as to the possibility of the development of a contagious from a non-contagious virus. And, as we before remarked, it is singular enough that the argument of those who adopt without reservation the doctrine of a specific contagious "vomito," and of those who deny altogether this opinion, assumes here the same course, and arrives at an identical conclusion.—This inquiry, however, we must defer to our next Number.

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* There are few men whose powers of observation have surpassed those of Jackson. Not only did he notice fully the variations in fevers arising from a common cause, but he remarked the important fact that a malarious disease "is subject to changes at shorter or longer, but unascertained intervals, without any visible material change in the qualities of the locality or climate." And in illustration he states, from personal experience, that intermittent fever did not once occur at Savanna-la-Mar, in Jamaica, from 1774 to 1776, while afterwards it became very prevalent. During the same period tetanus was so frequent, that every wound, scratch, or accident was viewed with apprehension. Afterwards this became very rare, according to his belief, but he does not speak with certainty on this point. In this occasional absence of a malarious disease for a term of years, a resemblance is perceived with the absence of epidemics for a longer or shorter period.

† Histoire Médicale de la Fièvre Jaune observée en Espagne, &c. dans l'année 1821. Paris, 1823.

ART. IV.

Guy's Hospital Reports. Second Series. Vol. V. With Eight Plates.—*London*, 1847. 8vo, pp. 212.

I. *On some disorders of the nervous system associated with pregnancy and parturition*; by J. C. W. Lever, M. D.

Our knowledge of the nervous system and its functions has been immensely extended during the last few years; and yet, as regards the higher arcana of the subject, we can scarcely be said to have passed the threshold. Every practitioner, of any experience, has frequently met with cases that baffle all his skill, and force upon him the unpleasant consciousness of profound ignorance. The paper before us contains several striking illustrations of this fact. It is easy enough to affirm, that the singular manifestations related are the result of *sympathy* with the peculiar condition of the gravid uterus,—and such is unquestionably the case; but to imagine that this affirmation is in the smallest degree explanatory, is a mere self-delusion. We give a name, but little more.

Dr. Lever does not enter upon the wide field presented to our investigations by hysteria, puerperal convulsions, and puerperal mania; his observations are limited to a few disorders, but these sufficiently interesting.

We have first, five cases of *chorea*, coexisting with, and apparently dependent upon, pregnancy. Of these the following is a fair example:

"Mrs. —— was married at the age of nineteen; was short in stature, of fair complexion, but with dark hair and eyes. She had enjoyed good health; her uterine functions had been performed with regularity and without pain. She was of a cheerful disposition: she had a liveliness of manner and a warm-heartedness which made her a general favorite. She was wooed and married within twelve months, and during that period suffered only once or twice from hysteric attacks, which seemed to be produced by some fancied or real impediment to the consummation of her marriage. As might have been expected, conception took place 'ere the torch of Hymen was extinguished.' For the first two months the symptoms of pregnancy presented no special peculiarity; they were chiefly mechanical, with gastric irritability; but at the commencement of the third month a perceptible alteration took place in this lady's manner; she became irritable and pettish: then convulsive movements were observed about the muscles of the face, and followed in a week by a tossing of the head to and fro. The right arm then became convulsed, then the left, and afterwards, successively, the left and right leg. During the progress of her case, her mode of speech became altered, her sentences were short, she hesitated before giving a reply to a question, and when an answer was obtained, she seemed to shoot it out. The tongue was clean; her bowels acted with regularity, and the range of her pulse did not exceed the natural standard. She continued in the same condition until the close of gestation, when her memory seemed weakened, and fears were aroused lest she should become imbecile. Almost every plan of treatment was put into practice without avail; purgatives were given in true Hamiltonian doses; zinc was administered in large quantities; the various preparations of iron were tried one after the other; arsenic, digitalis, colchicum, nux vomica, bark, quinine, musk, ammonia, and, lastly, the shower-bath: Electricity was talked of, but not used, lest it might excite premature uterine action. The question of inducing premature labour was also entertained, and having been duly considered, was discarded, from the belief that there were no pressing symptoms to call for its performance, and that the symptoms entirely depended upon the gravid state of the uterus, and would depart at the termination of pregnancy. At the proper period labour commenced, and the patient was delivered of a living girl after a tolerably easy and natural labour. When the uterine pains were present the convulsive movements ceased, but in the intervals they were most distressing. Delivery was succeeded by a long and quiet sleep. The patient was more quiet on waking, the symptoms gradually subsided, and at the end of the month she went to church without a vestige of chorea. The supply of milk was copious, and in seven months she weaned her child."

Soon after this she again became pregnant.

"Symptoms of chorea again manifested themselves, but at an earlier period. They pursued a course very similar to that displayed in her previous pregnancy; but about the fourth month an accidental fright induced an attack of hemorrhage from the uterus; this was followed by its premature evacuation. Although the large quantity of blood she lost kept her in a state of weakness for a long time, yet as soon as the gravidity of the uterus was put an end to, the chorea declined, and the patient gradually recovered her strength. She has not since been pregnant, neither has she suffered from chorea. She has good health, her spirits are buoyant, she is full of life and merriment, and contributes most materially to the happiness of her friends. She has had occasional hysterical feelings, though slight in character and evanescent." (pp. 3-5.)

In these cases the spinal system of nerves appears to be affected secondarily through the ganglionic. The suffering entailed by such an abnormal condition is great, but the appearance presented by them is not so formidable as in some other instances. Our author narrates a case of almost complete amaurosis, and another of deafness, in both of which the diseased states resisted all treatment until the period of pregnancy had terminated, and then passed away; the deafness returning for a time under the exhausting influences of lactation, but being perfectly removed when the child was weaned, and the general health restored by change of air, tonics, &c. And here we have an exemplification of the singular fact, that the same cause will, in different individuals, produce effects of a very opposite character; exalting nervous irritability in one, depressing or almost annihilating it in another. And this paralysis may be manifested on a much more extensive scale than in the cases noted above, the power of speech and of motion being greatly impaired, and the mental manifestations most strangely altered. We quote the following, which is, in some respects, one of the most remarkable we have ever met with:

"Case 12. On March 3d, 1845, with Mr. Hawkins, I saw Mrs. P——, 18½ years old, and married seven months. She had not been regular since her marriage. She was of a delicate constitution; had twice, in the eighteen months previous to her marriage, suffered from menstrual irregularity, which was soon relieved by medical treatment. She suffered but little previous to quickening, nausea and sickness being the chief ailments. Some family disputes taking place, she suddenly swooned, and continued for some time insensible. When restored it was found that she was hemiplegic on the right side as far as the upper part of the abdomen; no pinching, tickling, or any other irritation caused any movement in the right lower extremity. On the following day she swooned again, and this was followed by an apparent loss of motion and sensation in the right upper extremity. Matters continued the same for three days, when the limbs suddenly regained their usual power. Speechlessness now ensued, which also lasted for three days, and was followed by loss of power in the right lower extremity as far as the knee. Such was the character of the symptoms previously to my visit. Naturally a mild, sensitive young woman, about two o'clock in the day she became morose, self-willed, contradictory, and obstinate, refusing to answer questions, or if she ventured an answer, it was generally in the negative monosyllable, 'No,' or the equally emphatic, 'I shan't.' Her appetite was capricious, herrings and ale being her favorite diet. This strange alteration of disposition and habits occurred daily about the hour of two p.m., and was preceded by a sensation of giddiness in the head; it continued till night. She then went to sleep, and about five o'clock a.m. awoke, complaining of severe and throbbing pain in the back part of the head. When visited by me she was in bed, free from flushing or inordinate heat; the eyebrows were contracted, the pupils natural and obedient to light. She would not permit me to see her tongue. The mobility of the upper and lower extremities was perfect; the surface of the body warm and bedewed with moisture; abdomen free from tympanitic distension; fœtal movements turbulent; mammæ plump and full, and from the nipples there was dripping a copious lacteal secretion; the head was free from inordinate heat, but the temperature of its posterior part was higher than that of the anterior; the hair was long and thick. Her bowels were freely opened by purgatives. The treatment by Mr. Hawkins had consisted in local depletions, counter-irritation at the nape of the neck, the administration of sedatives and antispasmodics.

"I was consulted more especially as to the propriety of inducing premature labour; and in reply I advised, that as there appeared to be no danger to life, as the attacks were periodic, and as there was no apparent probability of ultimate mental aberration, premature labour should not be induced; at the same time I expressed my belief that matters would continue much the same during the persistence of pregnancy. I further advised, that if it should hereafter be deemed necessary prematurely to empty the uterus, it should be by opening the membranous bag, and not by the administration of ergot. Secondly, I advised that the patient's head should be shaved, especially the posterior part; thirdly, that all stimuli should be avoided, more particularly sexual intercourse; fourthly, that the utmost quietude should be enjoined, perfect freedom from the visits of strangers, she having the night before mistaken a friend of her father's for the 'headsman.' Lastly, the following mixture was prescribed: Mag. sulph., ʒiij; Pulv. valer., ʒss; Tinct. lupuli, ʒiss; Syr. aur., ʒij; Inf. calumbæ, q. s. Ft. mist. ℥vj. sumat tertiam partem ter quotidiè.

"The symptoms improved, but continued until the beginning of April, when she was delivered of a small, living, but apparently healthy boy. The labour was perfectly natural; no convulsive symptoms occurred, neither was there anything to excite the slightest uneasiness. She continued doing well; but after a few weeks appeared unequal to nurse her infant, which was accordingly weaned when between three and four months old. She then suffered from various anomalous symptoms, irregular menstruation, occasional hæmoptysis, &c. The child died at the age of six months from muco-enteritis.

"I have found on inquiry that this young woman has been a second time confined, and at the eighth month. Once during her pregnancy she suffered from speechlessness for a fortnight, induced by some domestic feud. Her labour was natural. On attempting to rise, a fortnight after delivery, she found she had lost all power over her lower extremities, but this she has gradually regained, and about ten days since walked out for the first time to my house, a distance of about 150 yards." (pp. 20-2.)

The feature of peculiar interest in this case is the *periodic mental change;* the miserable perversion of temper recurring with such strange regularity. We have ourselves observed a somewhat analogous tendency to the periodic recurrence of certain aberrations; but in the case to which we refer, the morbid condition was not associated with pregnancy. It is a curious, and, to us, inexplicable psycholo[illegible]al phenomenon. What is the state of the brain under these conditions? The exalted temperature of the posterior part of the cranium would appear to indicate some degree of congestion, and we have certainly found local depletions strikingly useful in clearing up the mental vision and calming troubled thoughts.

In another case related by Dr. Lever, the thinking powers appeared to be altogether crushed. "The change that had taken place in this girl," he says, "was remarkable. From being light-hearted and gay, she sat wherever she was placed, neither turning her head nor her eyes to one side or the other; she was a living automaton, her movements were automatic, there was life, it is true, but there appeared to be no mind; pale and exsanguine, her chiselled face seemed as if cut in alabaster." These symptoms persisted until delivery, after which the patient perfectly recovered.

Our readers will see that this is a most interesting paper; it well deserves an attentive perusal.

II. *Ophthalmic cases;* by John F. France.

These cases are of a somewhat miscellaneous character, but they illustrate some points of considerable interest. The first two which we shall select bear, among other things, upon the question of the cause of photophobia.

"Case 3. Elizabeth M., aged 48, became an out-patient at the Eye Infirmary, January 12, 1846. From her account, it appeared that twelve years previously she had been attacked with partial hemiplegia, which was of temporary duration, but accompanied with diplopia and impaired sensation of the right side of the face, and right conjunctiva. This incomplete anæsthesia was ushered in with a feeling of coldness of the affected surface, as if constantly exposed to a chilling draught, and had remained to the period of her application at the hospital. Vision had been good with both eyes, but the right was troubled with muscæ, and was the less perfect of the two, having a small speck on the cornea. She had been subject to so-called gout on the affected side only. Ten days prior to presenting herself at Guy's, on awaking from sleep, she found the right eye incapable of distinguishing objects, and affected with luminous spectra; these symptoms continued for a couple of hours, when the ordinary state of vision was re-established, but the globe was then observed to be bloodshot and filmy.

"Jan. 12. The patient complained of great drowsiness, but was free from headache, vertigo, or diplopia; the right cheek and conjunctiva were benumbed; but the forehead, the interior of the cheek, the tongue and teeth preserved their sensibility, and the muscles of the jaw and face were unaffected. The conjunctiva of the right eye was morbidly injected, apparently from common rheumatic inflammation; the cornea was extensively but superficially ulcerated, and deeply nebulous; and vision was of course much impeded. She had been briskly purged before coming under my care. I ordered a compound calomel pill to be taken night and morning, and some infusion of cascarilla with sesquicarbonate of soda three times a day; a blister to be applied to the nape; and a collyrium of vinum opii in thrice the quantity of liquor ammoniæ acetatis to be used occasionally.

"From the above date the case underwent steady improvement; the mixture appeared to induce headache, and was therefore discontinued, but the general plan of treatment was of decided service. By February 7th the paralysis was relieved, the conjunctiva being more sentient, though still far from naturally so. The vascularity of the membranes had subsided, the nebulous state of the cornea had diminished, its ulcerated surface was nearly healed, and vision was consequently improved. The patient remained another month under my care, during which, save the occasional recurrence of pain in the head, and once a relapse of conjunctivitis, the course pursued by her malady was one of gradual convalescence. The cheek and conjunctiva regained their powers of sensation to a great degree, but remained morbidly alive to impressions of cold, and subject to the feeling of formication on the contact of a finger. It was remarkable, and drew the spontaneous observation of the patient herself, that, as the sensation of the conjunctiva returned, intolerance of light arose, a symptom which had been entirely absent at the earlier period while sensation of the membrane was annulled, though both it and the cornea were then in a far higher state of inflammation." (p. 33.)

The peculiar localization of the facial paralysis in this case is well worthy of notice, though it would be mere guess-work to assign a cause. It also illustrates the connexion between the paralytic condition of the trigeminal nerve and the inflammation of the conjunctiva, the latter being cured by the removal of the former. Mr. France questions whether this conjunctivitis is really owing directly to the withdrawal of some peculiar presiding influence exercised by the nerve over the nutrition of these parts; and is more inclined to suppose that it originates from the unperceived, and therefore disregarded and continued, irritation of extraneous particles and currents of air, creating common inflammation of structures, the conservative powers of which are weakened (as those of every other part are well known to be) by paralysis. Two other cases recorded in this paper in which conjunctivitis did *not* follow paralysis of the nerve seem to favour this conclusion.

"CASE 4. Sarah B., aged 16, a thin, pale, cachectic girl. She dated the onset of her complaint to a period shortly before her admission, when the flow of the catamenia had been suddenly arrested from exposure to cold. Apparently from this cause severe strumous ophthalmia supervened, the intolerance of light accompanying which was most intense; and the right cornea first, and then the left, became opaque from violent asthenic inflammation. The routine treatment in cases of this kind was resorted to, such as a course of tonic purgatives, with alterative mercurials, leeching in the immediate neighbourhood of, and caustic irritation at a short distance from, the affected organs; but the usual beneficial results did not ensue to the wonted degree, photophobia continuing, and being little if at all relieved. The instillation of a belladonna collyrium, however, was attended with better effects upon this distressing symptom, which was finally removed upon a free application of nitrate of silver to the skin of the superior palpebræ." (p. 35.)

We need not continue the further narrative of this case, which is quoted to illustrate Mr. France's views of the cause of photophobia. The most common opinion is, that it depends upon irritability of the retina; but the cases we have copied throw great doubts upon the correctness of such a theory. In both the opacity of the cornea must have prevented, in a very great measure, the access of light, to this part of the eye. In the former, the symptom in question appeared only when the conjunctiva had regained its sensibility; in the latter, it was alleviated by the belladonna, which, while acting as a sedative on the nerves of the part to which it was applied, must have admitted a much larger amount of light to the retina, by reason of the consequent dilatation of the pupil; and it was finally removed by the caustic application to the palpebra, which would certainly act with more power on the conjunctiva than on the deep-seated retina. Mr. France, therefore, is inclined to believe that the fifth, not the second nerve or its expansion, is at fault; and we confess we think he has reason on his side. The same theory readily explains the paroxysms of sneezing so often caused by exposure of the eyes of patients with strumous ophthalmia.

"Grant it," he says, "and the parallelism in the causation of the phenomenon, when excited by an unusual stimulus, as snuff, applied to the quiescent schneiderian, or a wonted stimulus, as atmospheric air, applied to the same membrane irritated by nascent catarrh on the one hand, and when excited by light impinging on a morbidly irritable conjunctiva on the other, becomes apparent; different branches merely of the same nerve being, in that case, the afferent vehicles of an excito-motory impression. Again, lacrymation is excited simply by irritation of different branches of the same nerve, whether produced by stimulant particles applied to the nares, or to the surface of the conjunctiva. Why, in cases of intolerance of light, must it be originated through a separate nervous channel? We are no more bound, it must be observed, on the admission of this theory, to concede the least amount of *visual* power to the fifth nerve, than we are to acknowledge the residency of that power in camphor, or a solution of lunar caustic, because those substances exhibit an unquestionable appreciation of the presence of light. But, on the other hand, to attribute to the conjunctival filaments of the ophthalmic a consciousness (so to speak) of the presence of light,—independent of the properties of that agent in relation to special sense, but connected, it may be, with its general chemical influence,—is but to recognise their possession of a sensibility, supplementary indeed to, but in strict accordance with the prophylactic character of, that which they are universally allowed to enjoy. The ophthalmic nerve is the sentinel of the eye." (p. 38.)

We feel called upon to take exception to the phraseology in which Mr. France has expressed his ideas; since neither the terms "consciousness" nor "sensibility" can be rightly employed as expressive of the endowments of the filaments of a nerve. *Impressibility*, though a somewhat uncouth word, is the only one that is really appropriate.

The two next cases to which we shall refer, illustrate some points connected with the phenomena of the motions of the pupil. In the first, the patient laboured under fatal cerebral disease. The right eye was healthy, with good vision, and active pupil of medium size. The left eye was inverted, and the power of abducting it was lost; the pupil was obedient to the influence of light, but contracted to about half the diameter of the right pupil, though free from adhesions. The left nostril was insensible to odours, and the left side of the tongue incapable of taste; and a considerable portion of the same side of the face was paralysed both as to sensation and motion. About four days before death, belladonna produced dilatation of both pupils.

In the second case, the patient had had three attacks of hemiplegia. When admitted last, the left side of the body was still suffering in some measure from the disease. The left nostril was quite insensible to snuff, the left ear deaf; the facial muscles enfeebled, and the integument benumbed. Both eyes were perfectly amaurotic. The irides were free from adhesions, the right pupil was about the medium size, and circular; the left was contracted to half the diameter of the right, and rather oblongated in the horizontal direction. Belladonna caused dilatation of both, but the relative proportions remained unchanged.

Mr. France, in the last volume of these Reports, adduced a series of cases in support of the opinion that the contractility of the pupil depends upon integrity of the third nerve; and in reference to the two we have just noticed, he observes:

"Though falling very far short of demonstration, they yet strongly countenance the experimentally-derived opinion of Valentin, that the act of dilatation is likewise presided over by special nervous filaments; and that those filaments reach the iris through the ophthalmic nerve. For in both cases the ophthalmic nerve being affected with partial paralysis, the function attributed to it by the above named author failed, and the pupil became contracted accordingly. Other nerves, it is true, were paralysed at the same time, but it were easy to show how greatly the balance of probability is against their failure having aught to do with producing the remarkable contraction in question; as anatomy would teach, and common experience proves, that paralysis of the olfactory, auditory, and facial nerves has no effect whatever on the iris; and the abducens may be completely paralysed without the pupil displaying the slightest sympathy." (p. 59.)

Nor can it be contended that the contraction depended upon the amaurosis; for in all cases of this disease in which such a condition has been found, it has depended upon the occupation of the individual, having made it previously habitual, which was not the case in these instances.

III. *Observations on some obscure and difficult forms of hernia; with cases and illustrations;* by Edward Cock.

This is a valuable practical paper. Mr. Cock chiefly confines his attention to two forms. In the first, there is a prolongation of the hernial sac beyond the internal ring into the abdominal cavity, where it becomes dilated into a pouch of greater or less size, lying on the fascia iliaca, between the internal ring and spinous process of the ilium, and contained between the fascia transversalis and the peritoneum. On the inner aspect of this pouch, at a distance of between one and two inches from the internal ring, is the opening of communication with the peritoneal cavity; and the circle of this opening forms the seat of stricture. This pouch is no doubt of slow and gradual formation, and is probably the result of frequent and protracted manipulation to reduce an old hernia.

In the next form, the obstruction is caused by old-standing, irreducible omental hernia; irreducible, either because it has contracted adhesions to the walls of the sac and the margin of the internal ring, or from a gradual accumulation of fat, which prevents its return. When this is the case, the omentum, after passing down the abdomen, becomes contracted into a firm and unyielding cord as it approaches the internal ring, where it is firmly fixed; and if a coil of intestine passes under it, it may become jammed, as it were, and unable to free itself, and thus strangulation results.

IV. *An account of the dissection of two anencephalous monstrosities;* by Alfred Poland.

The chief points of interest in this paper are the additional proofs afforded that the nerves are independent, as respects their development, of the presence of the nervous centres,—for the brain and spinal cord were entirely absent in the one case, and the brain in the other, while the nerves were quite perfect,—and the evidence of the loop-like character of the central terminations of the nerves, on which point the author's statements entirely concur with those of Dr. Lonsdale. It is well to observe, however, that although such cases of monstrosity may be freely admitted as affording strong presumptive evidence that a *portion* of the nerve-fibres have a looped central termination, they cannot be regarded as demonstrating that this is true of *the whole*, or at all invalidating the concurrent statements of various recent observers, that a continuity may be frequently traced between the nerve-fibres and the stellate prolongations of certain vesicles of the ganglia, and of the cerebral substance.

V. *Two cases of dislocation, one of the humerus into the axilla, the other of the radius forwards and upwards;* by John Hilton, F.R.S.

VI. *A rare case in midwifery;* by Dr. Oldham.

The subject of this case had borne several children, and had many miscarriages. Dr. Oldham was called in consultation on June 30, 1845. Four days before this, being at the full term, she had been seized with sudden flooding, but labour-pains did not follow. When seen by Dr. Oldham she complained of diffused tenderness over the abdomen, increased by moving. There was no sickness, nor rigor, nor febrile symptoms. The abdomen was evenly distended, as usual, at the ninth month. The cervix uteri was shortened; the os readily admitted the finger, and the fœtal head covered by the membranes could be felt. The mammæ were distended with milk. No attempt was made to excite the uterus, the treatment being directed to the removal of the abdominal tenderness, &c.

After this period, a fetid, coloured discharge and gas escaped from the vagina, and the patient began to lose flesh and strength. Vigorous efforts were now made to rouse the uterus by the administration of ergot and subborate of soda, by friction, by electricity, and by attempting to dilate the cervix mechanically; but all was of no avail. Gradually the greater portion of the dead fœtus was removed piecemeal, but the patient sunk at last, three months after the time when labour should have commenced.

On examination, a cyst was cut into below the umbilicus, which contained a number of bones closely set together with a quantity of thick, dark-coloured putrilage. It was formed in front by the lower part of the abdominal walls and the bladder; it was covered in above by the small intestines and omentum, which had adhered together, but so feebly, that they were readily separated, while the back part was formed by the posterior wall of the cavity of the uterus. The whole of the anterior wall of the uterus was absent, leaving only the front lip of the cervix to complete the os uteri. The bladder was so thinned in one part, as to be quite translucent, and all but perforated.

We quite agree with Dr. Oldham in believing that this was *not* a case of extra-uterine fœtation, but that the fœtus was really included in the cavity of the womb; but how that organ could have become thus strangely altered, we feel unable to decide.

VII. *Report of the Clinical Society, from March* 1846, *to April* 1847. *The medical division*, by R. Finch, M.D.; *the surgical*, by Samuel Wilks.

These are well drawn up, but incapable of analysis.

VIII. *Select clinical reports. Cases and observations illustrative of the etiology of enlargement of the heart;* by G. H. Barlow, M.D.

Under the term enlargement, Dr. Barlow includes both hypertrophy and dilatation. The causes which produce it, may be reduced to three classes, viz. 1, obstruction from changes in the orifices of the heart, or in the course of the circulation; 2, obstruction arising from changes in the quantity or physical properties of the blood; 3, deficiency of strength in the parietes of the heart itself.

In reference to the first class, our readers will bear in mind that the proposition is now pretty generally assented to, that diseases of the heart are propagated in a direction contrary to the course of the circulation; e. g. when there is impediment in the aortic orifice, there ensues hypertrophy or dilatation, or both, of the left ventricle.

Our author commences his examination with the right ventricle, obstruction to the return of blood to which gives rise to hypertrophy and dilatation of the right auricle; engorgement and myristication of the liver;

obstruction to the functions of that organ, and to the portal circulation, and consequent ascites; engorgement of the cavæ, especially the ascending; anasarca; engorgement of the kidneys, and lesion of the function of those organs. Now, this obstruction may be caused immediately by disease of the tricuspid valve, a very rare affection; by disease of the sigmoid valves of the pulmonary artery, which is also rare; by disease of the pulmonary arteries and its branches; and chiefly by affections of the lungs and air-passages. The latter is illustrated by an interesting case of long-existing bronchitis, to which, as to all the others narrated in this paper, we must refer our readers, confining ourselves almost entirely to general statements. The same effects also are produced by defective development of the lungs, independently, to all appearance, of any antecedent structural lesion of those organs; and also by adhesion of the pericardium occurring before growth is completed, and impeding the full expansion of the lungs, by embarrassing the respiratory movements.

Obstructive disease of the mitral valve comes next in order as a cause of enlargement of the right heart, then disease of the aortic valves, and, lastly, of the aorta itself.

A common mode of death, when the aortic valves are diseased, is sudden syncope; the left ventricle becoming, after perhaps some undue effort, over-distended, and unable to contract upon its contents. But there is another termination which Dr. Barlow believes has not been hitherto expressly noted, viz.:

> "That it sometimes happens that the dilatation of the left ventricle thus produced, is greatly disproportionate to the hypertrophy; and consequently it contracts so feebly upon its contents, that but very little blood is thrown into the aorta; the effect of which must be that, though the cavity of the left ventricle is large, it can receive but little blood. Engorgement of the left auricle then ensues, and the condition of the patient is then rendered, in many respects, the same as if there was disease of the mitral valve; and death takes place, not as would be the case directly from loss of power in the left ventricle by syncope, but by engorgement of the lungs, right heart, liver, &c., terminating in death by apnœa."

In regard to the second class of causes, it is doubtful if there is such a thing as simple superabundance of normal blood, though, did such exist, it would probably give rise to hypertrophy or dilatation; but many cases of heart-disease are to be traced to abnormal conditions of that fluid. Magendie has shown that a liquid rendered somewhat viscid, is capable of being propelled through capillary tubes much more readily than water; and hence it must follow, that when blood loses its viscidity, and is brought to a state more nearly resembling water, as is the case in Bright's kidney, after repeated or large hemorrhages, and possibly in chlorosis, we must have a condition tending to produce hypertrophy and dilatation. But here, at the same time, the third class of causes comes into operation; for the diseased condition of the blood renders it unfit for healthy nutrition, and consequently the heart, while called upon to do more work, has its energies for working impaired.

Like all Dr. Barlow's writings, this paper deserves a most attentive perusal. But we have to complain of considerable carelessness in correcting the press, many gross typographical errors having been allowed to pass unnoticed.

On the whole, this volume is of much value and interest; although we miss any continuation of those valuable contributions to minute anatomy, physiology, and scientific pathology, which have so honorably distinguished the former numbers of this serial.

Art. V.

1. *Opere di* Maurizio Bufalini, Professore della Clinica Medica nelle Scuole Medico-Chirurgiche di Complemento e Perfezionamento dell' Universitá di Pisa in Firenze. Vol. I. Parte 1 e 2.—*Firenze*, 1844–5.

The Works of M. Bufalini, Professor of Clinical Medicine in the Medico-Chirurgical Schools of Completion and Perfection of the University of Pisa in Florence. Vol. I. Parts 1 and 2.—*Florence*, 1844–5. 8vo, pp. 378, 379.

2. *Elementi di Patalogia Umana di* Luigi Bosi, Ferrarese.—*Ferrara*, 1843–4.

Elements of Human Pathology. By L. Bosi, of Ferrara. Vols. I and II. —*Ferrara*, 1843–4. 8vo, pp. 446, 512.

3. *Nuovi Elementi Fisio-Patologici di Medicina Eclettica*. Dell Dottor Niccolo Celle, Medico nei RR. Spedali di Pisa.—*Pisa*, 1841.

New Physio-Pathological Elements of Eclectic Medicine. By Dr. N. Celle, Physician to the Royal Hospitals of Pisa.—*Pisa*, 1841. 8vo, pp. 490.

We are all ready to acknowledge Italy as the mother of the fine arts, the land of poetry and music, of beauty and romance; many can sympathise with the patriotic glow of Pellico for the soil which he reverences as the dust of heroes:

> "——— E il piû gentile
> Terren non sei di quanti scalda il sole?
> D'ogni bell' arte non sei madre, o Italia?
> Polve d'eroi non è la polve tua?"*

but few, probably, consider how deeply medical science is indebted to her sons. We shall therefore, without further allusion to the writers of ancient Rome, take a hasty review of the claims of the Italian school of medicine.

From the eighth to the twelfth centuries the only European schools of medicine, with the exception of the Saracenic schools in Spain, were the Neapolitan establishments of Monte Cassino and Salerno, which enjoyed a high reputation in the eleventh century. The latter school is supposed to have been the first where medical men were obliged to go through a regular course of study, and, after passing examinations, had a diploma granted to them. In the thirteenth century this university became eclipsed by the rise of a rival at Bologna, where Mondini obtained extended fame as a teacher of anatomy, and introduced the practice of dissection in the schools. His anatomical plates are very early, if not the very first examples extant.

The invention of printing, the consequent publication of numerous monographs, and the establishment of other universities at Padua, Pavia, and Rome, led to the manifestation of a spirit of general improvement in medicine. The great advance made in the sixteenth century by the labour of the anatomists is chiefly to be attributed to Vesalius and his successors, Eustachius and Fallopius. In the succeeding age, Borelli and Bellini made another step in advance, by explaining many of the phenomena of the living body by the laws of natural philosophy; the former illustrating muscular motion and action by mechanical principles, the latter explaining various other functions by the laws of hydraulics and hydrostatics. Up to this period almost all medical theories were founded upon the humoral pathology, and the first writer who systematically opposed this theory, arguing that any alteration in the fluids was owing to previous change in the solids, was another Italian, Baglivi. By thus drawing attention to the morbid conditions of the vital organs, a grand advance was made in pathology; and, in the seventeenth century, pathological anatomy may be said to have arisen in Italy by the labours of Valsalva and Morgagni. So far the Italians appear to have taken the lead in the scientific pursuit of medical study; but from this time they have rather followed their French, German, and British rivals. The zeal with which the doctrines of Brown were propagated by Rasori and adopted in the leading schools of Italy, forms a curious chapter in the history of our profession; and not less interesting is the candid manner in which Rasori afterwards acknowledged the errors he had fallen into, and combated the doctrines he had formerly introduced and advocated. It must be admitted that most Italian writers of the past half-century have devoted themselves much more to complex theory and abstruse speculation, than to accurate observation and the record of facts,—have laboured far more earnestly to establish their own doctrines, than to investigate the phenomena of disease and the effects of remedies,—and have endeavoured to make their experience support their favorite tenets, instead of inquiring how far facts were in favour of their truth: and there can be no doubt that in many instances the influence of this theoretical spirit upon practice has been most deplorable, and that the celebrated satirist, Pignotti, had some grounds for styling the physicians of his day "involuntary ministers of death," and for his lines to the effect that medicine destroyed more than the plague and the sword—

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* Francesca di Rimini, atto 1, scena 5.

"——— A spopolar la terra
Più dell' istessa peste, e della guerra."

But it is time that we should take a hasty glance at the more prominent doctrines of the opposed leaders of the modern Italian schools. Dr. Celle, in his chapter on the various systems of medicine, refers them to three classes—1, the humoral, or chemical; 2, the organic, mechanical, or solid; 3, the dynamic, or vital. The two former were the most prevalent in the earlier periods of our science. Stahl and Hoffmann were the first leaders of the latter. We need not here refer to the various doctrines of Boerhaave, Hoffman, Haller, or Cullen, as they do not appear to have had any peculiar influence upon the Italians; but Cullen's opponent, Brown, attracted a degree of attention by his doctrine of excitability, which affects this school to the present day. We must just remind our readers of the leading principles of his theory, in order to point out where the modern Italian writers differ from, or, as they say, reform him. Excitability he assumed to be a specific property or power, possessed by the living body—everything affecting the body acting as an excitant or stimulant upon this property, producing a healthy state of the functions when not excessive or defective. Stimulants, if excessive, produced exhaustion or direct debility; if defective, excitability was accumulated in the system, giving rise to what he called indirect debility. Thus all diseases depended upon a state either of direct or indirect debility, and he classed them accordingly into sthenic and asthenic diseases; and the treatment, without regard to particular symptoms, was solely directed to increase or diminish excitement—in 97 of 100 cases stimulating treatment being required. This doctrine, from its simplicity, the easy application of its principles to the various branches of medicine, and its wide general views of the origin of vital and morbid actions and of the operation of remedies, was eagerly embraced by a large majority of Italian physicians of that day, especially by the younger men, who are described as, "with a degree of fanaticism, almost believing it to be infallible." (Celle, p. 67.) It had a most remarkable effect upon practice, until Rasori arose to oppose some of its principles, particularly as regarded the action of remedies. He urged that all applications to the living fibre cannot be considered merely as stimulants of different degrees of intensity, but that, on the contrary, certain substances, instead of stimulating, depress, diminish, or even directly annihilate sensation and motion,—in a word, excitability in general; and therefore he divided remedies into two classes, stimulant, and contra-stimulant or debilitant. Again, he urged that the greater number of diseases are not asthenic or diseases of debility, as Brown supposed, but sthenic, or depending upon an augmentation of vital activity, almost always manifesting itself in the form of inflammation; which, considered in its nature and effects, however it may occur, whatever may be its degree, seat, or accompanying symptoms, is always an identical and uniform process, always the product of increased stimulus, absolute or relative, and always to be treated by contra-stimulant means. In practice he was the first to establish what may be called the law of tolerance, or the rule that the operation both of stimulants and contra-stimulants is tolerated by the living body, without disturbance of its functions in certain conditions of disease, and without any of the effects the same remedies would produce in the healthy system. For a long time these opposed doctrines of Brown and Rasori were agitated in the Italian universities, until the physiological system of Broussais was promulgated; but this was merely a modification of their systems, looking, not so much to the degrees of excitability, as to its modification by the special organization of each organ or system; and holding that the vital power, not being equally distributed, predominated especially in the stomach, and that almost all irritations depend upon irritation of this viscus by the intimate sympathy existing between it and all other parts of the system. Thus, the primary seat of most diseases is in the alimentary canal—gastro-enterite. The general indications of treatment drawn from these propositions were calculated to limit the materia medica to cold and acid drinks, and threatened, as Dr. Celle remarks, the extirpation of the whole race of leeches; but still there can be no doubt that the labours of Broussais had a good effect upon the science, by drawing attention to the irritability of particular organs, and to the effect of this upon the whole system.

If Broussais has led his followers into error by teaching that all diseases are primitively local, and that all general symptoms are merely evidences of sympathetic irritation excited by that local disease, he has undoubtedly done much by proving that this is *frequently* true; and by deducing from the intimate relations of the organs with each other, important illustrations of revulsion and sympathy, showing how disordered action in one organ is relieved by exciting action in another or on the surface. Thus, visceral disease is combated by exciting diaphoresis, cerebral congestion by promoting secretion from the gastro-intestinal mucous membrane, hepatic disease by diuresis, &c. All this had been acted upon in a measure before Broussais, but he first clearly expounded the doctrine; and showed also how disease of any organ may be produced by the continual exhibition of medicines which stimulate it, and thus disease of the whole system be induced. His illustrations also of the sympathies of relation and of organic life, have had most important bearings upon practical medicine; but we cannot now examine these, as we merely wish to point out the effect their promulgation was calculated to produce upon Italian medicine.

About the time that the system of Broussais was most in fashion in Italy, Paccinotti began to advocate his etiological doctrines. He perceived that the commencement of morbid action must necessarily be some point of transition between the healthy and the diseased condition of some part or organ, which must be connected with some change in the relation between external nature and the human organism. This observation of the spontaneous acts of nature, he established as the foundation of the science of disease. For this observation he selected certain simple forms as types of disease, and argued that the intimate process of disease cannot be determined, and therefore cannot form a foundation for any system of pathology; but that our attention should be directed to its salutary modifications, the processes of beneficial change which animal chemistry effects according to determinate periods and laws. Although this diseased condition of certain organs and functions can be recognised, the therapeutic art has no other sound foundation than imitation of the spontaneous action of nature. Thus in the study of pathology, endemic and epidemic diseases must be first observed, and no special mode of treatment can be laid down until any given disease has been analysed in the various occasions in which it has shown itself as an endemic or epidemic. This is the Hippocratic part of the doctrines of the etiologists; but they add to it the study of all the known morbid alterations discernible in the various organs and tissues; so that Paccinotti and his followers are said to go to the bed-side with Hippocrates on the right hand, and Morgagni on the left. From this empirical foundation, they advance to the general application of the system, on the ground that the etiological and prophylactical parts include the true philosophy of medicine. They study the laws of connexion between cause and effect, carefully distinguishing between a *succession* and *connexion* of phenomena; and as connexion can only be proved by an intellectual process of induction, they term their system an inductive pathology. The connexion between occasional causes of disease and the organic function electively disturbed by them, leads to the establishment of a general law of physiological affinity; and then the *idiopatia* or pathological condition must be studied with regard to the spontaneous processes of nature for its removal, and the therapeutic measures in imitation of their processes. There are four principal types of the idiopathical condition—the rheumatic, inflammatory, septic, and nervous. These are regarded as chemico-organic morbid processes distinguished from each other by varieties of cause, and by the different anatomical tissues and vital functions in which they operate. Remedies are expellent, imitating crisis by vomiting or purgation; secernent, also assisting or imitating crisis by perspiration, urine, bilious or non-bilious diarrhœa; denutrient and antiphlogistic—nutrient, or tonic, or corroborative—nervous or antispasmodic,—epispastic, or revulsive—and specific remedies, of which they only admit bark and mercury.

We have given the heads of Paccinotti's doctrines, which he supported with great ability and eloquence, because he appears to have opened in some degree the true method of induction in the study of disease. The observation of the spontaneous acts of nature, and the doctrine derived from it of the action and critical efforts of the *vis medicatrix*, are most ancient; but have been regarded rather as precepts of clinical prudence, than as the very foundation of the science,—which it became in this school, when associated with the study of morbid anatomy, and of the connexion between various causes of disease and corresponding functional derangement. There can be no doubt that good has resulted from the dissemination of the writings of this school, by leading to a system of medication more in accordance with the natural progress of disease. But, like other systems, it has its weak points; and we think that it would not be more difficult to show the fallacy of a too exclusive confidence in the *vis medicatrix*, than it has been to demonstrate the absurdity of that system (still, unfortunately, by far too much acted on), which regards every disordered condition of the system as demanding the active interference of the doctor, and the antagonistic agency of some incongruous combination of medicaments.

At the time when Bufalini first wrote, the schools of Italy were agitated by the opposed doctrines of Brown—then on the decline,—Rasori, Tommassini, who merely slightly modified those of Rasori and Paccinotti. Bufalini first practised in Bologna, and acquired considerable ability as a professor of clinical medicine there, and as the writer of a work styled 'Analytical Pathology.' He afterwards removed to Florence, being appointed physician to the large hospital of that city and clinical professor in the medical school. He is now generally regarded by the Italians as the first of their physicians, but is said to shine more at the bedside than in the lecture-room, and to possess remarkable tact in diagnosis, and ability and readiness in the adaptation of remedies. In the two volumes of his collected works before us, the only practical chapters are on the medical virtues of iron and on rheumatic tetanus, and these are of no very great importance, the remainder being solely theoretical and controversial. Instead of attempting to follow the course of these volumes, we shall simply lay before our readers an account of his system of medicine. His followers are called *mistionisti*, from the prominence of what is termed his doctrine of mixture—that is, that life is the result of the union of a specific principle with matter. He argues that life is not a primary, simple, indivisible faculty, but a secondary force which results solely from the combination of particles of matter, or rather is the result of a particular disposition of the primary forces inherent to these material molecules, as gravity, impenetrability, &c. Thus, this force, the first origin of vital phenomena, only exists by organization; and changes of the former are always resolved into those of the latter, in other words, they are merely various modifications of the organism. He regards the vital power as a pure result of organization, and in this sense secondary, not possessing power in itself, but communicating it to the organism when it enters into combination with common matter. If then the vital principle cannot produce the phenomena of life except when united with matter in the state of organization, it appears manifest that it can only form an element of the organic state. We must remind our readers that we are now merely stating what these opinions are,—not examining their soundness. Certain changes between the material particles of the organism constitute the primary actions upon which health and disease depend; morbid causes tending to disturb the order of the organization, remedies to restore it to the normal state. In short, all the functions of the animal economy, every change of every kind which is effected in the body, both in the healthy and diseased state, are material, not purely dynamic; do not proceed from a particular vital force *sui generis*, but by organic connexion; are not simple phenomena, but the product of an extended and obscure series of causes, the actions of which, however varied and manifold, are distinguished under two grand classes: 1, Those that act by simple contact in a manner physico-vital or simply dynamic; 2, those that act in a manner chymico-organic. These only differ in that the first merely induces a molecular change, the second a more lasting and material one. By the organism, Bufalini intends not only the solids but the fluids, as they form with each other in the human machine an inseparable whole, the one existing for and by the other; and it is on their reciprocal action that life is supported, by their equilibrium that health is sustained, and by their disorder all the principles of disease arise. Thus, disease may arise either—1, from alterations in the order, proportion, and nature of the principles which constitute the organism; or, 2, from the introduction of an incongruous or hurtful principle. After this, he forms a general division of diseases into two grand classes—1, Dynamic or mechanico-organic; 2, Chemico-organic. The first, or those which arise from an incongruous agent that mechanically excites to disordered movements; and as it is impossible to suppose that such an agent at the same moment disturbs every point of the animal machine, diseases so arising are considered as local. The second are considered as general or universal, and are not, as understood by the ancients, those in which all or many of the functions were affected as distinguished from local disease, in which few functions are affected; because this division refers rather to the effect of the disease than to the disease itself, since local irritation, worms, calculi, &c., excite general symptoms, and general diseases often present symptoms purely local. To the primary processes of disease secondary ones may be added, and remain after the action of the former has ceased, and this is effected—1, by consent; 2, by hydraulic and chemical disturbance or loss of balance (sbilanci); 3, by alteration of organic assimilation. In the first order are included all sympathetic affections, whether arising—1, by antithesis of organs; 2, from association or similarity of structure; 3, from peculiar relations of nervous action. Disease may be complicated not only by these secondary processes, but also by two primitive morbid processes of a different nature, which may arise and subsist independently of each other.

He makes a further division of his mechanico-organic and chemico-organic classes of diseases. Of the first there are three orders:—1, Lesions from altered reciprocal relation of the organs; 2, from alteration of connexion; 3, from the manifest presence of unusual matters or bodies in some part of the organism. In the first order, or diseases of relation, he places three genera—diseases of connexion, of arrangement, and of proportion between contained and containing parts. In the second order, or diseases of arrangement, we have two genera—1, Lesions of cohesion of organic particles—adhesions; 2, lesions of mass and composition—atrophy and hypertrophy, morbid transformations and organic productions. The three genera of the third class relate to the nature of the foreign substance, whether—1, fluid or gaseous; 2, solid; 3, living.

The second class, or chemico-organic diseases, consist in an aberration of the assimilative action or processes of vital chemistry. In this class are placed the following orders; plaustossia, plastollia, politrofia, oligotrofia, atessibrofia, idiotrofia. Increased assimilation is the character of plaustossia, and is peculiar to inflammation, which consists in three elements—congestion, angiocinensis, and plaustossia. In plastollia there is a tendency to organic dissolution; it varies in degree, being either serous, scorbutic, or putrid. Contagious diseases constitute a fourth genus of plastollia. The third order includes plethora, hypersarcogenesis, and hyperosteosis; the fourth, anemia, hyposthenia, and rachitis; the fifth includes the uric and phosphatic diathesis, comprehending gravel, rheumatism and gout; the sixth, the hidden and specific diseases proceeding from the scrofulous, scirrhous, and cancerous diatheses.

The cardinal points of these doctrines are thus shortly summed up by Dr. Celle:

> "That the state of vital power, or the first origin of all vital phenomena, is only a state of the various conditions of the organism, or an entity, or secondary force of the organization; that on the innumerable and incalculable alterations of affinity, order, and position of the material elementary molecules composing the organism, depend the infinite crowd of diseases to which the animal economy is subject; and, lastly, that diseases are only a particular or specific alteration of the material compound or mixture of the organs." (Celle, p. 93.)

. Probably the principal good which has resulted to the Italian school from the writings of Bufalini, is from the force with which he has argued that the ultimate operations of vitality are altogether incomprehensible to us, and therefore that theories and systems founded upon any such uncertain and speculative base ought not to influence practice. The primitive alterations induced by disease in the organic condition of any tissue cannot be defined, and therefore the choice of remedies cannot be regulated *à priori*. The voice of experience alone can be heard at the bedside. It is not necessary to know the precise mode in which medicinal substances act, this is far above our faculties. Our knowledge of life and disease is empirical, that of remedies must be empirical also, but it must be the empiricism of the rational physician who examines all the most minute circumstances of the case, confronts them, examines their bearings upon each other, and distinguishes the essential from the accidental with all the exactitude of analysis. Remedies, so-called *juvantia* and *lædentia*, are often very fallacious, "because the natural progression of the disease is confounded with the action of the remedies" (Op., p. 61); and besides this, the changes they produce in the state of the functions do not demonstrate their power of meeting the secret substantial alterations which are the basis of the disease. Thus the treatment of no disease can be decided by any *à priori* reasoning as to its nature, nor by any general relation between diseases; but every disease requires its own specific treatment. Quinine cures ague, sulphur the itch, mercury syphilis; practice demonstrates this, and it suffices for the physician. This is direct cure, because it is directly by the action of these substances that the morbid poison is neutralized or destroyed. A rational or indirect cure is that in which the physician considers the dynamic condition, that is to say, medicates symptomatically. When treatment has quieted some excessive motions, and so diminished the urgency of some symptoms, the benefit is indirect; the capacity for sensation of the sensible and irritable fibres is diminished, the disturbing morbid influence is less felt or better supported.

Bufalini is, by some of his Italian critics, regarded as holding an intermediate station between the pure Vitalists and Localists, because he insists so much on loss of balance between vitality and the organism; and he anticipated the eclectic school in showing that the fluids and solids form a whole, and are to be equally considered in the study of the origin of disease. But it appears to us that his opinions on life and organization have led him into many errors of the anatomical school. We are not disposed to go the length which the opponents of the latter would desire, and advocate the vital origin of all morbid organic changes, and regard disease as essentially alteration of function. The progress of animal chemistry and the new light which is being thrown upon the laws of morbid poisons, evidently point to the material origin of many diseases; but this need not induce us to overlook, with the anatomists, that opposite organic conditions may produce the same symptoms, and, on the other hand, identical organic alterations may produce the most opposite symptoms—that symptoms may disappear while the organic change is persistent, and again, the latter may disappear and the symptoms persist—that the most severe symptoms may be unaccompanied by any discoverable organic alteration after death, while extensive organic disease may be unattended by any characteristic symptoms. All this is proved by daily experience in practice, and shows that the science of medicine is a part of the science of life, which cannot be founded on observations made during the calm of death, after the storm of disease has passed.

Do not let it be supposed that we underrate the study of morbid anatomy because we admit the force of such arguments, and feel obliged to confess our ignorance of the exact seat of some of the diseases in the treatment of which we are most successful—intermittent fever, for example—and do not understand the *modus operandi* of the remedies by which we cure them. But this, so far from depreciating the value of morbid anatomy, should lead to its more ardent cultivation; since it is absolutely necessary as a foundation for correct diagnosis, and for a knowledge of the processes of nature. To be available for practice, however, it must be studied with regard to the first evidence of departure from healthy structure; the effect of one diseased organ or tissue on others and on the whole system; the laws of development of morbid action; the relation between the organic change and the symptoms and signs observed during life; the processes of spontaneous cure; and the modifications of healthy vital processes by disease.

But we are anticipating the writers of the Eclectic school. The great error of systematic physicians, or the danger to which any system, exclusively followed, exposes the mind, is a blind preoccupation or prejudice, often amounting to fanaticism. When the mind is occupied by one idea, facts are not observed in a truthful spirit. "Le désir de voir une chose fait que souvent on la voit partout," and we are all too apt to believe what we wish to believe. Thus, agreeable errors soon appear as realities, and are preferred to unpleasant truths, and a party spirit is engendered which has produced as baneful effects in the scientific as in the political world; all merit rests with the partisan, all error with the opponent; evils are not seen or allowed, and therefore not amended; doctrines are judged without examination, are blindly approved and as blindly condemned; and not only are opposite opinions condemned and persecuted, but their authors also. Hence unfriendly, uncharitable feelings, useless controversies, and scientific discords. And this is not all, for when unsound theories are carried out in practice, the effect is that of bringing disgrace upon an art. What is the practical result of a blind adherence to the system of any of the schools whose tenets we have been commenting on? The Humoral pathologists, regarding the fluids as the cardinal principles of the animal economy, and their various combinations and alterations as the proximate cause or essential constituent of every morbid affection, looked only to morbid matter, superabundance of humours, gastric impurities, spoke only of acidity, alkalies, corruption of bile, coction, crisis, &c., and were necessarily led to the abuse of so-called evacuants, purgatives, diuretics, diaphoretics, acids, and alkalies. The Solidists, on the other hand, considering the human body as an aggregate of molecules or atoms, or a machine having the properties of other solid bodies, preserved by the common laws of mechanics, and dependent solely upon its physical conditions, recognised no other cause of disease than alteration in the mechanism of the molecules, their figure, size, gravity, &c., and therefore their therapeutic measures were adapted solely with a view of regulating the temperature, density, or conformation of the solids. Hence their diluents, deobstruents, astringents, corroborants, and the like.

If these two sects, by attending exclusively to, or altogether neglecting, the condition of the fluids, fell into grievous practical errors, their successors, followers of Brown and Rasori, at least equalled them in this respect; the one, seeing in everything weakness, languor, direct or indirect debility, diffused with unsparing hands the most powerful stimulants, and condemned as universally all depressing methods of treatment; the other, regarding almost all diseases, acute or chronic, as inflammatory in character, only looked to the means of depressing the vital powers by abstinence, bleeding, and powerful medicines. It would be impossible to calculate with anything like exactitude which of their systems, fully carried out, would do most harm, but it is plain that either, exclusively followed, would render medicine, which in certain ages it has been described to be, rather a curse than a blessing to society.

The errors of those who exclusively attend to local lesions or organic changes in disease, and of those who neglect them or regard them as secondary to general disturbances of the system, are equally evident. Probably the former is the more prevalent mistake with us at present, particularly in all diseases which run a determinate course. Local lesions, which are secondary, or often probably in some degree a salutary effect of the disease, are attacked by active treatment, which must disturb the system, interfere with the curative process of nature, and this with a very doubtful probability of benefit. Again the expectant system to which these considerations would lead, if *always* acted upon, would doubtless lead to a criminal inactivity in some cases; but dangerous boldness is the more prevalent error of the present day.

These are the arguments of the Eclectic school, of which Dr. Celle is the apostle in Italy. He was preceded, however, by Medici, who wrote in 1819, with a view of harmonizing the discordant doctrines of the Italian schools, and turning the minds of physicians from trivial disputes to united exertions in the progress of science. He pointed out where the truth lay in the opposed dynamic, organic, and etiological doctrines; and his studies were directed towards conciliation, and to lay the foundation of a medical edifice on the union of those intellects by which the profession was divided into factions. He also endeavoured to show that this eclecticism, or organico-dynamic system of general pathology, was in direct accordance with the general facts or positive doctrinal precepts of Hippocrates. Drs. Celle and Bosi have followed the same track, and have each given forth a classification of disease on these principles. Their works are very long, not very well written, and are thoroughly Italian in refined speculations upon subjects which, from their very nature, cannot be perfectly understood. An English reader, on opening a work entitled, "Elements of Human Pathology," would expect to find some account of the progress and effects of disease in the various organs of the body; whereas in Dr. Bosi's work, not a syllable to this effect is to be found, but instead there are long dissertations on *general notions* of medicine, symptomatology, etiology, nosology, &c., the doctrine of predisposition and temperaments, the theories of inflammation, irritation, and asthenia, the morbid processes of assimilation, morbid periodicity, some generalities upon organic diseases and monstrosity; a review of the progress of medical doctrine, with a concluding essay on diagnosis, prognosis, crisis, and critical days. And on these subjects we have upwards of a thousand octavo pages, which have sorely tired our patience while looking them over, in hopes of finding something valuable amid such a mass of lumber, and what is worse, have afforded us an unsuccessful search. Dr. Celle's book is almost as long, and rather better written. It has the organization of a good book, but vitality is wanting. It is divided into three parts: the first an analysis of all the systems of medicine; the second a physiological analysis of the formation of the animal economy, with essays on general pathology and therapeutics, and a classification of diseases and remedies; the third is a sketch of special pathology and therapeutics, or a clinical application of the doctrines of the Eclectic school. The two volumes of Bufalini's works, as we before stated, consist almost entirely of controversial pamphlets in support of his peculiar tenets. We have gone through them with very little either of pleasure or profit. The author is a good, though rather prosy writer, and his work shows that he is a scholar, and a gentleman in his sentiments, but we cannot say much more in his favour.

Anything like a critical analysis of these works would have been a sad trial to our own and our reader's patience; and we have therefore preferred to lay down as briefly as possible the result of the reflections excited by their perusal.

Art. VI.

Traité Pratique et Historique de la Lithotritie. Par le Docteur Civiale.—*Paris,* 1847.

A Practical and Historical Treatise on Lithotrity. By Dr. Civiale.—*Paris,* 1847. 8vo, pp. 610.

Notwithstanding the extent to which this subject has been discussed on former occasions, we consider that a review of the state of opinion in this country with regard to the relative value and success of lithotomy and lithotrity in the treatment of urinary calculus, is required; as we have observed in the books and journals of the day, in the lectures of practical men, in the wards of our hospitals, and the conversation of our friends, what we consider to be proofs that lithotrity has not yet met with a just appreciation on this side the Channel,—that the use of instruments, which experience has shown to be imperfect or dangerous, is still persisted in by many,—that important recent modifications in earlier methods are by no means generally adopted,—that surgeons do not prepare themselves for operating on the living by acquiring the requisite manual dexterity by practice on the dead, and, consequently, that very often the method is blamed for unfortunate results, of which the operator is alone guilty,—that the classes of cases in which lithotrity is considered to be applicable are not accurately defined, the ill-success of some exclusive advocates of lithotrity having led many British surgeons as exclusively to adopt the older operation, and the majority to undervalue the new,—and, lastly, that preparatory and after-treatment are not attended to with sufficient care. The publication of the work before us by the real practical originator of lithotrity, renders the task we had prepared for ourselves a comparatively light one; as we so fully concur in almost all the practical statements of the surgeon whose experience in this department of his art has been the most extensive in the world, that we have little more to do than to lay before our readers an analysis of the results of M. Civiale's long observation as presented in the first section of his book. A few preparatory remarks, and slight comments from time to time, are the only additions we shall make to this analysis; not entering upon the question of rival claims to invention, or enlarging upon instrumental modifications of secondary importance; but principally endeavouring to point out the cases in which lithotrity, properly performed with suitable instruments, should be preferred to lithotomy,—what is the relative success of the two operations,—how lithotrity should be performed in simple and complicated cases,—what preliminary and after-treatment are necessary,—and how the various accidents or unpleasant symptoms, which may arise during or after the operations, are to be avoided or remedied.

Success is the grand criterion in surgery; but success can only be justly appreciated by statistical results; and unfortunately, as yet, no accurate tables can be formed of the comparative results of the two operations, so few surgeons having published the general account of their experience. Dr. Prout made an attempt to ascertain the proportion of cures to deaths after lithotomy, and from returns of the Bristol, Leeds, and Norwich Infirmaries calculated that the general ratio was 1 in $5\frac{12}{66}$, but it varied greatly with the age of the patient and the magnitude of the calculus. The tables will be found at the end of his work on 'Stomach and Urinary Diseases,' and it is much to be regretted that similar ones are not drawn up at our metropolitan hospitals. This general ratio is drawn from about a thousand cases, and the further researches of Dr. Willis show that the truth is probably thus nearly approached. Doubtless, great diversity will be found in the practice of different surgeons: we hear Mr. Liston talking of 24 successful cases consecutively; we know that Mr. Martineau only lost 2 in 80; we also know that a surgeon of a celebrated Northern hospital is so uniformly unsuccessful, that the students cite an occasional recovery after an operation by him as something almost miraculous; and M. Civiale states that in Paris, in 1836, there were 25 deaths to 45 cures. This diversity exists, but the general average is pretty well made out. Hitherto this has not been done for lithotrity. Mr. Key, Mr. Teale, and others, have published a few cases; and we remember Mr. Cæsar Hawkins stating, at a meeting of the Medico-Chirurgical Society, that he had attended 60 cases with Sir B. Brodie, only one of which had proved unfortunate; but nothing from which a table could be compiled of the results of English practice has been laid before the profession. In endeavouring to analyse the results of the practice of other French surgeons, M. Civiale found that, from 1832 to 1838, 124 patients were submitted to lithotrity in the principal hospitals of Paris; 78 cures and 27 deaths are alone recorded, the result in 22 not being stated, or the cure being incomplete. In 1842 M. Civiale again writ[illegible] that, since 1836, various surgeons had applied his method in 38 cases, 22 cures and 11 deaths resulted; while in the same period, of 78 operated on by himself in the Hôpital Neckar, there were 73 cures, and only 5 deaths. This is quite sufficient to show that even in Paris the proceedings of M. Civiale and other hospital surgeons must differ essentially; for this surprising difference in the results is not to be [illegible]ounted for simply by the selection of cases, as of 111 cases of calculus occurring in the other hospitals, only 38 were judged fit for lithotrity, while of 97 in the Hôpital Neckar, 78 were submitted to this operation. In the Neckar, three fourths of the calculous patients were subjected to lithotrity, and the deaths were 1 in 16; in the other hospitals only a fourth were thus treated, and the deaths were nearly 1 in 3. Our own experience has not been very extensive, and, as far as numbers go, will be unfavorably contrasted with that of M. Civiale; as in 33 cases, 5 deaths resulted. In all the unsuccessful cases, however, the patients were upwards of 60 years of age; two of them were between 70 and 80; and in 3 the operation was attempted rather as a last resource, than with any great hopes of success, and had probably little influence upon the duration of the patient's life. In all these cases we should rather have performed lithotomy, but the patients obstinately refused to submit to it, and earnestly begged for lithotrity.

The practice of M. Civiale may be divided into two periods: in the first, from 1823 to 1836, he had used his three-branched instrument, crushing the stone in all cases, when it was not too large or too hard, between the branches and the head of the perforator, and if this did not answer, having recourse to perforations to facilitate the crushing process. In the second period, from 1836 to 1845, he used the screw and percussion instruments, either alone or combined with his older ones, and the results are stated as follows:

"In recapitulating all the facts, we find them,

"1. In the first period, until 1836, 506 patients attended; 109 were not operated on, and 307 operated on, of whom 7 died, and 3 only obtained an imperfect cure. In 1 the result was unknown.

"2. In the second period, from 1836 to 1845, 332 new cases were presented to me. Of this number, 241 were lithotritized. To these operations 25 must be added, proceeding from the return of the stone in 26 patients, which gives 266 operations of lithotrity during the last nine years of my practice. The result has been 259 cures, some of them incomplete; the patients having, besides the stone, serious lesions of the bladder or prostate, have continued, after the treatment, to suffer some functional derangements due to these lesions.

"Four cases in which death followed at a greater or less interval, after attempts at crushing by other surgeons, are only here related to keep them in mind, and because I shall speak of them hereafter.

"I have made known the presumable cause of death when it occurred during the treatment. I have also noted two cases in which the patients perished from diseases unconnected with the operation and the calculous affection, the one two months, and the other six months after the operation, and the complete cessation of the symptoms of stone.

"In 10 cases, which form a special table, lithotrity was attempted but not borne; and lithotomy being either also contraindicated, or the patients refusing to submit to it, death followed. In several, especially in 6 cases, the attempts at crushing have caused the morbid phenomena to assume a certain severity, which may have contributed to hasten death. Still great prejudice would be required to put these last cases to the account of lithotrity; the art of diagnosis not having been perfected, the operation has been attempted in unsuitable cases.

"In 8 patients it was thought useful to combine lithotomy and lithotrity with modifications required by each particular case; 7 were cured, although placed in serious circumstances.

"In 43 cases neither special explorations nor operation, properly so called, were performed, the conditions being evidently so opposed to the employment of the new method, that one could not even think of practising it. Some of the patients have continued to live, still bearing the calculus, others perished from the progress of the disease. In 2 of these patients other surgeons, against my advice, attempted lithotrity, and the results were unfortunate.

"In 28 other cases, unsuitable to lithotrity, the patients submitted to lithotomy, and 17 were saved.

"The new facts confirm, in all points, the exactitude of the deductions I had drawn from the 307 former ones.

"Nearly 600 patients have been lithotritized by me only, and all the varieties of the disease have presented themselves sufficiently often to remove the least doubt of the value of the new method in all cases. I have operated from the most tender age (less than 2 years) to decrepitude (87 years), and in all shades of the disease, from the most simple to the most severe and complicated cases.

"In comparing the results of the first and second periods, a great difference is found in the application and extent of the new method. During the former years nearly half of the cases appeared to be unsuitable; now, about three fourths of the calculous patients are submitted to it,—a proof, at the same time, of a progress in the art, and a happy modification in the ideas of patients and surgeons. Art, more confident in itself, can now attack cases which prudence formerly commanded it to abandon. As to the patients and surgeons, they are now better aware that, to assure the success of the operation, they must not wait until the health is ruined and the stone large, for then art becomes too frequently impotent.

"The mortality is greater in the new than in the former list. Looking only at the figures, such a result would imply a contradiction with what ought to be expected from improvements in the instrumental apparatus and operative proceedings. To explain this, I must enter into some details, rendered necessary besides by the erroneous interpretations which my first successes have called forth.

"At the commencement of my practice, I felt strongly that the destinies of lithotrity were attached to the fortunate or unfortunate results of my operations, because other surgeons had not then operated either with my instruments or with those they themselves designed. It was necessary, therefore, in order to ensure these results, to use extreme care and caution, especially in the choice of cases; and I think it of great importance to observe that, in my first operation, applying my method to very simple cases, that is to say, to those of small stone, healthy bladder, and good general health, the cure was constant, and the treatment proceeded with a regularity and promptitude that I did not observe under other circumstances. I restricted myself, then, in the first period, to operations upon simple cases only, and accordingly had only to fear fortuitous accidents, which are rare when the necessary precautions are taken. Doubtless I was thus apt to refuse the succour of lithotrity to patients it might have cured; but the destiny of a new method was at stake, to the account of which would certainly have been placed results, which would have depended solely upon an injudicious choice of subjects. Success alone could impose silence upon an opposition each day more threatening; and, to obtain success, it was necessary to operate only in those cases in which it was almost certain. But when the new method was appreciated, when it had attained the position in surgery which could henceforth never be contested, our duties were different, for humanity commanded recourse to the operation which offered the greatest chance of saving the patient; thus, whenever lithotrity, without promising a certain result, still offered a greater probability of success than lithotomy, it was practised. Now it will be understood that, practised in doubtful cases, more or less beyond the sphere of its methodical and regular application, it necessarily led to a greater proportional mortality, and brought on accidents previously unknown. The consequence was natural, and the difference in the results should have surprised no one. Still the fact itself has been falsely interpreted, especially with regard to the real value of lithotrity. This demands further notice.

"Experience has established—

"1. That the art of crushing the stone, applied as it ought to be, and within the limits which are now well known, is not of a nature to endanger the life of the patient.

"2. That, beyond the limits prescribed either by the size or number of the stones, or by concomitant organic lesions, it may still be applied, but does not then present either the same harmlessness or the same certitude.

"3. That some of the accidents and dangers which have followed its application, have resulted from defective proceedings, and the greater part from the disorders caused by the length of time the patient had suffered from calculus (*le séjour prolongé du calcul*)." (pp. 574-8.)

It is quite evident, then, that although a correct general average of the success of lithotrity cannot as yet be drawn, and the same diversity in the relative success of different surgeons is observed as in lithotomy, still a very decided numerical advantage can be shown by the advocates of this new method. M. Civiale's success is greater than the most sanguine lithotomist could reasonably expect to obtain, and is so strongly contrasted with the numerous failures of other lithotritists, that the greatest possible weight should be given to his opinions.

M. Civiale explains the ill success of other Parisian surgeons, by stating that they do not make themselves practically acquainted with the instruments by operating on the dead body, and that they are ignorant of their defects, and of the real difficulties of the operation. Seeing an expert surgeon operate easily, they think others can do the same, "hence so many failures." Again, they operate without previously obtaining a correct knowledge of the condition of the stone and of the urinary organs, and without proper preparatory treatment, make the sittings too long, manœuvre without sufficient precaution, and frequently do not employ an injection to keep the coats of the bladder from the stone and instrument; they use complicated and imperfect instruments, and often substitute mechanical contrivances for the hand of the surgeon; some bind their patients, and fix their instrument to an immoveable support; and almost all neglect the precaution by which retention of urine and arrest of the fragments of stone are prevented. If these statements be correct, and our own observation convinces us of their truth, not only as regards French but some British surgeons, it is not surprising that their results should be so different; nor need we offer any other reason for giving a close analysis of the practical portions of the important work before us.

In the first chapter on the instrumental apparatus of lithotrity, the author enters at considerable length into a description of his first instruments and their successive modifications. Here we need not follow him; as the principal improvements, consisting in the substitution of curved for straight instruments, the introduction of percussion and the screw, and the opening in the extremity of the instrument, are well known. The instruments most successfully employed in the present day are, first, the screw lithotrite, generally used in this country; the second, the rack and pinion, delineated in Mr. Fergusson's book (Manual of Surgery, 2d edition, p. 632), but a French instrument and used by many French surgeons; the third is the modification introduced by M. Charrière, which is now almost universally preferred on the Continent, and is the instrument we have always used ourselves. (Civiale, pp. 27-31.)

The first of these three is decidedly the worst. It is so heavy that the screw portion has to be disconnected until the stone is seized, and the subsequent fixing is an important loss of time. The principle of this screw is also bad, as it does not convey to the hand of the surgeon an idea of the amount of force he is using, and he may apply enough to bend or break the instrument without being conscious of the great force he puts in action. Neither does it allow him to make those rapid motions of the handle backwards and forwards, resembling slight percussions, which are so effectual in breaking up hard stones. The second and third differ principally in the handle, and we must prefer the latter. It is more manageable, and the strongest man could not employ a dangerous degree of force, the resistance of the pinion being so graduated that the teeth would give way before the instrument would bend or break. It is light, easily cleaned, and the teeth on the extremity of the female branch which assist in fixing the stone, are shielded by a very simple contrivance from injuring the mucous membrane in the withdrawal of the instrument. The male branch is graduated in order to show the size of the stone. Percussion may be employed if necessary, and in this case the means are afforded of fixing the instrument. As some explanation of the cause why the French makers are so much better known than our own all over the Continent, we may mention that we only paid M. Charrière 35 francs for this instrument (less than 30 shillings), and it was beautifully finished. A friend of ours paid nearly four times this sum to an English maker of some reputation, and this very instrument broke in our hands while we were experimenting with pieces of coal on a table.

The extremity of these instruments between the curve and point is always now made with an opening, so that the fragments cannot prevent closure of the blades, but there is often much difficulty in seizing the stone, on account of the narrowness of the branches. It is plain that a spherical or ovoid body would be very apt to slip unless seized exactly in the centre, and to diminish this evil, M. Civiale constructed instruments with large flattened extremities. He found that the female branch became clogged because it was too deep, and larger at the bottom than at the entrance. He, therefore, flattened and widened the beaks, so that what was gained in breadth was lost in thickness. The introduction of these instruments is not more difficult, and the seizure of the stone much more easy. Their power, however, in breaking a hard stone is not so great as those we have just figured, so that it is often necessary to use the others at the first sitting, and employ the wider beaked one only to crush the fragments. These instruments are delineated at page 34 of M. Civiale's work, and by their aid the stone is generally very easily seized, and instead of the larger angular pieces of calculus thrown off by the narrow beaks, a kind of rough powder is formed which easily passes the urethra.

With regard to fixed supports for the instruments, and means of fixing the patient, M. Civiale justly remarks that they are more injurious than useful, and totally unnecessary when any screw instrument is used; but when the size or hardness of the stone call for percussion, some support is needed. The instrument must then be maintained in a state of complete immobility, and it matters little in what manner, either by a hand-vice or one fixed to the bed of the patient. The hammer should be small, with a short handle; hard blows should never be struck, but light ones rapidly following each other.

Having thus indicated the necessary instruments for breaking the stone, we shall now describe the manner of using them, considering first *the preliminaries of the operation.*

Upon the table placed near the ordinary bed of the patient, the surgeon arranges a catheter, oil, a syringe filled with tepid water, and an instrument proportioned to the size of the urethra and of the stone. The patient lies horizontally on the bed, with a pillow or cushion rolled up in a sheet under the sacrum; the legs being separated, and the thighs slightly flexed. A vessel is placed between the thighs to receive the urine or any of the injected fluid which might escape and moisten the bed. By this position the bladder is not brought into contact with the walls of the abdomen, and the pelvis is so elevated that the most depending portion of the bladder is opposite the internal orifice of the urethra. It is there that the stone is almost always found, when the vesical parietes are distended by an injection. This position is also the least alarming and fatiguing to the patient. The surgeon should place himself on the *right* side of the patient, and gently introduce an ordinary catheter. If the bladder contain urine, it should be allowed to empty itself. This precaution, though not indispensable, is particularly advisable at the first sitting, and especially when the stone is large; because the quantity of urine contained in the bladder is not exactly known, and consequently the operator does not know how much fluid he should inject. In some cases of thickened and irritable bladder, however, this precaution is more injurious than useful. The catheter being supported in the left hand, the syringe is adapted to it with the right, and the fluid injected, not hastily or in jerks, but very slowly, and in a continual stream. As soon as the patient feels a desire to urinate, the syringe is removed, and the catheter closed by the thumb and gently withdrawn.

In an irritable bladder, forced injections should never be employed; in some cases quoted by M. Civiale, they have produced the most disastrous effects. In general the quantity of fluid injected should be sufficient so far to distend the bladder, that the manœuvres are executed in water; from ten to twelve ounces ordinarily suffice, even when the stone is very large. In some subjects whose bladder is very large and atonic, a much larger quantity might be introduced without exciting contraction, but it would be inconvenient, especially in cases of small stone, to enlarge too far the cavity we have to explore.

> "In certain cases, the bladder being thickened and hardened (*racornie*), and contracting too powerfully, particular precautions are indispensable; by neglecting them we should be liable, as it has frequently happened, to see obstacles arise which might lead to the belief that lithotrity could not be practised. I suppose that these cases will not be confounded, as appears to have been done in England, with those in which, the catheter not having reached the bladder, its eyes are obstructed by the neck or the engorged prostate, and the liquid only reaches the cavity of the organ with difficulty, and causes pain; it is enough to know that this circumstance may present itself, to guard against the deceptions it might lead to. But I wish to speak here of cases in which the bladder, hardened and very irritable, revolts as it were against the injection, and contracts as soon as it receives the first jets of liquid; if the injection be continued, the fluid returns between the catheter and the canal. This case may be embarrassing, and is more worthy of attention as it is very common." (p. 46.)

In such a case, in an old man, we lately tried the effect of the inhalation of ether; but although perfect loss of consciousness and insensibility to pain followed, the contractions of the bladder were not in the least diminished, and we were obliged to postpone the operation, and submit the patient to a course of leeching, hip-baths, and opiate injections into the rectum, before repeating the attempt, which was then successful. The plans recommended by M. Civiale, are to elevate the pelvis much more than usual by a very thick cushion; to introduce the catheter very gently, and not allow the urine in the bladder to escape; to inject with extreme slowness, so that the dilatation of the walls of the bladder may be almost imperceptible; to stop when the patient has an urgent desire to urinate, and if the liquid escape between the catheter and the canal, to compress the latter between the fingers during the withdrawal of the former, and immediately to introduce a screw instrument of sufficient size to fill the urethra. These precautions are generally all that are required; but sometimes it is well to allow the patient a few minutes' repose, as some subjects after a short rest will easily support an injection which the bladder previously rejected; indeed we know that the contractions of this viscus have but a short duration. Some very excitable patients who cannot bear injection, can still allow a sufficient quantity of urine to accumulate in the bladder to render the operation safe; in other cases the operation must be adjourned, and a preparatory treatment adopted which will be afterwards described. M. Civiale generally employs tepid water, and sometimes a slightly emollient decoction, as the injected fluid. He remarks, with much truth, that the oily, mucilaginous, and sedative injections recommended by many inexperienced surgeons are either useless or injurious, and deprive this part of the operation of the simplicity by which it is distinguished.

A chapter follows on the use of straight crushing instruments, which we need not notice, as our readers are not likely ever to employ them; its conclusion, however, on the duration of sittings, is of great importance, as the author shows very clearly, from his early experience, that long sittings were generally followed by febrile reaction. He formerly occupied from twenty to thirty minutes, afterwards he restricted the period to five or ten, and even less; in some cases the bladder was so irritable that not more than one or two could be borne, yet the patients were safely cured. It is a most important precept to make the sittings very short, and operate very slowly and gently. To this simple rule M. Civiale attributes a great part of his success. It should be especially followed in the first sittings; afterwards, when the irritability of the bladder is diminished, and the organ is accustomed as it were to the manœuvre, the duration of the sitting may be safely protracted to ten minutes.

Use of the crushing instruments. It is well, before introducing the instrument, to give the patient a slight inclination towards the surgeon, so that the stone may fall to the right side of the bladder, the position in which it is most easily seized. It must be remembered that the curve of the instrument is shorter and less regular than that of an ordinary catheter. It consists of two portions, one straight, the other almost straight, united by an elbow. This must be remembered at the moment the instrument clears the pubic arch and neck of the bladder, and the curved portion must be constantly maintained in the direction of the canal. Those who neglect this rule experience great difficulty, and injure the urethra. Thus, while the curved part passes the cavernous portion of the urethra, the instrument should be inclined either to the right or left side, the penis being drawn with one hand towards the hand which holds the instrument. When the point arrives at the bulbous portion of the urethra, the penis and the part of the instrument which remains external must be brought to a perpendicular direction, so as to make a right angle with the body of the patient. The curved portion thus continues in the direction of the canal under the arch of the pubis, and as it advances the external extremity must be brought towards the patient's thighs, with a slow movement, without jerk or force; the operator passing the instrument onwards as it is lowered, and never forgetting that the curved portion only should be in the direction of the urethra. If the prostate be in a normal condition, the instrument clears the neck of the bladder without the least difficulty. If the prostate be slightly tumefied, the hand holding the instrument must be further lowered, and still further if the tumefaction be more considerable, but always with the greatest caution, at the same time one hand being pressed above the pubis to diminish the action of the muscles of the anterior wall of the abdomen. Sometimes a fungous excrescence of the prostate opposes the passage of the instrument at the moment it should enter the bladder. This difficulty must be overcome in the same way as that from considerable tumefaction of the gland. Sometimes a transverse fold suddenly changes the direction of the urethra; the instrument arrives easily as far as the internal orifice of the canal, and then it is suddenly arrested; but once passed, there is not that impediment to a free motion of the instrument in the bladder which occurs in fungous excrescence or considerable prostate enlargement. All that is necessary to clear this obstacle is to lower the instrument without advancing it. By neglecting this precaution the fold might be torn or perforated, as many examples have proved.

"When the instrument has reached the bladder, the position of the stone is to be determined. If it does not present itself, the two branches are separated to the distance of eight or ten lines by withdrawing the internal branch, and the foreign body is again sought for by inclination or partial rotation of the instrument. When it is felt, which generally occurs on one or other side of the fundus of the bladder, especially on that towards which the patient is inclined, the instrument is directed towards this point, so that its curved portion lies upon the stone without pressure. If not sufficiently opened, the branches are to be separated more widely, they are lightly pressed upon the stone, and slight movements backwards and forwards suffice to indicate when it is seized, or to effect its seizure.

"In simple cases, if the stone is small, and a lithotrite with a wide, flat, and short beak be used, the seizure of the calculus is in general easy; it is the affair of a moment to a surgeon who is much in the habit of operating, and the patient experiences but little pain. In such a case the curved part of the instrument is turned downwards towards the rectum.

"When, on the contrary, the stone is large, the bladder indurated, and an instrument with its curvature long and open is used, the affair does not proceed in this way. In the first place the lateral and other motions of the curved portion in the bladder are supported with difficulty by the patient; there is inevitable rubbing against the internal surface of the organ, which causes pain and troubles the surgeon. The difficulty and pain are still greater when the surface of the bladder is irregular, and especially when prostatic tumours or fungus exist. In these cases also, when the presence of the stone has been determined on one or other side, where, I repeat, it is commonly found, the two branches of the instrument slightly separated are applied upon it; they are then more widely separated by drawing one forwards and pushing the other backwards, until they reach the borders of the foreign body. The instrument thus opened is to be pressed upon the stone laterally, so that the branches become placed between it and the walls of the bladder. But in proportion as this manœuvre is executed, the branches are approached to determine if the calculus is embraced; sometimes they slip over it, showing that they have not been sufficiently separated from each other, and that it is necessary to separate them more widely, in order to embrace the whole of the body, and then to close them again to see if it is seized. Sometimes they approach each other under the stone, proving that in passing the branches between it and the walls of the bladder, they have passed beneath it or to the other side. They should then be slipped below it in the same way as when above and not sufficiently separated. The essential point is to place them towards the middle of the stone, and this is the real difficulty. To overcome it, the surgeon, especially if his tact is not very fine and practised, is condemned to grope painfully to his patient and with discouragement to himself. But precisely because this is an affair of touch or tact, precepts are useless; nothing is easier than to say what ought to be done, but once at work every one must do his best." (pp. 65-6.)

One important point, however, should be kept in view; the stone is generally displaced by very slight touches of the instrument, and those who close the branches by drawing the one and pushing the other, frequently defeat their object, especially if the stone be small. The female branch placed backwards, and towards the fundus of the bladder, is generally the only one in contact with the stone; and if this branch be moved to close the instrument, the stone is displaced, falls away from the branches, and cannot be seized. The female branch should be kept immovable, and the male branch only be moved in closing the instrument. If this be done gently, it is seldom that the stone escapes seizure.

> "When the stone is seized and solidly fixed, it is seldom difficult to crush it either by pressure or percussion. I will speak first of the former of these methods, at present the most in use. It may be executed in various ways.
>
> "1. If we have to deal with a small stone, or a fragment of a large one, the diameter of which does not exceed seven or eight lines, and the hardness moderate, pressure with the hand may suffice, and should be had recourse to, because it is a power preferable to any other. The palm of the right hand is applied to the extremity of the mobile branch, the medius and index-fingers being arched against the projecting part of the fixed branch, and by a strong contraction of the flexor muscles, a pressure is exerted sufficient to crush the stone. This proceeding is the most expeditious, simple, and snrgical that can be adopted; no other equals it.
>
> "2. If the stone or fragment, on account of its size or hardness, resist the effort of the hand, the position of the instrument must be by no means changed, and the screw put in action." (p. 67.)

Here the left hand holds the instrument, and the screw is managed with the right; and in changing hands it is necessary to be careful that the moveable branch is kept steady, so that the stone cannot escape. It is crushed either by steadily turning the handle so as to push the moveable branch onwards, or by a rapid succession of slight jerks almost resembling percussions. The branches are reopened by an inverse motion of the handle when the stone is crushed, and other fragments seized. But the case may be a more difficult one.

> "The stone is hard and large, has been seized at the centre, and resists the action of the screw; we must discontinue the pressure, and push the instrument charged with the calculus until it touches the posterior wall of the bladder, slightly incline it to the right or left side, and separate the branches for a few lines, so that they cease to compress the stone. As soon as the latter is free between the two branches and the point of the vesical parietes upon which it rests, the branches are brought towards the circumference of the stone by pivoting the instrument (*en faisant pivoter l'instrument*), and we endeavour to seize the foreign body upon the border in order to graze it (*l'écorner*) by strong pressure, and detach a portion of its exterior layers. By proceeding thus I have succeeded in crushing stones which had resisted when they were seized in the centre." (p. 69.)

When the stone resists pressure, lithotrity must be renounced, or percussion had recourse to. In employing the latter, the instrument must be fixed, and it must be remembered that the disintegration of the stone depends less upon the violence than upon the number of strokes with the hammer. It is difficult to say how many strokes can be borne; this will of course vary with the state of the patient. Several hundreds and even thousands have been supported, but the results have not been the most favorable; once the stone is broken, the screw suffices to crush the fragments. "Percussion should never be employed except to commence an operation in cases of hard and large stones; it is to the curved instruments what the system of perforations is to the straight ones, a means not of entirely destroying a calculus, but of diminishing its force of cohesion, and rendering it capable of being crushed." (p. 70.)

At the end of each sitting, before withdrawing the instrument, we must be very careful that no fragments remain between the branches. By a few turns of the handle of the screw backwards and forwards, any accumulated detritus is cleared away, and the graduated scale on the male branch shows when none remains by its exact approximation to the female. We may remark here a practical fact of some importance. We once saw a surgeon operate, who, instead of simply oiling his instrument before introducing it, covered it with some simple ointment, thinking the urethra would be thus better protected. The stone was broken, but some of the smaller fragments became incorporated with the grease, and so clogged the instrument, that it was necessary to dilate the meatus by incision before it could be withdrawn. This shows upon how slight a matter the success of an operation may depend.

In our analysis of these chapters we have purposely omitted all directions regarding the use of the straight and articulated instruments, as they have been superseded by the curved screw and percussion apparatus. We now arrive at the third chapter, in which the author enlarges on the

Preparatory treatment. After some common-place remarks upon the necessity of attending to the general health, allowing a patient to rest after a journey, attending to the diet and digestive organs, using diluents and baths, and not injuring him by purgatives, opiates, tonics, &c., the author treats of what he calls *local preparation*. He shows that, in many calculous patients, a simple sounding is often accompanied with great pain, and is sometimes followed by serious consequences; accordingly, with the exception of a very small number of cases, in which he has found the urethra not only free, but slightly irritable, he has always introduced daily, or every other day, a soft bougie of middle size, and left it for a few minutes in the bladder. If this is done slowly, the patient suffers very little. At the third or fourth time, the bougie is replaced by a larger one of the same composition. In general three or four sizes suffice; the first 2¼ lines in diameter, and the third 3¼ lines at most. The urethra thus gradually loses its sensibility, and other instruments may be passed without pain.

> "This preparation, which I cannot recommend too strongly, because it contributes greatly to the success of lithotrity, I apply to simple sounding whenever I have reason to suppose that the urethra and neck of the bladder are very irritable. When exploratory sounding is practised at once, as is the custom, the patient suffers greatly, and the pain frequently prevents such a continuance of the exploration as would be required to obtain the necessary information; in continuing it there would be danger of giving rise to serious consequences...... The duration of this local treatment varies. If the stone be small, the urethra and neck of the bladder but slightly irritable, and the bladder healthy, it need not be persisted in; generally, it is well to have a few days' interval between the exploration and operation. If the sounding has not been painful, and the patient has only afterwards experienced a slight increase of irritation, which has subsided in a few hours, the operation may be performed without delay; but in general it is more prudent not to hurry, to observe the patient, to know the effect of each bougie upon the urinary organs, and especially to watch the digestive and other functions. Long practice has proved to me the utility of all these precautions. I will further say, that, from having neglected them in some circumstances, and having yielded to the desires of persons who wished to finish the business at once, I have had the regret of seeing accidents arise which, even in three cases, have obliged me to renounce lithotrity." (pp. 79-81.)

Exploration. After enlarging considerably upon the uncertainty and inefficiency of the means hitherto employed to determine, with precision, the form, size, situation, and even existence of stone in the bladder, and the concomitant organic conditions which may render modifications in the mode of operating necessary, M. Civiale describes his own method of exploration. The patient is laid on his back, the pelvis slightly elevated, and a middle-sized catheter, with a slight curvature, is introduced. If the bladder be large and contain much urine, it is allowed to empty itself; and the manner in which the urine flows denotes the degree of vesical contractility. If the bladder be feeble, and still of considerable capacity, if the liquid fall instead of being projected, further exploration would be premature, and might produce serious consequences. On the other hand, if the organs are so irritable that pain and involuntary contractions are excited, exploration must also be adjourned; and in both cases preparatory treatment adopted. When the organs are in a fit state to bear the presence of instruments for a sufficient time to afford the information required, M. Civiale uses slightly curved catheters in preference to sounds, for, as the urine escapes, the stone is generally brought to the back of the catheter and detected at once. If the usual movements of the catheter in sounding still fail to detect the stone, he makes repeated injections of tepid water if the bladder contracts strongly, and of cold water if the contractions are feeble. In this way the form and capacity of the bladder are so changed, that the smallest foreign body must be infallibly detected; and calculi have been thus discovered, which could not be felt in any other manner.

It is worthy of remark that, in thickened and indurated bladders, small stones are apt to appear very large, because the bladder contracts strongly upon them, and prevents their isolation by the sound. In cases of enlarged prostate, with a sort of depressed sac behind it, the catheter, even with a very short curve, may not detect a stone; in such a case, a lithotrite may be introduced, and, by opening the branches, any foreign body would be certainly detected, and its size be determined by the graduated scale on the male branch, but not so exactly as is generally supposed.

Let us now pass to the *application of lithotrity;* and first to its application in simple cases, of which M. Civiale describes three series. In the *first series* a small stone of moderate consistence exists in a healthy bladder of ordinary capacity, the urethra being free, the prostate not tumefied, the general health good, and the digestive functions regular. It is only after exercise that the urine becomes turbid or sanguineous, and this ceases after repose. Preparatory treatment may or may not be required, according to the degree of irritability of the urinary organs. A middle-sized lithotrite may be used, and generally one sitting, of five minutes' duration at most, suffices to destroy the stone. If the surgeon be dexterous and careful, the patient suffers very little, the urine scarcely becomes bloody. The fragments partly escape with the first discharges of urine, if the bladder contract with force, and there is not much spasm of its neck or of the urethra. In the contrary case, the fragments, especially the larger ones, are not expelled until after a hip-bath, or on the following days. There is no fever, and merely, for some hours, a frequent desire to pass urine, and some little difficulty in doing so, with occasionally some sensation of heat. But in general, the day after the operation, no traces of it remain but in the memory. The cure is complete, and, after two or three days, the patient returns to his usual habits of life. Sometimes he will walk to the surgeon's residence in the hospital, submit to the operation, and return immediately afterward. M. Civiale says this has occurred in his practice several times, in our own it has happened twice without any bad consequence; but it is always better to order two or three days' rest even in the most simple case, as irritation of the neck of the bladder is much more easily avoided than quelled. In some such cases the forceps of Sir Astley Cooper might perhaps be successfully applied; but it is always safer to crush a calculus, however small, than to run any risk of our dilating the urethra, or of impacting it in this canal.

Second series. Let us suppose the organs to be still sound, or, at least, that any morbid alterations are but slight, and the general health good, but the stone is from 8 to 12 lines in diameter, and sufficiently hard to resist moderate pressure. In this case the broad and flat-beaked lithotrite is the most useful instrument. M. Civiale says, he has never met with a calculus less than an inch in diameter which has resisted it. After the division of the stone the frogments must be crushed; and to do this, the operation may be prolonged to five minutes if the patient do not suffer much. In many cases everything goes on as favorably as in the first series; but in others the urine only carries off the smallest particles, the larger fragments remaining in the bladder. Sometimes this only produces slight excitement, which ceases under the influence of a bath or an opiate enema, and on the following day the operation may be repeated; at others the excitement is greater, the passage of the fragments causes pain, and those which remain excite uneasiness and frequent desire to pass urine. These symptoms are generally most urgent the day after the operation, but gradually diminish during four or five days, in which baths, enemata, and diluents, repose and a milk diet are ordered. The operation may then be repeated, and this second sitting generally completes it, the patient suffers less than before, and the reaction is slight. It is very seldom that more than three or four sittings are required, and the whole treatment does not last more than from 8 to 15 days.

Third series. Here we also suppose that there is no important organic lesion, and that the bladder is capable of containing from 6 to 8 ounces of fluid. The stone, of moderate consistence, is from 12 to 20 lines in diameter. Hitherto the success of lithotrity has been wonderful compared to lithotomy; in this series, although serious difficulties may arise, the foreign body may also be destroyed, generally without danger, in five or six sittings.

In this case, if the broad-beaked instrument be used, at the moment the surgeon attempts to fix it the stone escapes, indicating that it is too large for the branches of the instrument; but these, in approaching each other, scrape the stone and remove some of its softer exterior layers, producing, in some cases, a quantity of fragments so large that, although the stone is not crushed, its volume is greatly diminished. If the stone be hard, it is simply found to escape as the branches are brought together. In this case the larger-branched, open lithotrite must be used, and the screw first tried, and, lastly, percussion, if this fail. Whatever instrument be used, it is especially desirable not to prolong the first sitting, and to precede it by careful preparatory treatment. It is not rare even then for fever, pain, and dysuria to come on in a sufficient degree to render postponement of the second sitting for a few days prudent. After this, matters generally go on as in the former series. The operation is more difficult and painful, but yet the result is generally satisfactory, arrest of the fragments in the urethra being the only accident likely to give trouble. Still in some few cases irritation and vesical contractions persist, and if the size and hardness of the stone are such as to render a long treatment inevitable, it is better at once to have recourse to lithotomy.

We shall now follow M. Civiale in his section on the application of lithotomy in complicated cases. He commences with the least serious.

1. *Excessive sensibility of the urethra and neck of the bladder, with or without increased contractility of the bladder.* This may be simply from a general nervous excitable state of the patient, or from organic disease. In the former case, if the stone is small and not very hard, lithotrity may be practised, but the sittings must be very short and at considerable intervals; and as soon as the sitting is over, a full-sized catheter with large eyes must be introduced, in order to bring away, by repeated injections, the greatest part of the fragments, and thus prevent their accumulation at the neck of the bladder. In most cases this will be successful, but occasionally the pain and fever are increased by the manœuvres. If this does not depend upon any fault of the surgeon, which it often does, lithotrity must be renounced. When the excitability depends upon organic disease, lithotrity must be often renounced without hesitation, unless the stone be very small.

> "In some patients the sensibility of the urethra and neck of the bladder is not notably increased, but the contractility of this viscus is excessive. As soon as two or three spoonfuls of urine are collected in its cavity, the person experiences an irresistible desire to evacuate it; and if injection be attempted, one or two ounces are scarcely thrown into the bladder before the desire of expulsion is strongly felt, even when the person has been properly prepared, and when, by great elevation of the pelvis, he is placed so that the vesical contractions are not assisted by those of the voluntary muscles. Hence the quantity of fluid introduced is insufficient to keep the walls distended, and to allow of manœuvring in liquid; sometimes even we are obliged to operate without fluid (*à sec*)." (p. 127.)

Perhaps proper local and general treatment will remove these conditions; but they may not do so, and the patient continues to urinate every half hour, has violent straining in passing the last drops, and as soon as one or two spoonfuls of fluid are injected, there is irresistible desire to reject them. In such cases, says M. Civiale—

> "I inject the fluid with the precaution I have pointed out. As sóon as the patient manifests an irresistible desire to urinate, I withdraw the catheter and introduce the crushing instrument with a little more haste than under ordinary circumstances. As soon as the branches are separated, I hasten to seize the stone. Almost always in this manœuvre a part of the fluid is ejected, but the calculus is seized; I know its size, and if it is capable of being broken, I proceed at once to crush it, which can be done without the least impropriety, even if no fluid remain in the bladder. The crushing does not cause pain. I do nothing more at the first sitting, and afterwards go on in the same manner to complete the operation. If, on the contrary, I perceive that the size and hardness of the stone would render lithotrity very difficult or impossible, I leave it as soon as measured, disengage the instrument, and withdraw it, the sitting being then nothing more than a simple exploration. . . . . . In many cases I have seen the first attempt produce an effect altogether opposed to what it would be natural to expect; the contractions of the bladder, instead of being exasperated by the manœuvre, were diminished." (pp. 128–9.)

The last remark is well worthy of being borne in mind. We have ourselves operated in cases in which we had great anxiety and fear of the effects of leaving a quantity of fragments in an irritable bladder; yet, to our great surprise, all the distress of the patients diminished as soon as the stone was broken; the straining and dysuria went off, and the bladder admitted a larger injection at the second sitting, which was much less painful than the first. Probably this is owing to the weight of the stone being distributed over a greater portion of the bladder, or taken from its most sensitive part.

> "After the first sitting the vesical contractions may increase and persist during several days, so as to render existence insupportable to the patient. I have encountered some such cases; I had manœuvred with great precaution, had made a very short sitting, and yet the contractions of the bladder increased in such a degree that it would have been imprudent to continue. Then I have suspended the process, and after having calmed the symptoms by an energetic medical treatment, I have sometimes succeeded in being able to recommence the crushing, but when the treatment proves useless, I do not hesitate to propose lithotomy, even when the stone is small, and it has been attacked at the first sitting." (p.130.)

In some cases this increased contractility is accompanied by hypertrophy of the vesical walls, the consequence of prolonged irritation; and when this is the case, the contractions become stronger in proportion to the increased thickness. This state is common in persons who have long suffered from stone, and if the stone is large, shows that lithotrity is not applicable. In some few cases it may be attempted, but accumulation of fragments about the neck of the bladder must be carefully avoided. As soon as the stone is divided, the smaller fragments only should be seized and crushed, taking care not to open the instrument sufficiently to admit the larger ones. By proceeding in this manner, and using injections with a large catheter, our object may be attained. Upon the first sitting success will generally depend, and the patient must be afterwards carefully watched, as the bladder contracting powerfully is apt to force fragments into the posterior part of the urethra with the first discharges of urine. The rule in these embarrassing cases is to free the bladder as soon as possible; by lithotrity, if the stone be small, and capable of being crushed in one, two, or three sittings, which should then follow each other every or every other day; by lithotomy, if the stone be large, or if there be several small ones. The case is urgent, the patient dies if nothing be done, and an operation, although dangerous, may save him. The true surgeon here shows his superiority, and takes the happy medium between a blind confidence and exaggerated fears.

2. *Atony of the bladder* is not rare among calculous patients, and is generally accompanied by augmentation in its capacity; the urine is never entirely expelled, and the vesical parietes never forcibly contract upon the stones, so that many of the symptoms of the disease are wanting. A large quantity of fluid may be injected, and lithotrity is thus rendered very easy, but no condition gives rise to more serious results, although to all appearance everything is satisfactory. There is no desire to void urine during the operation, nor soon afterwards, but when after some time this desire is felt, the patient cannot satisfy it; there is a true retention of urine, followed by general disorder of all the functions, increasing prostration, some organ becomes the especial seat of disorder, and the patient dies in a few days.

"Whenever I find that the bladder of a calculous patient does not entirely empty itself, after having diminished the sensibility of the urethra and neck of the bladder by preliminary treatment, I commence by injecting tepid water once or twice a day, according to the greater or less quantity of liquid retained by the bladder. After some days I diminish the temperature of the injected water, and at last use it cold. Cold enemata are employed at the same time. I have also recourse to a light tonic regimen, and even to a stimulating medication. Under the influence of these means, and above all being very careful never to allow the urine to remain long in the bladder, these conditions are rendered much more favorable. The repeated introduction of catheters and cold injections dispose the urethra, the neck and internal surface of the bladder to support the contact of instruments......... When the patient is properly prepared, when the urine is clear and its strong odour has disappeared, when it issues easily, without effort and almost completely, the operation may be performed, but with the greatest care. The sitting must be very short, the manœuvre gentle and slow. Pain and rubbing are easily avoided, because the capacity of the bladder is always rather large than small, and the walls contract but little during the operation. After the sitting repeated injections are made, to bring away the detritus; but the most important point is to carefully watch the patient afterwards. If the urine does not flow freely, the catheter must be passed once or several times daily, for the least over-distension of the bladder would produce a reaction, and disorders would arise under its influence. The only means of preventing the latter is to pass the catheter as often as the necessity for it is felt, never allowing the patient to suffer from retention of urine. This is not the case for leaving a catheter permanently; since if even the patient could bear it, it might do harm. This may be done, however, towards the end of the treatment, when the state of the bladder does not alter; or one may be left some hours during the night, if the patient is likely to suffer from delay. When fragments accumulate at the internal orifice, or deep part of the urethra, they must be immediately pushed back, or extracted in a manner I shall afterwards describe." (pp. 142-3.)

The result of the first sitting shows what may be expected from lithotrity. If accumulation of urine in the bladder, and if fragments in the deep part of the urethra, be prevented, which is generally the case, it is rare that unpleasant symptoms arise, and success is the rule. But, in some exceptional cases, inflammatory or nervous disorders, local or general, come on with obstinacy and intensity; lithotomy is then the sole resource.

3. *Strictures and other abnormal states of the urethra* may render a peculiar preparatory treatment necessary, and after the operation cause an arrest of the fragments. Simple strictures are best treated by dilatation with soft bougies, not left longer than five minutes at a time, to avoid danger of reaction. When they are long, hard, and callous, permanent catheters must be employed, only withdrawing them to introduce the lithotrite, and bringing away the fragments by copious injections. The catheter should be large enough to fill the canal, or fragments might collect between it and the walls of the latter, especially if the bladder retained considerable expulsive power. In some cases of hypospadias it has been necessary to enlarge the orifice by incision; this answers better than dilatation or cauterization.

4. *Engorgement of the prostate* exercises a great influence over the execution and result of lithotrity. We have seen how it affects the introduction of the instruments; during the operation the projection of tumours of varying form and size into the bladder interferes with the movements of the branches, and gives rise to painful attrition of their surface. The fundus of the bladder appears deeper than usual, and the calculus is

masked or shielded, as it were, by the tumour, and is found with difficulty. It is necessary to introduce the instrument much farther than usual, to apply its concave surface over the tumour, and, by elevating the handle, to turn the beak towards the rectum, displacing the stone by lateral movements of the branches, which should be slightly opened. Assistance may be rendered by the finger of the left hand in the rectum. These tumours also are great impediments to the expulsion of the fragments; many a patient, whose stone has been broken, has been obliged afterwards to submit to lithotomy, because the accumulation of fragments in the bladder excited dangerous symptoms. These considerations should lead to great reserve in applying lithotrity to such cases, and to the observance of the precept never to apply it when this condition really exists to a great degree.

5. *Urethro-vesical barrier*. This is found to occupy the inferior face of the vesical orifice, behind the verumontanum, and to cause an abrupt change in the direction of the deep portion of the urethra and of the neck of the bladder to an extent which may reach to an inch. It also interferes with the introduction and movements of the instruments, and with the expulsion of fragments; but these difficulties are to be avoided by proper caution, as before remarked.

6. *Fungous tumours* of the bladder are most commonly found in or about the neck, and, although they do not absolutely oppose the application of lithotrity, still they contribute to render it more difficult and painful. The difficulty is to distinguish the cases in which they oppose lithotrity, from those in which they do not. Suppose the existence, situation, and size of the fungus have been detected, it remains to determine if the tumour can be destroyed, and, if so, whether this shall be done in order to attack the stone more easily.

"I suppose now the case of a fungus with a large base, coexisting with a stone, seated towards the fundus of the bladder, and not susceptible of sudden extirpation. If this tumour is large, if it supports with difficulty the contact of instruments, if it bleed in abundance when touched,—if, again, the stone is large, and its destruction would require many sittings,—lithotrity must be renounced. It is not that the operation is absolutely impracticable; experience has even shown that it may be performed with success, as various facts prove, the details of which I have published, with others taken from the practice of my brethren. But occasional success is not a reason for always braving the serious accidents to which we are liable: and other cases, the details of which have not been made public, prove that these tumours, thus irritated, are susceptible of undergoing degeneration, which compromises the life of patients.

"When, on the contrary, the tumour is small, slightly sensitive, does not bleed, wherever it may be situated,—if the stone at the same time be one whose destruction would require but few sittings,—we ought not to hesitate to apply lithotrity. It must be borne in mind, however, that the manœuvre will be always painful, perhaps difficult; that groping (*tatonnement*) may be unavoidable; and that, instead of the stone, the tumour may be seized by the instrument. We should proceed slowly, and stop if the patient feel pain, or when we perceive that the body seized is flaccid and soft. The sensation that the surgeon then experiences cannot be described; but it will be understood that it cannot be the same on pressing a calculus and an organized mass. Suppose the fact be determined; if the tumour has been seized, the instrument is immediately opened, and the stone sought for, which is generally found around the base of the fungus; or the latter is extirpated. In the former case the crushing manœuvre differs from that in ordinary circumstances. The instrument must be opened very little, the curved portion turned towards the fundus of the bladder, the tumour passed over, and the fragments seized. With patience, a practised touch, and great habit of operating, we thus succeed in destroying the stone; but one who does not possess these qualifications will do well to abstain, for the case always presents great difficulties. When the fungus is small and occupies the fundus of the bladder, its existence is sometimes only discovered during the course of the operation, in seeking to seize the last fragments of stone." (p. 168.)

In these cases relapse, if not inevitable, is greatly to be feared. In some rare instances the internal surface of the bladder presents to a considerable extent fungiform excrescences, incrusted with calcareous deposits, the quantity and consistence of which might lead to the diagnosis of a large stone. Lithotrity, of course, should never be attempted in such cases.

7. *Columnar and sacculated bladder.* In many cases of hypertrophy of the walls of the bladder, the fleshy columns are irregularly developed, forming projections or irregularities, called columns. Between these columns spaces often exist, in which the mucous membrane, for want of support, protrudes and forms cavities, varying in size, number, and situation. These irregularities of the surface of the bladder, however, although embarrassing to an unpractised surgeon, are not such serious impediments to the operation or to the evacuation of fragments as might be supposed. M. Civiale details several interesting cases, in which he succeeded in crushing stones distinctly sacculated. Great care is of course required to avoid pinching the projecting bands of mucous and muscular tissues, but otherwise there is no peculiarity in the mode of operating. The case is more serious when atony of the bladder, with or without catarrh, accompanies this condition. M. Civiale says that his experience of such cases has convinced him, that when, by temporizing and the judicious employment of palliatives, patients may be brought to a state of tolerable comfort, it is better not to run the risk of an operation on the strength of a few successful cases. If lithotrity be practised, copious injections must be frequently repeated to assist in the expulsion of the fragments, and the patient carefully watched for a long time afterwards.

8. *Catarrh of the bladder* is a very common and important complication of stone. In its relation to lithotrity it is most desirable to ascertain if it have preceded the formation of stone, or is a consequence of the presence of the latter, if it have been specially produced by the foreign body, or if it depend upon morbid conditions of the neck or other parts of the bladder, and what these conditions may be. The catarrh which accompanies atony must be distinguished from that which accompanies hypertrophy of the bladder. As a general rule, it is only to small stones that lithotrity is applicable when catarrh of the bladder exists, but a remarkable exception to this rule must be remembered. When a stone has been produced under the influence of the catarrhal affection, it is almost always phosphatic, very soft, and easily crushed, so that its size under these circumstances would not be an objection to lithotrity. We lately crushed, at one sitting, a stone, an inch and a half in diameter, in an old gentleman who had long suffered from a catarrhal affection. Not a bad symptom followed, but, on the contrary, marked amendment. The precautions to be taken in these cases are the same as when irritability of the bladder is present, with the additional use of copious injections several times

daily, to bring away the fragments and the mucus mixed with the urine. Injections are still more necessary when there is also atony of the vesical walls. After lithotrity and succeeding injections the bladder recovers its contractility, and the catarrh diminishes progressively, often rapidly; so that these cases, in appearance unfitted for the application of the new method, in reality afford more satisfactory results than would be obtained by lithotomy in analogous circumstances.

The cases in which catarrh is produced by the operation will be afterwards considered. In cases of purulent catarrh, where the energetic contractions of the bladder show that its walls are hypertrophied and probably ulcerated, or when the catarrh is complicated by serious organic lesion, unless it is certain that the stone can be promptly and easily crushed, lithotrity must not be attempted. Lithotomy should be performed if not also contraindicated.

9. *Renal diseases* are most serious and insidious complications of calculus, but their influence relates to the propriety of performing the operation and to its results, not to its execution. We must therefore refer our readers to the works on diseases of the kidneys, simply remarking that some of these diseases, particularly those, as Dr. Prout observes, "connected with anæmotrophy, and occurring in early life," so enfeeble the vital energies, that the slightest shock is almost sure to destroy life. Thus, any operation would probably shorten the life of the patient, and give no chance of recovery. Inflammatory affections of the kidney, after proper preparation, do not forbid operation, although it is not free from risk.

10. *Cases in which we are called to perform lithotrity where others have failed.* M. Civiale quotes upwards of twenty cases in which he has succeeded, in many of them without difficulty, after some of the first surgeons of France had failed. He brings them forward as proofs that lithotrity is still imperfectly known, and that defective instruments are employed. We only allude to them here to impress still more strongly upon our readers the attention with which his cautious mode of operating should be regarded.

With this section the article on lithotrity in complicated cases is concluded. The following is upon the limits of its application, and this we have partly anticipated in our introductory remarks; but if an exact answer be required to the question, When is lithotrity inapplicable? we must say that this question has not yet been resolved. The size of the stone has been by some regarded as the chief matter, and a certain diameter given, which it is impossible to exceed; yet experience shows that the largest stones at all commonly met with are successfully crushed. Others attend chiefly to the morbid conditions of the bladder, prostate, and urethra, and also lay down laws, of which experience has proved the fallacy. All that we can say is, that when a stone is so large, that the strongest lithotrite cannot seize and fix it, or when organic lesions do not admit of the possibility of manœuvring the instruments, the inapplicability of the new method is indisputable. Still, it must be remembered that many persons, *apparently* in these unfavorable conditions, have been cured by an operation which at first sight appeared to be clearly contraindicated. We ourselves have crushed a stone, the fragments of which weighed four ounces and a half, in the bladder of a sexagenarian, whose condition was so unfavorable, that other surgeons had thought him unfit for lithotomy. Yet ten sittings in three weeks completely removed his stone, and he was soon restored to good health. Some who shudder at the very mention of sounding, on account of the irritability of their organs, may by care be brought into a state in which the operation can scarcely be called painful. On the other hand, in some cases, apparently favorable, the first sitting will prove that any existing derangements will be so much aggravated by a continuance, that death would follow if other measures were not adopted. We have seen by M. Civiale's statistical statement in how large a number of his calculous patients he has found that no operation could be borne, or that lithotomy, which offered the sole chance of success, was not submitted to by the sufferers. This was generally owing to the long continuance of the disease, from the negligence or ignorance of patient or surgeon. In 28 cases, in which lithotrity appeared inapplicable, lithotomy was performed, and of these 9 deaths resulted—not an unfavorable proportion, when we consider that nearly all the patients were advanced in life, and of course in unfavorable conditions. Eleven of these persons are stated to have undergone the high operation, and others *appear* to have done so also, although the fact is not distinctly stated; in some cases the bilateral section of the prostate was made. M. Civiale appears to prefer the high operation, and attributes its success to the prevention of the passage of urine through the wound by a catheter permanently fixed, and cleared from clots and mucus by injections. In 8 cases both lithotrity and lithotomy were performed; and from the results we learn the utility of immediately submitting patients to the latter operation, when attempts to perform the former have produced an exacerbation of the morbid phenomena, which persists more than a few days. In 10 other cases, in which lithotrity was followed by exacerbation of the symptoms, and in which lithotomy was either refused or contraindicated, the patients died.

Hitherto we have been engaged with the art of crushing a stone in the bladder; but this is not all that is required to cure a patient. The fragments must be carried from the bladder. We therefore enter upon the consideration of the means of liberating this organ, and of the immediate or remote accidents which may arise from an arrest of the fragments—in a word, of the treatment required and precautions to be taken after the application of lithotrity. Here we shall closely follow M. Civiale: his first article is *on the expulsion and extraction of calculous fragments.*

"In most cases the bladder has an expulsive power sufficient to eject with the urine all the fragments of stone, the diameter of which does not exceed that of the canal. The operator, so to speak, has nothing to do—he remains an observer, ready to meet the accidents which may arise, and of which I shall speak when treating of the arrest of fragments in the urethra. But the contractility of the bladder may be excessive or defective, an important distinction, which has still been neglected in practice. Now, in the two series of cases, the symptoms which may arise will not be the same, and the conduct of the surgeon will be different also. In the first, the bladder ejects fragments which cannot traverse the urethra. In the second, it has not the power to empty itself, or to force the fragments into the urethra...... These cases are not rare, and I have not wanted opportunities of trying the different means which art places at our disposal in regulating or assisting the expulsion of fragments, and of modifying them as necessity required." (p. 222.)

The best known of these means is the catheter. M. Civiale has tried various modifications of straight and curved, elastic and metallic catheters, and, after all, concludes that a large ordinary metallic catheter, with thin walls and very large eyes, is the instrument to be preferred. Various species of stylets have been added by different surgeons for the purpose of breaking up any fragments which might become impacted in the catheter, but one of ordinary whalebone answers every useful purpose.

In ordinary practice, when the sitting of lithotrity is terminated, the patient is to sit up before the operator on the edge of the bed, and the catheter is introduced; but, if there be engorgement of the prostate, it is better to do this while the patient is still lying on the bed. When the catheter reaches the bladder, the liquid injected before the operation escapes, and with it a greater or less quantity of fragments. An injection is practised, and, while this escapes, the syringe is charged for a second. Three or four injections of tepid water are thus made, one after the other, stopping as soon as the liquid returns colourless and limpid, or when the patient is fatigued. If a fragment remain in the canal of the catheter, it is better to withdraw the instrument and clear it, than to crush the fragment by stylets. In some few cases, a fragment may become impacted in the eye of the catheter and project, its presence being indicated by impediment to the injection and to the withdrawal of the instrument. It may be cleared by a fresh injection, or by pushing the fragment back into the bladder with a whalebone stylet.

We have described this as the ordinary practice—it was so in the earlier part of M. Civiale's career; but latterly he has not adopted it generally in simple cases, the passage of the fragments by the urethra not being painful. But in most children, in adults whose bladders retain great contractile power, when the neck of this organ is dilatable, and large portions of stone have been crushed, and, on the other hand, when the bladder has partially or entirely lost its expulsive power, these injections are absolutely necessary.

The method of detecting fragments in the bladder, and of determining if the cure be completed by their total evacuation, is not the least important duty of the lithotritist. Many persons have died from the irritating effects of remaining fragments, or have suffered relapse from the same cause, in both cases bringing discredit upon the operation,—discredit which should have been reflected upon the surgeon only. Ordinary sounding does not suffice to determine if the cure be perfect or not: large stones may thus escape notice; how much more likely then are small fragments to remain undetected. M. Civiale makes his explorations in a much more efficacious manner. The patient lying down, a catheter is passed, and the bladder is explored while the urine is escaping, several injections being afterwards practised, one after the other, with tepid water if the bladder contract strongly, with cold if it be slightly contractile. Each time while the fluid is escaping, researches are made in every direction. At last the bladder, stimulated by the instrument and the repeated introduction of water, contracts forcibly so as to compress the catheter, which is then moved with difficulty, and not without pain. At this moment the space to explore is so reduced that, by gentle and repeated movements, a practised hand can very generally detect the smallest particles of stone. If any doubt should still remain, one of the short broad-beaked lithotrites should be introduced, after injecting a quantity of liquid only just sufficient to prevent the branches from injuring the mucous membrane of the bladder; and exploration should be made with the branches more or less separated. When the urine is charged with mucus, exploration should not be made until, after repeated injections, the fluid escapes perfectly clear. By proceeding in this manner, the complete liberation of the bladder can be determined upon with all desirable precision.

A long article follows on the extraction of foreign bodies accidentally introduced into the bladder, in which M. Civiale points out the great advantages offered to the surgeon by the use of the lithontriptic instruments. He narrates some cases of interest, but as they do not bear directly upon our present object, we pass to the succeeding section, *on lithotrity in the female.*

M. Civiale states that statistics prove that lithotrity is rather more successful in the female than in the male, and that, if we except certain unfavorable conditions of the organs, success is certain. As soon as the instrument has cleared the meatus, it passes the urethra much more easily than in the male, and in the female the spontaneous expulsion or artificial extraction of the fragments are much more easy. The urethra being shorter, broader, more flexible and elastic, larger bodies traverse it, and with less pain; less reaction is excited, and the shortness of the canal allows of great precision in the manœuvres within the bladder. In England, dilatation of the urethra has superseded most other methods, but M. Civiale says, that although rational before the invention of lithotrity, it can be so no longer. In our opinion the general question still remains open; but one or other method should be adopted, according to the circumstances of individual cases. Lithotomy is successful in females, as far as life is concerned; but the frequent occurrence of vesico-vaginal fistula after its performance must lead to its general abandonment. If lithotrity be practised, the same general rules and precautions must be observed as in the male subject. The only difficulties likely to be met with are an irritable contracted state of the meatus, and a sort of double pouch formed in the bladder by the projection of the uterus upon its postero-inferior surface. When the vesical walls are lax, a pouch is very likely to be found in the vagina, behind the internal orifice of the urethra, in which small calculi or fragments are apt to lodge. Should we be applied to in the last period of pregnancy, or during labour, if the stone could be immediately and completely crushed, and nothing forbids, lithotrity should be performed; but if the size of the stone or other circumstances would render several sittings necessary, the case must be left to the accoucheur to keep back the stone, so as to allow a free passage to the child, or the stone must be at once removed by incision.

Lithotrity in children is very seldom practised in England, lithotomy being generally preferred. It is thought that the small size of the urethra prevents the passage of instruments of sufficient diameter to ensure their stability, and impede the escape of fragments, and that children are more subject than adults to the harder species of calculi. Dr. Prout's statistical researches have proved the partial incorrectness of the latter argument, and experience is daily showing that other difficulties have been overrated. We ourselves have operated successfully upon two boys between 2 and 3 years of age, one between 3 and 4, one but little more than 4, one about 7, and a girl 9 years old. All these cases terminated successfully. In one of the two youngest, the calculus was mulberry, and about the size of a bean. It was crushed in one sitting, and not a bad symptom followed. In the other the calculus was phosphatic upon a base of lithic acid. It was about the size of an almond (with the shell), and ten sittings were required, but the cure was complete on the 30th day. In the case of the boy in his 4th year, the calculus was mulberry, and scarcely so large as the last. Three sittings were required on alternate days, and after the third the cure was complete. In the next in point of age, the calculus was also mulberry, and about the same size. Four sittings, at different periods within 20 days, completed the cure. In the boy aged 9, a calculus of lithate of ammonia, larger than a bean, was crushed in two sittings on alternate days. In the girl a lithic-acid and lithate of ammonia calculus, of the size of a large walnut, was complicated by a deranged state of the general health, hectic, and catarrh of the bladder, the symptoms of stone having been observed upwards of four years. Yet in 24 days, after six sittings, the bladder was completely liberated. The only accident was the detention of two large fragments at the meatus after the first sitting, and the child has since continued in good health. We have spoken of these cases because we are convinced that the difficulties and dangers of lithotrity in children have been much exaggerated. After the first sitting they have generally very little fear of the instruments, certainly not more than of a common sound. We one day found one of our little patients playing with his brothers on the bed, imitating our proceedings with a long straw, and addressing his patient after our fashion, all parties evidently regarding " playing the doctor " as an excellent joke, by no means connected with painful associations. In 1827 M. Civiale operated upon a boy, aged 7, who had a very large stone, and with complete success. He has since repeated his method upon many much younger, in some cases having been obliged to dilate the meatus.

The size and curvature of the urethra in children, although it has not the immediate bearing upon the performance of the operation which the opponents of lithotrity would urge, must still be remembered ; a greater number of sittings, as a general rule, being required to crush a stone of any given size, than in the adult. The neck of the bladder also being more dilatable, large fragments are too apt to pass into the urethra, and become arrested there. These are the real objections to lithotrity in children ; how far they may tend to render lithotomy the more successful operation, future experience, honestly recorded, can alone determine.

With regard to the performance of the operation, the cries and struggles of the child at the first sitting are the only real difficulties to be apprehended—probably either may be successfully employed to obviate these. Some surgeons have bound their young patients, but it only serves to frighten and fatigue them, causing sobbing, straining, and involuntary movements of the diaphragm and abdominal muscles. The best plan is to lay the child upon a mattress on a table, and to have a couple of assistants who keep his legs out of the way of the surgeon, and, above all, take care that he does not spring up into a sitting posture. By a mild quiet manner even these assistants may often be dispensed with, after the first sitting or two has shown the child how little pain he has to fear.

We now pass to the sixth chapter of M. Civiale's work, *On the accidents* *of lithotrity*. Some of these are real, and depend upon the method itself more or less directly; others are assumed or imaginary.

> "Among the first we must distinguish—1st. Those which are inherent in the method, and which have been observed even when it has been applied with all necessary circumspection. 2. Those which more particularly depend upon one or other proceeding which has been adopted. 3. Those which are produced by the surgeon, or which depend upon faults committed during the operation...... Under the head of imaginary accidents may be classed a number which have no real existence, or which depend on other causes than the operation." (pp. 274–5.)

Some remarks upon the most frequent and important of these accidents, under their respective heads, will probably prove of use to our practical readers; and first, we find in M. Civiale's list—

Pain; this, in simple cases, when the surgeon is dexterous, is little more than that which attends ordinary catheterism; and when we read accounts of acute and prolonged pain, the fault invariably rests with the operator. At the first sitting, however, patients are very apt to exaggerate their sensations, and cry out at the least touch. We remember one of our early patients, an Italian gentleman, who cried out so lustily at the first sitting that we became alarmed, and stopped with the instrument in the bladder to ask him what he felt, when, to our surprise, he answered, "Niente, niente, continuate; lasciatemi gridare, è uno sfogo." "Nothing, nothing, go on; let me cry out, it's a relief." He afterwards said that he had suffered much more in previous soundings than during the few minutes occupied at this sitting. The pain, which is the immediate consequence of the operation, if the latter be well done, is in the great majority of cases very slight; but pain may come on afterwards, and, as in the former case, depend in most cases upon the operator's want of skill. Straining with pain may come on after the best performance, but this soon yields to a hip-bath, a poultice of bran to the abdomen, or an opiate enema; the passage of the fragments is seldom attended with anything more than a sensation of heat, and we have often noticed the curious fact, that patients complain less of a tolerably large angular fragment, than of the gravelly powder we are so anxious to form. It is only in cases of severe organic lesions, or of very large stone, that severe pain is likely to follow lithotrity, *if carefully and properly performed.*

2. *Fever.* All surgeons know that an attack of fever, having much analogy with an intermittent, is apt to follow simple catheterism or the introduction of a bougie. The same thing is sometimes observed after lithotrity, and the longer the sitting, the more likely is it to occur, and particularly after the first sitting. It is generally of no consequence, and goes off without the assistance of medicine in a few hours.

> "When the cold fit is short, and the succeeding perspiration abundant, and the necessary precautions are taken in changing the linen of the patient, the shivering does not return the next day, and the patient remains as well as usual, only with a white tongue, and pustules are afterwards developed around his lips. But when the shivering has been violent and prolonged, the heat uncomfortable, and the perspiration insufficient, the cold fit reappears the next day and perhaps the day after also. Still it is of no great consequence, and in general nothing else is necessary than to excite a copious perspiration, which terminates the fever." (p. 289.)
>
> "Some cases slight in appearance are serious in reality. The febrile attack is not frank or regular; the shivering is slight, does not last, and perspiration does not

follow; the shivering returns in a few minutes. I have several times observed these irregular shivers, followed by heat, but without abundant perspiration. The attack generally lasts some days, during which there is uneasiness, depression, loss of appetite, disturbed sleep, debility, and rapid emaciation, even when the local symptoms are slight and evanescent, and when the operation, after which these general disorders have occurred, has been performed with the greatest care. In several of these cases I have been unable to discover any organic lesion which could explain these phenomena." (p. 289.)

In some cases this urinary fever has a more typhoid character. Again, in another series, the fever does not commence by a rigor, but the patient feels a continued uncomfortable heat which does not terminate in perspiration. These are the most serious cases of all, as the fever generally depends upon disease of the kidneys or other vital organs.

When regular intermissions continue, M. Civiale considers the sulphate of quinine of great service. In the other forms he does not seem to place much reliance on any medical treatment. We can say nothing on the matter from our own experience; but in typhoid urinary fever following perineal abscess, we have several times thought we have seen great benefit derived from free doses of Dover's powder in hot port-wine negus.

3. *Dysuria, retention of urine.* Sometimes the spasmodic condition of the neck of the bladder, which has been noticed as a source of difficulty in the operation of lithotrity, is only manifested after its performance and under the influence of the manœuvre. This is apt to occur in nervous subjects, and to cause retention of urine. Atony leads to the same result. Whenever after lithotrity and a hip-bath a patient cannot evacuate his bladder, the catheter should always be passed, or retention is sure to come on, with local and general disorders, which are attributed to the operation, but which are in reality owing to the neglect of this simple precaution.

4. *Discharge of blood, pure or mixed with urine*, may follow the passage of lithontriptic instruments, although no injury to the mucous membrane has been inflicted, simple friction being sufficient in some patients to excite a sanguineous exhalation from the mucous surface. If the instruments have been gently used, this exhalation ceases in a few minutes, and is rather a good than a bad symptom, often effecting a salutary evacuation of engorged vessels. True hematuria, when it occurs, generally depends on over-distension of the bladder with urine after the operation, or on efforts which the patient makes to void it during the performance. In such cases a catheter must be passed several times daily, or left in permanence. M. Civiale has never seen bleeding sufficiently abundant to excite alarm, or call for further means of suppression, and believes that when this has occurred in the practice of others it was the fault of the operator.

5. *Cystitis* is a much less frequent sequence of lithotrity than is generally supposed; the pain, straining, and difficulty of evacuating the urine, with general agitation and disturbance, which are often regarded as symptoms of cystitis, being simple results of the presence of stone and of the contraction of the vesical walls upon it. The local symptoms in both cases are much the same, but the general differ, as in one they may last with intervals of diminution or suspension for weeks or months, in the other progressive increase is observed, and the patient dies in a few days. All the cases M. Civiale has seen have perished. He has only seen it follow

lithotrity in cases of prostatic tumour or fungus; but he has also seen it in many cases of persons upon whom no operation has been performed, because their disease appeared to be too far advanced when they applied for assistance. Lithotomy alone can offer any chance of success, as the presence of the foreign body is the cause of the inflammation, and of the inutility of all remedial measures.

6. *Abscess in the substance of the vesical walls, or in the neighbouring cellular tissue*, is sometimes met with. M. Civiale quotes two cases in which the matter was collected in the former situation. In one it was discovered while performing the high operation of lithotomy, in the other, after the first sitting of lithotrity, an abscess pointed in the hypogastrium and was opened. Much pus was discharged, but the wound was closed in a fortnight, and the result was quite satisfactory.

7. *Peritonitis* is not observed after the skilful performance of lithotrity in proper cases. When it does occur, M. Civiale lays the blame upon the operator.

8. *Ulcerations of the bladder* are found after death, in the bladder of calculous patients who have never undergone any operation, or who have been subjected to lithotomy, and also after lithotrity, when the surgeon has pinched and bruised the mucous membrane of the bladder. Lithotrity has been often blamed for such appearances, when they were solely attributable to the long continuance of the disease, or to the carelessness of the surgeon.

9. *Eruptive spots, articular pains*, are the most marked of a series of general disorders which have a close relation with diseases of the genito-urinary organs, a relation which has only been observed of late years, and has not yet been fully investigated, but which will probably form an interesting chapter in some future treatise on purulent absorption.

10. *Various accidents* may occur during the treatment of a patient by lithotrity, which may render its application for a time painful and difficult, or may lead to a suspension. 1. Infiltration of the prepuce may prevent exposure of the glans, and alarm the patient, but is of no consequence. 2. Inflammatory engorgement of the glans, with slight urethral discharge, is sometimes observed in persons whose meatus is small. A few days' repose, fomentations, or a poultice, is all that is required in the way of treatment. 3. Orchitis is one of the most common sequences of lithotrity. It is in general slight, and only leads to a suspension of the treatment for a few days. It is sometimes observed during the preparatory treatment by bougies, and in cases of organic disease of the prostate or neck of the bladder is apt to prove very obstinate and troublesome. 4. Disease of the thoracic viscera or brain may be simple coincidences, and still lead to the abandonment of lithotrity. 5. An attack of gravel, "coliques néphrétiques," is rare when a stone exists, but is occasionally observed, and may lead to a suspension of treatment. This must not be confounded with the lumbar pains, which are sympathetic with affections of the bladder or rectum, and disappear with the principal disease.

11. *Fracture or bending of the instruments* has never occurred to M. Civiale in all his long practice. He says that every accident of this nature is owing to the surgeon having employed defective instruments, not having known how to use those which he employed, or having neglected his first duty, that of proving their strength on a table before using them. He

says that he has broken in his trials several apparently good instruments, which the makers assured him had been proved. We have already said that the same thing once happened to ourselves, the maker being a London cutler of some reputation. M. Civiale quotes nine cases from the journals, in which such accidents have happened to various surgeons; the practical inferences are that the fracture may be complete or incomplete; in the former case the broken part falling into the bladder may be extracted by the natural passage or by lithotomy; in the latter the broken portion is displaced only, and remains adherent at an angle with the rest of the instrument which prevents its withdrawal. Should such a case present itself, the question is, what should be done? Mr. Liston has advised pressing back the penis as far as possible, cutting the instrument through close to the glans, then performing the lateral operation of lithotomy, and turning the curved end through the perineal opening. M. Civiale, on the contrary, recommends the hypogastric section. This has been had recourse to, but the mistake was made of first sawing the instrument at the meatus. An important guide in the performance of the operation is thus lost, and the proceeding may be unnecessary, for the deformity of the instrument may be such as can be easily redressed after the bladder is opened, so that the instrument can then be withdrawn by the urethra.

12. *Violence exercised in the urethra or neck of the bladder* has been noticed in the introduction of instruments, and in the withdrawal of others bent or broken, or charged with calculous fragments; and when a fixed handle has been used, the involuntary movements of the patient have injured the bladder. All this is the fault of the surgeon, not of the method.

13. *Pinching or perforation of the bladder* must be the fault of the operator. It is scarcely possible to seize the walls of the bladder if the stone be of moderate size, and the walls are properly distended by injection; but in cases of tumours, excrescences, or sacculated state of the bladder, a fold or projection may pass between the branches of the instrument. But a careful surgeon would be on his guard in such cases, would close the branches gently, and would readily feel if a soft yielding body, or a hard inorganic substance, were between them. A clot, or mucus mixed with calculous fragments, might give rise to doubt; but the sensations of the patient and the freedom of motion of the instrument would remove all uncertainty, great caution being used until this was the case.

14. *Serious and inexplicable accidents*, sudden death among the number, have followed the application of lithotrity, as they have the simple introduction of a bougie or any other surgical operation; the relation between apparent cause and effect not being traceable.

We have now arived at the last practical chapter of this important work. It is entitled "On the Arrest of Fragments in the Urethra, and on Urethral Lithotrity."

Experience has shown that the immediate local effects of the arrest of a foreign body in the urethra are most frequently observed in its membranous and prostatic portions, and that they often consist of dilatation, destruction of more or less of the tissues, infiltration of urine, and diffuse inflammation of the surrounding cellular tissue. The general symptoms are febrile, of an intermittent or typhoid character. But experience has also proved that a collection of fragments may take place and dilate the urethra without producing uneasiness or injurious results, their presence not being even suspected; while, in other persons, so much pain and irritation are excited by a small fragment, that the measures for extracting or crushing it can scarcely be supported. When a dull, deep, vague pain, with extreme uneasiness, anxiety, fever, sleeplessness, great physical and moral agitation, come on, the condition is probably owing to the arrest of a fragment in the deep part of the urethra. Many patients have been lost because surgeons have overlooked this fact, and have not removed the cause of the evils. The sensations of the patient do not lead to suspicion of the true nature of the case, and exploration must be made with a large bougie of soft wax, and with the finger in the rectum.

A great variety of useless instruments have been contrived for the purpose of returning, extracting, or crushing calculous fragments in the urethra; the grand point is to prevent the occurrence of the accident. We have already noticed the different circumstances under which it is to be feared, and pointed out that, by the use of a large catheter and injections in some cases, and, in others, by the permanent use of an elastic catheter, our object is generally gained. It is also a useful precaution to desire the patient only to void his urine when lying down, so that much calculous matter is not carried at once into the urethra. These means, however, are not always successful, and we have to follow M. Civiale in his account of the methods he has found most useful when the accident has occurred. He distinguishes several series of cases.

1. *Arrest of fragments in the navicular fossa* is not uncommon, as the external orifice of the urethra is one of the narrowest and least extensible points of the canal. Obstruction to the passage, frequent desire to urinate, irritation of the orifice, hardness and tumefaction on the glans, are the results. Here, as in cases where it is necessary to introduce large instruments, the orifice may be enlarged by incision. M. Civiale always uses an instrument for this purpose, which he calls *urétrotome*. It is simply a sheathed bistoury, which is introduced, opened, and withdrawn, cutting its way out, and can only be opened so far as to form a passage which will admit a bougie three lines in diameter. If the fragment be then too large to pass, it must be crushed with a strong pair of forceps. A small curette is sometimes useful. If the fragment should have become impacted in the walls of the canal, incision is necessary for its removal.

2. *Fragments arrested in the moveable portion of the urethra* are often so difficult of extraction, that many surgeons have resorted to incision for their removal. But this often gives rise to troublesome fistulæ, and M. Civialé has modified the urethral forceps of Hunter, so that the elastic branches, when projected from their sheath, do not present the form of the letter V and allow the fragments to escape, but approach each other more closely at their extremities than at the centre, and completely encage anything they have seized. He has also added a central stylet to enable us more readily to recognise the fragment between the branches, without the necessity of closing the latter, or to push it backwards, if it be too large for extraction and too hard to be crushed by pressure of the branches. In using this instrument, the urethra must be kept on the stretch by an assistant; the branches opened when their extremities reach the fragment, by withdrawing the shield; the left hand is applied to the urethra behind, and with the right slight rotatory movements are given, so that the

branches are insinuated between the fragment and the internal surface of the canal. This plan generally succeeds; in other cases a curette, seven inches long, flattened and curved at one extremity like a hook, will be useful. By stretching the canal, and preventing the return of the fragment by a finger of the left hand, the curved portion can often be passed behind, and the foreign body extracted. M. Leroy has contrived a joint curette, which is introduced straight, and the extremity then curved by a very simple contrivance. But either of these latter instruments is objectionable, as the fragment is apt to be dragged against one or other wall of the canal. M. Civiale has also constructed a small urethral lithotrite on the same plan as those for the bladder, but the curved portion is very short. It is opened when it reaches the fragment, and the female branch is then passed behind in the same way as the curette. When once this is effected, the foreign body is readily crushed. In our own practice we have found this instrument and the branched forceps very useful, indeed all that was required; and have never had occasion to use the curette.

3. *Arrest of fragments in the bulbous portion of the urethra* is rare, and extraction easy with the branched forceps slightly curved.

4. *Arrest of fragments in the membranous and prostatic portions of the urethra* is more common than in the bulbous portion. They may be either pushed back into the bladder, extracted, or crushed. The pushing back should be first attempted; extraction or crushing not being thought of, unless it is very difficult to return the fragment into the bladder. Very often it will return of itself, as it were, before a large soft bougie. If it be large, angular, and advanced in the membranous portion, the bougie may or may not push it back; if not, it will still show the form and size by the impression, and thus serve as a useful guide; and a large catheter may be passed, and several injections forcibly practised, pushing on the catheter at the same time. The latter method will frequently and easily succeed without pain, especially if the fragments are near the neck of the bladder; but, in some cases, when they are in the membranous portion of the urethra, all efforts at repulsion fail, and it is necessary to extract or crush by the proceedings before described.

In some cases fragments, or a quantity of gravel, may collect behind a spasmodic stricture, or an organic stricture, which had not been sufficiently dilated before the operation. Under such circumstances, incision would probably be required; but it would be much better to prevent the occurrence, by leaving a flexible catheter permanently in the canal whenever it was to be feared.

We have now terminated our analysis of the operative portion of M. Civiale's work, and shall conclude with a few remarks on the relapse of calculous affections after lithotrity. M. Civiale has devoted a chapter to the exposure of errors on this important part of his subject. He argues that relapse from fragments having been left in the bladder is solely the fault of the surgeon, and ought not to be considered an imperfection in the method; and that, although a new collection of calculous matter may take place in the bladder after lithotrity, if the general state of the system remain unaltered, this occurs more often after lithotomy than after lithotrity. He gives a table of 26 cases in which relapse occurred. In 12 of them the second calculi were of the same nature as the first, lithic acid predominating; the relapse not taking place until after some years. In the other cases, circumstances favoured the formation of the phosphates, and here these salts were deposited with surprising rapidity. The more rapidly the new stone forms, the softer and more friable it is, crumbling under the slightest action of the instrument, so that the patient may be relieved in a single sitting. In three cases relapses occurred after lithotomy, and the new stone was crushed. Lithotomy, like lithotrity, only removes the product of a morbid condition of the stomach or urinary organs; it is upon this morbid condition that relapse depends, and by its correction that relapse is to be prevented. If the general health of the patient be neglected after either operation, a second calculus will be apt to form, and M. Civiale's statistical researches would show that this has occurred more frequently after the old than after the new operation.

We here take leave of our author, as the remainder of his work, although interesting to those who are anxious to trace the history of lithotrity from its origin through its subsequent development, is so full of repetitions, personalities, and disputes as to the invention of various instrumental modifications, which have been long since settled by committees of the Academy of Medicine, that we can only wish, for M. Civiale's credit, that it had been left unpublished. His claims upon the gratitude of mankind are not disputed, and yet he wastes his energies in personal quarrels about trivialities. Indeed his history of lithotrity has given us a truly painful view of the quarrels, jealousies, and slandering of our Parisian brethren; and afforded us the consoling assurance that, although such vices are not unknown in our own metropolis, the tone of professional feeling must be generally far higher and more friendly than in the French capital.

Our readers may form their own opinion of the value of the practical chapters of the work from the analysis we have given of it; as we believe that no point of the least importance has been left unnoticed, although extreme condensation has been required on account of the remarkably diffuse style of M. Civiale, and of his very frequent repetitions. The best thanks of the profession are due to him for this record of the results of his observation.

ART. VII.

Mémoires de la Société de Chirurgie de Paris. Tom I. Fascicules I et II.—*Paris*, 1847.

Memoirs of the Society of Surgery of Paris. Parts I and II.—*Paris*, 1847. 4to, pp. 239.

THIS volume of Memoirs, or as we should term them, Transactions, emanates from a new Surgical Society which has been recently formed in Paris, and which already numbers among its members many of the most eminent hospital surgeons of that capital. For this class of practitioners it is indeed especially designed, enabling them, by means of weekly meetings, to bring under the notice of their professional brethren any important facts they may have had the opportunity of observing, and printing such of these as may seem worthy of a permanent record. This publication at first took place in the pages of the 'Archives Générales;' but the

Society now feels itself strong enough to support a separate issue. That much valuable information will in this way be diffused, there can be no doubt; and we have only a word of objection to offer to one of the arguments employed in favour of the foundation of the Society, contained in the following passage of the preliminary address: "Appreciating the advantages of the division of labour, we saw with regret that there did not exist in Paris a society which occupied itself solely with the important branch of the art of healing to which we have devoted ourselves. The fusion of the various sections of the Royal Academy of Medicine has, in point of fact, deprived surgeons of the only special means of meeting together which they have possessed since the suppression of the Academy of Surgery." Without offering any opinion as to how far, under present arrangements, the interests of surgery are duly represented at the Academy of Medicine, we may state that we deem the reason here adduced for its isolation as generally fallacious. However unavoidable, and indeed, in a large capital, desirable, a division of labour may be in its application to the practice of surgery, there is nothing to be gained by adopting this for its scientific cultivation. On the contrary, the experience of our own country proves that, just as surgeons have eschewed such isolation, and have imbued themselves with the general principles of medicine, so have they raised the character and improved the practice of their branch of the art of healing. All our own medical societies, and especially the leading one, in its very title of Medico-Chirurgical, acknowledge the necessity of this indivisibility in the pursuit of medical science; and we feel convinced that their continuing to do so, can only tend to the advantage of one and all of its departments.

Of the truth of this position, the character of the very first paper affords a most apt illustration; for its subject is fully as much medical as surgical, using these terms in their ordinary acceptation. This paper, from the pen of M. Cullerier, is thus entitled: "*Upon some of the ill effects which result from the use of the preparations of iodine; and especially functional and organic lesions of the testes and mammæ.*" The writer truly observes that incautious generalization as to the employment of powerful medicines frequently gives rise to much mischief, and leads, in the end, to their becoming unjustly discredited. Iodine so employed has operated injuriously in many cases, which have not been hitherto brought under professional notice with sufficient distinctness. M. Cullerier, on the present occasion, contents himself with simply adverting to the fact, that this drug sometimes produces various forms of *cutaneous eruptions*, and a species of *salivation*, more akin to that of pregnant women than to mercurial salivation. Occasionally the *nervous system* suffers from its use; symptoms, in some cases, analogous to those caused by alcoholic intoxication, and in others resembling those of hysteria, being induced; and it is a remarkable fact that, when iodine acts thus injuriously upon the brain, the ill effects it induces disappear in less than 24 hours after its suspension; while, when it injuriously affects the glands, these may persist for months after its discontinuance.

Very contradictory opinions have prevailed respecting the effects produced by iodine. Thus, while M. Ricord and others have noted salivation as one of the occasional consequences of its employment, other practitioners have recommended it as a means of checking this effect when induced by mercury. M. Cullerier has employed it for this purpose both locally and generally, but in vain; and when iodine and mercury have been given simultaneously, it has exerted no preservative effect of this kind. Again, iodine is well known to exert an action, not only upon the glands, but upon fatty matter; and yet all must acknowledge the justice of M. Ricord's observation, that persons may increase in *embonpoint* during its administration. This seeming contradiction is, however, easily explained.

> "Suppose iodine were given for a certain period to individuals for the sole objects of studying its action upon the economy, some of these, perhaps, would experience no effects; but others would very certainly suffer from the atrophying influence exerted by the drug upon the glandular and adipose systems. But, on the other hand, let the same substance be given in similar doses to a like number of syphilitic or scrofulous patients, and especially those labouring under that form of syphilis (the tertiary) for which iodine is so very applicable; in such cases the substance will act as a therapeutic agent, neutralizing a morbid principle, and ridding the economy of it, and permitting the vital powers to resume their activity, the consequences of which are a re-establishment of nutrition, and a recovery of flesh." (p. 8.)

The chief object of M. Cullerier's paper, is to direct attention to the effects of iodine upon the testes and mammæ; effects which have been, on the one hand, much exaggerated, and, on the other, erroneously denied. The first case that offered itself to his notice occurred about ten years since, in the person of a young man, to whom moderate doses of Tr. Iodin. were given for the cure of an obstinate gonorrhœa. This it effected, but the testicles were found to have become diminished in size and consistency, while the patient no longer possessed venereal power, and suffered the greatest depression of spirits. Under the use of tonics and a variety of stimuli, the generative organs imperfectly regained their power. Since the occurrence of this case, M. Cullerier has always taken the precaution of inquiring concerning the state of the generative organs, after iodine has been for some time administered. In another case it was given for a slight urethral discharge, appearing only in the morning; and, although the testicles appeared normal, the power of erection became much enfeebled, until the iodine was left off, and tonics substituted. A medical man seized with a *syphilomania* about the time when iodine came into vogue, administered large quantities to himself, for six months in succession, when he perceived a great diminution in the size and power of the testicles.

> "These facts come in proof of the proposition which I just now laid down, that iodine seems to exert a more direct action upon the glands when it is given to an individual in health, than when the constitution is deteriorated; and among a considerable number of patients, really syphilitic, to whom iodine is administered, how few are there who even present a slight and very transient alteration in the functions of the testis.
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> "That the action of iodine upon the testis has not been oftener noticed, is because, in a great number of instances, it has not been appreciated, inasmuch as patients do not always observe the organic changes which occur in these parts, and frequently refer to some other cause the functional debility that ensues. But when it is upon the breast that this injurious influence is exerted, it rarely escapes notice, since women are almost always alive to the slightest modification this organ undergoes." (p. 12.)

The utility of iodine, in the removal of a hypertrophied state of the breast, has been recognised by several practitioners; but if only one breast is affected, the other may become atrophied during the treatment. A case is related of an hypertrophy resulting from chronic inflammation of the mamma, occurring in a woman of lymphatic temperament, not yielding to the usual resolvents, and treated by means of iodine, administered both internally and externally. In a month's time a very great improvement had taken place; but just in proportion as the hypertrophy had diminished, so had the healthy gland on the other side wasted away; and when the woman left the hospital, three weeks after the suspension of the iodine, the breast continued diminished by one half. Six months afterwards it was resuming its normal condition. Another case is given, as showing the greater susceptibility of the breasts than the testes; since, although the medicine was given for an infected state of the economy, it still operated injuriously on these organs. A young woman, suffering from suppurating syphilitic nodes, had administered to her, after a course of corrosive sublimate, the iodide of potassium in doses of at first 15, and then 30 grs. per diem. The patient in a week perceived some difference in her breasts; but as this was not appreciable to her attendant, he ordered the medicine for another week. The patient felt indignant that her first remonstrance had not been attended to, inasmuch as the organs were now visibly and remarkably diminished in size; and they were long in recovering their normal proportions.

Two other cases are related, for the purpose of showing that, as in the case of testis, mere functional as well as organic lesion may take place. A woman, eight months advanced in pregnancy, was put upon a course of proto-iodide of mercury for the cure of syphilitic ulcerations of the tongue and vulva. This was continued for about a month; when she was delivered of a healthy infant. Six weeks after delivery the iodide was resumed, and, at the end of a week, she complained of a diminution of milk, which, as the medicine was continued for four or five days longer, went on increasing. Its use being then suspended, the milk in another week was as abundant as ever; but, being resumed in a fortnight, at the end of a week the same effects recurred. Corrosive sublimate was now substituted, and the woman did very well. A nursing mother had been treated with iodide of potassium, for the purpose of combating a hypertrophied state of the os uteri, during about six weeks, without any ill effect upon the breasts in general health; when, for the sake of more energetic action, the syrup of the proto-iodide of iron was substituted. The breasts soon began to decrease in size, but the supply of milk was especially diminished; and both the glands and their secretion resumed their normal characters upon the suspension of the syrup. Dr. Sperino likewise mentions a case in which a great diminution of milk took place, while the iodide of potassium was being taken. The knowledge of the existence of similar cases to these has doubtless led to the employment of this drug for the relief of excessive secretion from the mammary gland; and Dr. Riesemberg relates, in the 'Journal de Chirurgie,' 1844, a case of *galactirrhea* thus cured. A woman had suffered from this excessive secretion during eighteen weeks, in spite of various remedies, and had become emaciated and hectic, when Dr. Riesemberg prescribed iodine; twelve days afterwards, the menses appeared, and galactirrhea ceased. After her next confinement, the child was not even presented to the breast: and yet in fourteen days the same excessive flow of milk took place; iodine was again successful. These cases show the necessity of caution in prescribing this remedy, in the case of women giving suck.

"Such are the practical facts upon which I am desirous of fixing your attention; and I have brought them before you, because in books we find few complete histories of iodic accidents, and because I believe a knowledge of these results is very capable of throwing some light upon the real causes of certain affections, which have often been prejudged in consequence of the success of the treatment to which they have been subjected. Thus, when we observe to what an extent important organs, such as the breasts and testicles, may be influenced by iodine, we can easily understand that it may act much more energetically upon much smaller glands. In this way we explain the success which, in the hands of Bréra, Coindet, Richond, and others, has attended the treatment of gonorrhœa; often kept up as this is, by the inflammatory hypertrophy and excessive secretion of the prostate or mucous follicles; without considering it necessary to suppose the intervention of any constitutional derangement, which would explain the case as affected by general treatment.

"So, too, we can understand that, in some cases of chronic flux of the salivary glands, and of mercurial ptyalism, iodine may exert a good effect, although it fails in the acute stage. Thus, as regards this particular case, certain assertions which appear contradictory, really differ only through want of an appreciation of the exact period of the disease to which they apply; the general action of the iodine varying according to the condition of the economy." (p. 12.)

M. Huguier contributes a short paper upon what he terms "*Utero-follicular polypi*," so called in consequence of the resemblance which their anatomical and microscopical characters bear to those of the cervix uteri. "Just as in the liver, the breast, the salivary glands, thyroid gland &c., we occasionally see portions which become detached, and have no further connexion with the principal mass, than by means of a pedicle more or less narrow; so may a portion of the uterine substance become hypertrophied, project, and detach itself from the surface of the organ, forming a tumour which, in this case, is termed a polypus. It is probably in consequence of the great analogy which exists between the substance of these tumours and that of the uterus,—tumours which cannot be of rare occurrence,—that polypi have often been mistaken for the womb in a condition of prolapsus or inversion."

Thus, when Bauhin, in his appendix to the translation of Rousset on the 'Cæsarean Operation,' refers to nineteen cases of removal of the uterus, several of the women becoming subsequently pregnant, it is evident that a portion of diseased vagina, or a polypus of the above-mentioned organization, has been mistaken for the womb. Levret, Hoin, Collin, Laumonier, and others, have removed hollow polypi, which, from their great resemblance to the uterus, have been mistaken for it. The same error has been committed by eminent practitioners of our own epoch. Thus, Richerand and Cloquet having, in 1823, removed an enormous polypus, were only assured at the patient's death that this was not the uterus itself. A woman applied to M. Velpeau on account of a large conical tumour projecting from between the labia, and he, believing it to be the uterus, returned it into the vagina, and kept it up by a pessary. The woman dying of peritonitis, the tumour was found to be a polypus implanted within the uterus by means of a long pedicle. Doubtless, in many instances, the polypus in question has been mistaken for a diseased or misplaced uterus; and this may easily be the case for the following reasons: 1. This form of polypus is usually ovoid or conoid. 2. Its structure and consistence are like those of the womb. 3. It possesses a circular or fissural aperture, which may be taken for the os uteri. 4. From this aperture flows a thick, elastic, transparent mucus, very like that found within the cavity of the cervix uteri. 5. A flow of blood from this aperture, and from the surface of the tumour, may take place during the menstrual period. 6. It contains a more or less extensive cavity lined with a mucous membrane, into which a probe may be passed.—These characteristics lead to the opinion that this polypus is only formed at the neck of the uterus, that being the only portion of the organ possessed of muciparous follicles.

The most elaborate paper in the collection is a valuable monograph by M. Morell-Lavallée, upon "*Hernia of the lungs*." This subject has been hitherto only incidentally alluded to by writers upon wounds of the chest; and two cases having fallen under the author's notice, he determined, while reporting them, to endeavour to fill up the blank which existed, by a careful study of the phenomena they presented, and by the analysis of all the recorded examples of the affection he could meet with. In noticing the article, we cannot do better than follow the order he has pursued.

Causes.—In reference to etiology, we must bear in mind that there are several varieties of hernia of the lungs. These are—(1). *Congenital hernia.* Of this one example only is on record, and that observed only after death by Cruveilhier. In the body of an infant affected with spina bifida, he found the apex of the right lung extending as high up as the upper border of the larynx; and M. Morell-Lavallée has noted an undue elevation of the pleural sac, as if preliminary to the formation of a hernia in the cervical region. (2). *Traumatic hernia* is perhaps the least rare of all the forms, and generally results from the thrust of a sword or other cutting instrument; the wound having been large in all the cases in which its extent is specified. (3). *Consecutive hernia* may follow a fracture of the rib, or the perforation of the wall of the chest by an abscess, of which last, however, there is but one case on record. If a hernia does not appear at once after a wound of the chest, it may still thrust before it the enfeebled parietes, so as to raise up the cicatrix; the time required for this varying from a month to several years. (4). *Spontaneous hernia.* The chest differs from the abdomen in possessing no natural apertures at which its viscera may become extruded, except to some extent in the cervical region, where the large vessels pass out. The occurrence of hernia at that point, however, is of far rarer occurrence than at the intercostal spaces. In this latter situation we might attribute it to the existence of an abnormal debility of the parietes, either congenital or acquired, in cases of special emaciation of the chest, as in phthisis; but the occurrence may take place in athletic subjects, in whom the local debility must be referred to some other cause, or, which is less probable, to an excess of that power which strives against the resistance of the walls of the cavity. The author, in considering the mechanism of the production of the hernia, enters into a detailed examination of the actions concerned in ordinary respiration, and endeavours to prove, by numerous quotations, that the views entertained by Richerand, Cloquet, and others, upon this subject are erroneous. We have no space to follow him through the various steps of this inquiry, and shall content ourselves by transcribing his own view of the subject.

"During inspiration, the lung is only drawn outwards by the thoracic parietes, and so far from having any tendency to pass beyond these, the retractile effect of the bronchi would impart to it a contrary direction. In expiration, the walls of the chest close upon the lung, which, without separating itself from them, retreats before them without any outward pressure or resistance. As the lung thus only follows the parietes in their outward movement, and precedes them on their return inwards, and is submitted to little or no excentric pressure during the respiratory movements, there is nothing here favorable to the production of hernia; everything, on the contrary, seeming disposed for its prevention. It is by stopping at this point of view that excellent observers, not perceiving the cause of this affection, have been reduced to imagine for its explanation the existence of an emphysema; while it was only necessary to extend their view a little farther, for the discovery of the key of its mechanism in certain modes of expiration.

"In fact, although the lung in ordinary expiration retreats before the thoracic parietes, or is gently pressed back by them, this is not so in sudden and forcible expiration. In the first case, the air of the cells, gently displaced by the elastic retreat of the lung, flows out freely and slowly, without sensibly repelling the internal surface of the viscus. In the second case, when, by a powerful muscular effort, this stage of respiration is accomplished in a sudden manner, the air, compressed on all sides, and finding but an insufficient issue at the trachea, increases in resistance as it diminishes in volume, and reacts upon all points of the mucous surface, exactly as if the gaseous current were directed from above downwards into the vesicles. We may imagine this surface divided into a great number of discs, having the diameter of the trachea, each of which supports the same amount of pressure as if it were placed perpendicularly at the end of the tube, e. g. at the orifice of the larynx. This pressure soon overcomes the elasticity of the bronchi, and the parenchyma is driven outwards, with a force equal to the excess of the excentric resistance of the air contained in the lungs over the concentric resistance of the bronchi; and such excess is exhibited in all somewhat forcible expirations, being necessarily proportioned to their intensity. In this case, if there be a penetrating wound of the chest, the lung may easily become engaged within it. When there is no aperture, the viscus may press the parietes outwards; such pressure may easily suffice to produce a hernia at a point weakened by an abscess, a fractured rib, or a cicatrix; and, by repetition even upon the uninjured parietes, it may at last overcome the resistance of the least firm portion. It is the act of coughing which usually leads to this unfortunate result; the rarity of that of sneezing neutralising its effect. The act of coughing, in fact, unites the most dangerous conditions in reference to the production of this accident. By the first part of the movement, a large quantity of air is inspired; while by the second, not only is it driven out with vigour, but the canals which it has to traverse narrow themselves before it, in order that, by the rapidity and force of the current, it may the better sweep over the air-passages, and remove the products of secretion, &c. The extent to which the intercostal spaces are raised up during coughing, proves that it is not less to be dreaded for the energy of its action, than for its frequency. These two elements doubtless unite in the production of spontaneous and consecutive hernia; but one usually predominates over the other in such a degree, that the parietes appear sometimes to yield from progressive distension (repetition of the cause), and sometimes by sudden rupture (intensity of the cause). Authors scarcely admit any other than the first of these two varieties, most of them not even mentioning the second." (p. 95.)

An interesting case of spontaneous hernia, occurring in the person of an athletic officer of the French army in Spain, is quoted from Chaussier. He was seized, without any known cause, with a most distressing paroxysmal cough, accompanied by pain in the left hypochondrium. In a few days a tumour, of the size of an egg, appeared at the left side of the chest, enlarging during inspiration, diminishing during expiration, and disappearing under pressure. Ecchymosis showed itself over the whole of that side of the body. Rest, regimen, and the application of a compress, enabled him after a while to resume his duties; still feeling some uneasiness in his side, and being obliged to support it whenever he took much exercise. A year afterwards, a similar cough attacked him, and a hernial tumour exhibiting itself this time on the right side of the thorax, he obtained his discharge. Both tumours required, during coughing, a greater pressure than that which he habitually employed; and upon examination after their reduction, apertures could be felt, seemingly resulting from rupture of the intercostal muscles.

The most feeble portions of the parietes are of course those at which the hernia appears. Thus it has been observed at the neck, in consequence of the species of herniary canal that may be said to exist there; and especially at the anterior portions of the seventh, eighth, and ninth intercostal spaces, near the junction of the ribs with their cartilages—where the spaces are largest, and the muscles composed of but one layer, and this the thinnest. The adhesions which take place after wounds of the chest, between the lungs and the serous membrane, offer accidental obstacles to the production of hernia; and others depend upon the nature of the wound. When we consider that coughing has produced a herniary protrusion through the parietes of a strong man, we might feel surprise that the accident is not of more common occurrence after wounds of the chest. The patient, however, in such cases, takes every care to prevent coughing, from the pain which it causes; and the respiration, almost entirely diaphragmatic, is performed very gently. So, too, the patient instinctively supports the part with his hand, when forcible expiration becomes unavoidable. If the wound is oblique, the air effused does not escape from the pleura, and becomes an obstacle to the protrusion of the lung; while, if the wound is large, the pleura becomes completely filled with air both from within and without, which, during cough, must be expelled before the lung can be protruded. Moreover, the column of air which takes the direction of the wound, and which would force the lung towards the parietes, were these intact, becomes powerless, inasmuch as the lung is open, and allows of its issue into the pleura. Thus there are only two examples of traumatic hernia, with wound of this organ, on record.

Anatomical Characters.—Pneumocele is almost always seated at the anterior part of the chest, on a level with the breast. The herniary aperture has frequently been imperfectly noted; but it is in general found to be large, and in old spontaneous herniæ the ribs bounding it have usually possessed an abnormal mobility. If the pneumocele be slowly formed, it requires a sac at the expense of the pleura; and, even when the suddenness of its appearance prevents this, it may afterwards become invested by an adventitious serous membrane, a dissection of such a one being here quoted from Cruveilhier. The portion of lung protruded generally possesses all its integrity, although its congested state has not unfrequently given rise to a belief of its gangrenous condition, and has led to its removal in place of its return.

Symptoms.—Observers have nowhere detailed the symptoms of *traumatic* hernia apart from those of the wound inducing it; and those arising from *consecutive* and *spontaneous* hernia are so similar, as to admit of being considered together. The consecutive form usually commences gradually and without pain, while the spontaneous may occur more suddenly, and induce great pain at first; and during the progress of either form, the suffering is often very considerable. The volume of the consecutive hernia may vary, according to the nature of the preceding injury, from the size of a nut to a really enormous magnitude, as in the case related by M. Huguier, in which four ribs were the seat of double fracture; and in no case in which the affection has depended upon fracture of these bones, have less than two or three of them been injured. Spontaneous hernia, too, may be very small at first, but usually increases in size, especially under the influence of coughing. Sometimes little or nothing of the tumour is perceptible, except during forcible expiration, in which case it may be termed *intermittent;* as it may be termed *continuous*, when, although diminishing during inspiration, it never spontaneously diappears. An intermittent hernia may, however, become a continuous one.

In some cases, when the hernia is extended during sudden expiration, it produces a noise which may be heard at a distance, and which Plater has compared to that caused by a dry bladder; but the application of *auscultation*, properly so called, to this affection, M. Morell-Lavallée believes was first undertaken by himself. During the expiration of coughing, a small, intense, vesicular murmur is heard under the stethoscope, which much resembles the normal respiratory sound, but that it is stronger, being less softened and less veiled by passing through superjacent structures. It is almost a crepitant *râle*, or rather like the noise which would result from blowing into a pulmonary lobule just under the ear. Nothing is heard during inspiration, or during effort. In a case of cervical hernia, the phenomena are less distinct, being obscured by the extra-vesicular noise of the cough. Moreover, in this case, the lungs were the seat of spontaneous emphysema, a lesion which remarkably diminishes the intensity of the vesicular murmur; while the hernia was intermittent, the form least favorable for the development of the sound.

The hernia conveys an impulse to the hand in forced expiration; during which may be felt "a vesicular rustling of a gas traversing multiplied and small cells. It is just as if a pulmonary lobule were blown into, while under your fingers, conveying to them the sensation of the supple and spongy consistence of its tissue." When the respiration is moderate, *direct* pressure only gives to the fingers a sense of depression in the parietes of the chest, though to the eye a tumour may be apparent; but when this is directed *obliquely*, the passage of a number of fine air-bubbles can be perceived. When the hernia is not reducible, but has been apparently returned, a number of these fine *bullæ* can be always felt, and thus the error detected. In the *intermittent* form, the hernia being already filled with air on extrusion, the crepitation, which in the other form results from its entrance into the cells, is neither heard nor felt; and the tumour imparts much more of the sensation of an enterocele.

Diagnosis. Prior to the application of auscultation an error was easy of commission; as for instance, when a portion of the omentum presents itself between the sixth and seventh ribs, a case of which was authenticated by three professors at Montpellier in 1787. An interesting case is reported by J. Cloquet, occurring in the person of a man, who had much violence done to the upper part of the abdomen by the passage of a cannon over it. A tumour, the size of a nut, presented itself between the eighth and ninth ribs on the left side, at their junction with their cartilages. It was painful and irreducible, and, during exertion, would increase to the size of an egg, after which all the symptoms of strangulated hernia were sometimes produced. A non-reducible tumour containing fluid could not easily be mistaken for pneumocele, if it were seated over a solid surface; but when such tumour is seated on soft parts, which sink in during inspiration, and project during expiration, as at the supra-clavicular triangle, error may arise. An instructive case is detailed at length, in which M. Marchal mistook an abscess in this region for a pneumocele; chiefly because the patient declared it had appeared suddenly, and because a mucous *râle* could be heard, communicated through it from the chest. There was present, however, neither the characteristic crepitation nor vesicular murmur. But the abscess may be reducible; and Foubert relates a case of this kind, in which the abscess presented itself between the false ribs and the xyphoid cartilage, and was believed erroneously by several to be a hernia. The sense of fluctuation, together with dullness on percussion, would generally suffice to distinguish such a case; and if they did not, the absence of vesicular murmur would do so. When the reducible tumour results from a vomica emptying itself beneath the skin, so as to give rise to a collection of both liquid and gas, the distinction is less easy, and is chiefly derived from the *gargouillement* produced by the reduction of the tumour, and a smaller *gargouillement* from pressure of the fingers, and, during expiration, contrasting with the vesicular sound of pneumocele.

Prognosis. If we derived this from the consideration of the somewhat more than thirty cases now on record, it would not be very unfavorable; and as regards wounds of the chest it might even seem a favorable complication, since of two cases, in which the lung is represented to have been lacerated, dried up, or gangrenous, only one terminated unfavorably, in spite of irrational treatment. But such a conclusion would be an erroneous one; for not only in this, as in other diseases, have practitioners been far more ready to publish their success than their failure; but in this particular affection, many observers have recorded their cases as almost miraculous examples of cure, while the reverses have been neglected, as merely representing the ordinary course of events. Still, the prognosis of traumatic hernia cannot be regarded as very unfavorable; and of the complications of penetrating wounds of the chest, it is the least serious. Left to itself, gangrene performs the removal of the part; and under judicious management there is less danger still. Consecutive and spontaneous hernias, apparently of less serious account than the traumatic form, would probably often become of greater importance, by reason of the pain, dyspnœa, and cough they give rise to, did not art intervene for their cure. "There is at first something surprising in this favorable character of the prognosis of thoracic hernia, as compared with the severity of abdominal hernia. It is because the one is exempt from the accident which constitutes almost all the danger of the other—strangulation. Nothing analogous to this is met with, in either spontaneous or consecutive pneumocele; and we cannot designate the ill-defined congestion, which has been observed in the traumatic form, as strangulation. Is this exemption due to the slight susceptibility of the viscus, or to the extreme compressibility of its tissue?" Quoting the case related by Dr. Bell, in Duncan's 'Medical Commentaries,' vol. ii, of supposed strangulation, and removal of the protruded portion of the lung, the author expresses his conviction that the omentum has been mistaken for this.

Treatment. Although the first idea of a hernia should seem to relate to its reduction, yet authors have only directed their attention to the facility which the protruded part affords for removal. This has doubtless often arisen from the changed appearance of the part having given rise to the belief of its mortification. In one case only is the reduction of a *traumatic* hernia recorded; and this was successful, although the lung had been wounded, and much blood lost. It occurred in a child, to whom Dr. Angelo was called immediately after the accident. The return of the hernia may, however, be very difficult, requiring the enlargement of the original wound, which is better accomplished by a dilating instrument than by incision, the former being less likely to give rise to a consecutive hernia. The protruded part is not, as formerly, to be cut off to lessen the mass to be returned, but it should be submitted to a sustained compression; and, before having recourse to more active measures, the amount of dilatation obtainable by inspiration should be ascertained. The management of *consecutive* and *spontaneous* hernia is simple. Over the tumour, when reduced, a bandage is to be firmly applied, having a firm compress beneath it. Velpeau in this way obtained a radical cure in six days; but the amount of success will much depend upon the size of the aperture. *Spontaneous* hernia is the most difficult of cure; inasmuch as no means acts efficaciously against the inherent debility of the parietes which originally give rise to it. Compression of *cervical* hernia would not be prudent.*

The last article we shall notice, is an interesting paper contributed by M. Gosselin, upon "*Laceration of the lungs without fracture of the corresponding ribs.*" Its purport is thus stated:

"I wish to describe that species of solution of continuity which occurs to the lung, without any instrument having penetrated the thorax, and without the existence of a fracture, the fragments of which could have produced the lesion. Two facts of this kind came under my notice, in respect to which, as no autopsy took place, I was obliged, by the careful study of their symptoms alone, to determine the nature of the affection. I had at first some difficulty in conceiving such an occurrence; but having met with some recorded cases with autopsies, and being, moreover, certain of the care I had taken in my investigation, I believed I had not been led into error. If I am not mistaken, this subject is but little known. I have certainly here and there met with the accounts I have alluded to, as well as certain allegations quite destitute of proof; but I do not hesitate to say that these slight materials are in general unknown, or have become quite forgotten. It has, however, seemed to me that a lesion of the lung, the walls of the chest remaining untouched, is not a matter of indifference; and that it would be useful for the surgeon to be well acquainted with its possibility, its symptoms, and its issue. I have thought, in one word, that the details relating to this affection, required stating with more precision; and this is why I have collected all the published facts, and, by comparing them with those I had myself observed, have presented as complete a history of this form of laceration of the lungs, as the present state of science permits." (p. 202.)

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* As M. Morell-Lavallée's valuable paper contains the only medical history of this affection we are acquainted with, we subjoin his principal references: Rolandus, Chirurg., lib. iii, c. 25; Schenckius, Obs. Méd. t. ii, p. 290; Fabricii Hildani Op. Obs. Chirurg. Cent. Obs. 22, 1611; Loyseau, Obs. Méd. et Chir.. 1617; Rhodius, Obs. Méd., Padua, 1657; Felix Plater, Obs., p. 96, 1641; Tulpius. Obs. Méd., p. 194, Amst. 1672; Ruysch, Obs. Anat.-Chir., Amst. 1691, Obs. 53; Boerhaave, in De Haen Instit. Pathol., 1709, tom. i, 333; Bruns and Ricther, in Ricther's Chir. Bibl., t. iii, p. 138, 1774; Grateloup, Journ. de Vandermonde, tom. liii., p. 416, 1780; Bell, in Duncan's Med. Comm., vol. ii., 1785; Mercier, Journal Gén. de Méd., tom. xxxiv, 378, 1799; Voisin, Gazette Médicale, 1832; Sabatier. Méd. Opérat., 1810; Richerand, Phys. tom. ii, p. 42, 10 ed. 1833; Larrey, Mém. de Chir. Milit., t. iii, p. 91, 1812; Chaussier, Bulletin de la Faculté de Méd. de Paris, t. iv, p. 50, 1813; Cooper, Surg. Dict.; J. Cloquet, Nouveau Journ. de Méd., tom. vi, p. 308, 1819; Cruveilhier, Anat. Pathol., 1834; Angelo, Gaz. Med. di Milano, Feb. 1844.

M. Gosselin details the two cases which came under his notice at great length; but the following is an abstract of the essential particulars. A man, æt. 22, was brought to the hospital, having fallen from a window. He complained of great pain at the left side of the chest; but, on careful and repeated examination, no fracture could be detected. Next day hæmoptysis appeared, together with the general and local signs of traumatic pneumonia. But the source of this pneumonia seemed to be a laceration of the lung; inasmuch as there was *gargouillement* and a peculiar sound resembling metallic tinkling. So much did these sounds resemble those heard in tubercular excavation, that had the patient been auscultated without the antecedents being known, a tubercular cavity would have been declared to exist, the sounds being evidently produced by the passage of air into an excavation containing fluid. The second case occurred in the person of a child, æt. 12½, over the left side of whose chest a wheel had passed, leaving its mark there. No fracture was discoverable. The respiration was very much disturbed, the ribs on the affected side being nearly immoveable. Slight bloody expectoration occurred. Evident signs of pneumothorax presented themselves on that side of the chest; and opposite the supposed laceration, *gargouillement* and metallic tinkling were heard.

An attentive search of the periodicals has brought to the author's knowledge little that bears upon the subject, in consequence of the rarity with which the nature of the case has been sufficiently specified. Four cases alone are alluded to, as being such as reliance may be placed upon. In the 'Archives de Médecine,' t. xix, is given an account of a case furnished by a M. Roques, in which the patient is described as having fallen from a height, and died immediately. At the autópsy, a laceration of the lungs and a fracture of the ribs were discovered, but the one so far removed from the other, that it certainly could not have resulted from the projection of fragments of bone. Hewson published the following case, which has been repeatedly quoted, in the third vol. of the 'Med. Obs. and Inquiries.' A young man died after having fallen from a window. There was fracture of the cranium and of the first rib, with subcutaneous emphysema. A large quantity of air escaped from the cavity of the pleura, on a puncture being made. The lung was not torn opposite the seat of fracture, but at its base or concave portion, and consequently very far from the seat of fracture. At that part of the pleural cavity, some blood and air were effused; and insufflation at the trachea exhibited the laceration of the lung and pleura there. The emphysema was due to a small opening in the parietal pleura opposite the fracture. Smith, in the 'Dublin Journal' for 1840 (we have not been able to verify this reference), relates the case of a man who died almost immediately after being run over. The right side of the cavity of the chest was found to contain a large quantity of air, and three extensive lacerations of the lung existed without any fracture of the ribs. The same writer states that he has met with two other cases of laceration of the lungs without fracture.

A case of supposed laceration is detailed in M. Saussier's thesis on Pneumothorax (1841). A man, whose chest had been severely compressed between a post and the wheel of a carriage, was brought into the hospital, suffering under intense dyspnœa. No fracture of the ribs could be discovered; but in the left supra-scapular region a tumour, somewhat less than the fist, was observed. It felt soft to the touch, imparting the sensation of a fresh bladder partly filled with air, and could be made to disappear temporarily by pressure. To the ear it furnished a noisy respiratory sound, with a remarkable *sifflement*. On the right side of the chest there was great sonorousness, an enfeebled respiratory sound, with amphoric resonance and metallic tinkling, especially near the upper border of the liver. The following day the amphoric sound was heard near the tumour, and persisted even some days after that had disappeared. M. Saussier believed this case to furnish an example of a double laceration of the lungs, that on the left side being seated near the apex, that on the right near the base.

Since the completion of this paper, Dr. Bermond has called the author's attention to a case, which he published some time since in the 'Journal de Méd. de Bordeaux.' A boy, 10 years of age, was run over and died the next day. Much air was let out of the right pleural cavity, and a large longitudinal laceration of the upper lobe of the lung, unaccompanied by any fracture of the rib, was found to exist.

We are indebted to the kindness of Mr. Curling, for the particulars of the following case:—A child, æt. 5½, was brought into the London Hospital, July 1835, having died instantly after the compression of its chest by the wheel of a stage-coach. Upon examination of the body next day, air escaped from the chest as soon as it was opened. The left side of the cavity contained about eight ounces of extravasated blood, and there was an extensive and deep laceration of the lower lobe of the left lung. About six ounces of blood were found on the right side, and there was also a laceration of small extent in the upper part of the superior lobe of the right lung. A small quantity of blood was effused into the anterior mediastinum and the pericardium. None of the ribs on either side were fractured.

In endeavouring to explain the nature of this accident, M. Gosselin observes, that its production implies a great suppleness on the part of the costal cartilages and the ligaments attaching the ribs to the spine; without which these bones could not be depressed upon the contained organs, and the force would produce fracture, as in the great majority of cases it does. In this point of view it is important to remark, that the lesion especially occurs in the young.

"In fine, two things present themselves to us in the study of the mechanism of its production; a certain shock communicated to the viscera of the thorax, and a possible compression of the lungs by the ribs. But this does not solve the question; since, for a contusion or laceration, two things are necessary, a shock and a point of resistance; and where is the point of resistance in the present case? The lung is a spongy and softish organ, whose cells, filled with air, communicate freely through the air passages with the external atmosphere. Compress any part of it, and you displace the air and produce a subsidence of the lung, but do not induce its rupture. On the other hand, we cannot regard the vertebral column as a point of resistance, for, after all, and it is important to bear this in mind, the depression of the thorax to which I alluded is very inconsiderable. It is evident, from anatomical considerations, that, before pressure exerted on the ribs could crush the lungs against the spine, an enormous depression would be required, which could not be effected without inducing multiplied fractures. We must, I believe, introduce into the discussion a physiological element, which authors seem to have overlooked. This is the occlusion of the larynx at the moment when the chest receives the shock. Suppose that the lungs are distended with air, and that, whilst any obstacle existing at the larynx or trachea prevents its exit, external violence acts powerfully upon the chest without fracturing the ribs. The slight yielding which their anatomical attachments allow to the ribs, would produce a certain amount of pressure upon the lungs, especially at the point opposite to that at which the force is applied. Thus compressed, the lungs have no means of yielding, inasmuch as the occlusion serves as a point of resistance; and their texture, unable to withstand the shock, becomes lacerated. Is not this supposition which I propose realized by the mechanism of great efforts? Have not MM. Cloquet and Bourdon shown sufficiently that, during these, the larynx is closed after an inspiration by the contraction of the muscles of the glottis; and that, from this period until the cessation of such contraction, expiration is prevented?" (p. 230.)

We have not space to enter into an examination of the validity of this explanation; but may observe, that while M. Gosselin cites these experiments of Cloquet in corroboration of his views, M. Morell-Lavallée, in his paper upon Hernia of the Lungs, endeavours to confute the conclusions drawn from them at great length, and, as it seems to us, with justice on his side. When the shock is at the same time *powerful* and *sudden*, as in all the cases of this affection which have been put on record, it seems to us that the ordinary impediments to the free exit of the air through the bronchi, tracheæ, and larynx, are quite sufficient to account for the *momentary* compression of that which the lungs may happen to contain, and for the consequent rupture of their tissue by its elasticity; without the necessity of supposing that the larynx is closed, and the exit of the air entirely prevented, at the time of the occurrence of the shock.

Laceration being once produced, a communication may in some cases be established with the pleura, and in others none, producing corresponding modifications of the symptoms, which will be either those of hydropneumothorax, or of those of a pulmonic cavity. A subcutaneous emphysema may be produced in two modes: either, as in Smith's case, by an infiltration into the subpleural tissue around the great vessels, and so on into the anterior mediastinum and the cellular tissue of the neck, or by the fracture of a rib, at a point distant from the laceration of the lung. After such an accident, pneumonia is the most common occurrence, while in some cases a pleurisy or bronchitis may be set up; circumstances which serve to show that the prognosis must be carefully delivered. Still, in the two cases related by the author, a prompt cure followed the employment of antiphlogistic measures.

M. Gosselin has here drawn our attention to a form of accident which we doubt not is of more common occurrence in children than has hitherto been suspected. Every one acquainted with hospital practice, must have often witnessed with astonishment the rarity with which the bones of the chest are fractured in them, even under the application of enormous violence.

Besides the papers we have noticed, this volume of Memoirs contains others of minor importance, bearing the following titles:—"Luxation of the humerus directly downwards;" a case of numerous "Scirrhous tumours;" "Report on a case of chronic inversion of the uterus;" "Luxation of the upper end of the radius inwards." The analysis we have given will amply suffice to show that this first publication of the new Society is a very creditable production, and that much benefit may be anticipated from its labours.

Art. IX.

Mind and Matter, illustrated by Considerations on Hereditary Insanity, and the Influence of Temperament in the Development of the Passions. By J. G. Millingen, m. d., m. a., First Class Surgeon to the Forces, late Resident Physician of the County of Middlesex Lunatic Asylum at Hanwell, &c. &c.—*London*, 1847. 8vo, pp. 464.

> "We must, I fear, come to the melancholy conclusion, that, until egotism and ambition cease to be the chief principles of action of mankind, we must humbly submit to the inscrutable decrees of the Omnipotent. Such is life! Such is our doom! Our existence is expended in dreamy speculations, till death closes the busy scene!"

We have let Dr. Millingen speak for himself, by quoting the closing paragraph of his work. We add, however, that he speaks not for ourselves, nor we believe for many of our profession. Our existence, we trust, is a hopeful and active reality; and we humbly submit to the inscrutable decrees of the Omnipotent without repining, and without expecting that, under any possible combination of circumstances, either in this world or the next, we can do otherwise. If the golden age were restored, we must submit; if the millennium were arrived, still we must submit; if all things apparently (as they do now really) worked together for good, still there would be inscrutable decrees requiring our submission.

We somewhat regret to see a book like the one before us, because it is apt to leave an erroneous and rather unfavorable impression on the minds of the lay public, as to the metaphysical habits of thought of the medical profession. We are already sufficiently under the ban of bigots—a numerous tribe—for our assumed materialism and infidelity; we therefore do not thank writers of Dr. Millingen's class for the publication of hasty views in moral or mental philosophy, founded more or less on medical doctrines, and calculated to add to the distrust with which the relations that the latter bear to the former are viewed by a very large and influential class of readers. It behoves the medical philosopher to take a wider view than is taken by Dr. Millingen.

Mind and Matter are two words of deep import—words that require to be thoroughly understood, and therefore correctly defined. When thus defined, their application should ever be in accordance with the terms of the definition. "Mind and Matter" is a taking title, in truth, for the popular reader with an inquiring mind, ever asking to be taught the mystery of his being. How much thought has been devoted to this subject, how much labour expended, how much midnight oil consumed in wearisome yet ever-exciting researches, is known but to few; but all know, that the inquiry has occupied the greatest intellects that have ever appeared among men. With what result let the reader say, if there be one, of Plato and Aristotle, of Spinoza and Hobbes, of Locke and Condillac, of Stewart, Cabanis, Kant, and a host of others, the names only of whose works on mental philosophy and metaphysics would make a lengthy catalogue.

Mental philosophers have arranged themselves into two great and antagonistic arrays. On the one side are those who exclude the physiology of man from their researches, or allude to it incidentally only, and base their systems on pure metaphysics. On the other, are those who have looked more deeply into the corporeal organization, and, with more or less success, have applied physiology to the illustration of man's spiritual and moral nature. To this class belong the majority of medical writers, and it is for this reason that they have derived from the partisans of the opposite array the opprobrious epithet of "materialists." Many distinguished names grace this array; we can specially claim Bacon, Hobbes, and Locke. Bacon's general principle, embodied in the opening sentence of the 'Novum Organon,' must ever be the only true guide in moral philosophy, and as such will ever firmly link physiology with metaphysics. *Homo, naturæ minister et interpres, tantum facit et intelligit quantum de naturæ ordine re vel mente observaverit, nec amplius scit aut potest.* "Man, the servant and interpreter of nature, can only understand and act in proportion as he observes or contemplates the order of nature; more he can neither know nor do." This is eminently true of mental and moral philosophy; the pure metaphysicians have only studied a small part of the order of nature; the sensual metaphysicians, although farther extended in their views, have really limited their inquiries within the most narrow circle of human phenomena. But mind is not limited to man; the mental order of nature is as wide as nature itself. Everything teems with the phenomena of mind; the vast cosmos, the planetary system; earth, and all its swarming millions of organisms; and that not only *now*, but in the depths of a profound Past. Mind, to be studied and known aright, must be studied in a sphere widely emancipated from the microcosm of man; it must be studied through its phenomena in an infinite creation.

There are two things of which every man is certain; first, of his own existence, and therewith of a power to feel, perceive, and will; secondly, that he never had this consciousness apart from his body in any previous epoch, or during the present period of his existence. He may ignorantly imagine that in sleep his soul wings her flight to those regions to which he dreams he is transported; but the observation and contemplation of the order of nature certifies him of his mistake. There is to his own personal knowledge no mental act of his own, without the aid of the corporeal organism.

Although our consciousness assures us of our own existence, and of the existence of something that feels, perceives, and wills apart from, yet, through the organism, we can only *infer* the existence of other entities like our own. We see that the results of mind in action in the men around us are the same as we observe to belong to ourselves. We conclude that they depend on a similar cause. Extending our observations into creation in general, we see phenomena that further assure us of the existence of mind. We see force in action to definite ends; adaptation; foresight. The difference in our nomenclature between the cause of the 'two classes of phenomena is one of degree, for the difference in the visible phenomena is one of degree only. We call the creative mind Divine, and Infinite; we endow it with the same attributes as are possessed by our own, but give them an infinite greatness. But even of this, the Divine mind, we can learn nothing (independently of Revelation), except by the accurate observation of the phenomena of nature. Seeing the attributes of mind displayed in creation, we infer its existence; by analogy we clothe it with consciousness, and the other faculties of the finite mind.

Having so wide a field of observation, it is to be regretted that metaphysicians have not sedulously occupied themselves with the observation and classification of mental phenomena as they appear throughout the wide expanse of creation, instead of confining themselves to barren or empirical disquisitions on the faculties of the human mind. The singular faculty in living beings of adaptation to surrounding circumstances, is well worthy a detailed investigation and analysis. How is it that this adaptation takes place, and with what concurrent changes in the organism? Surely, phenomena of this kind have a higher bearing, than simply to illustrate the existence of Mind, and its wisdom and goodness. If we would know its nature, we must observe and contemplate its phenomena in the spirit of the Baconian philosophy.

Nor should the metaphysician have the field allotted to him alone, for the moral philosopher may seek and find knowledge in the same magnificent sphere of observation; for while the admirable adaptation displayed in all parts of creation, is an unanswerable proof of the most consummate wisdom, the *end* of that adaptation is a consummate proof of Divine goodness. In many creatures, that end is the good of the creature, its preservation, its pleasure. To the attainment of this end, the creature is endowed with suitable faculties and instruments, the action and use of which give rise to instinctive, emotional, and volitional acts, and develope the phenomena of instinct, passion, will. Thus it is that the phenomena of the moral world must be studied as natural phenomena, and as natural phenomena only, if apart from revelation; the Baconian principle is here strictly applicable.

Dr. Millingen, in imitation of his numerous predecessors, founds his views regarding the passions on this basis, because, in fact, there is no other patent.

> "Self-love and reason to one end aspire—
> Pain their aversion, pleasure their desire."

Like his predecessors, he contents himself with a mere metaphysical disquisition, in which we find nothing new, nor can there ever be anything new adduced, so long as the same beaten path is trodden. Let, however, the philosopher carry his analysis far into the organisms around him; observe, arrange, and contemplate their phenomena, and then set up his induction; and we venture to say, the process would add more to the philosophy of the passions, than the labours of the last two thousand years have done.

At a first consideration, this proposal to investigate the nature of the mind of man through inferior organism, may seem derogatory to his dignity as a rational creature, and as a being made in the image of God. What relation, it will be asked, can there possibly subsist between the mental phenomena of man and the merely vital unconscious acts of lower creatures? What can we learn of their nature and his destiny from the capricious actions of infusory animalcules, of crawling annelids, and creeping insects; of monstrously ugly reptiles, or ravenous unreasoning beasts? We already know much more from Revelation than can be gathered from such mean and undignified sources. To all this, and to similar opinions, it may be answered, that he has formed an unworthy estimate of the Divine mind, who thinks there is no longer a revelation of His will to man. All the phenomena of creation constitute a glorious revelation of God's will and wisdom. "There is neither speech nor language, but their voices are heard among them," speaking plainly and intelligibly to the humble listener, impressing him with his own dignity, as a being like to the Divine mind, bidding him commune in confidence with his Maker, and elevating him from a terrestrial creature to a spiritual, from earth to heaven. If the phenomena of creation thus reveal God to us, surely we may search amongst them for some revelation of the nature of man, without injury to our pride, or abatement of our dignity; if God's nature be found amongst them, we shall surely find something amongst them as to our nature who are made in His likeness.

The first question which Dr. Millingen moots, is the hereditary transmission of insanity, or, more properly, of the predisposition to the disease, and with no great success; he leaves us where we were. The law of propagation of species is only another form of hereditary transmission; and until this be thoroughly understood, it is to be feared that little more than an empirical knowledge can be attained as to the transmission of disease from parent to offspring. We are completely in the dark as to the force or forces by which the various organs are elaborated out of the amorphous embryo, whether animal or vegetable.

When we consider the transmission of minute peculiarities from parents to offspring, and especially of acquired peculiarities, we cannot but be struck with the all-pervading nature of the forces in action. As regards the cerebral development, and the mental constitution thereon dependent, there cannot be a doubt but that peculiar manifestations will pass from generation to generation, as surely as the ordinary characteristics of the outer form of humanity. We lately traced the drunkard's mania through four generations by the male side. Nothing is so easy as to multiply illustrations of the general fact of transmission; but we know nothing of the law which regulates it, in any part of the process, from the moment that the male stamps his peculiarities on the ovum, to their final evolution. When we talk of mere matter and of materialism, how little do we know of the forces of organized matter! how rarely do we remember that they escape from even the wildest hypotheses!

The condition of the brain which predisposes to insanity, is analogous to that which predisposes to other diseases and affections of the nervous system, as epilepsy, hysteria, apoplexy, irritability, or violence of temper, imbecility, &c. It is almost always dormant, however, for a great length of time; thus hereditary insanity rarely shows itself in children or young people before puberty. This exemption arises, partly perhaps from the circumstance that the brain, or that portion of it predisposed to disease, has not arrived at the stage of development requisite to the disorder of function, or else that the excitants are not applied during the dormant period. It is of importance that the *exciting* causes of insanity, that is to say, the antecedents of the morbid change, should be better understood with a view to prophylaxis.

Many of the instincts of lower animals lie dormant until the excitant is applied; there is no inner or spontaneous development. Thus the cub-tiger or lion is harmless and docile, easily restrained, and without ferocity until roused by the smell or sight of blood. The pheasant will have no instinctive desire to devour the ant, until the instinct is excited by the presence of the animal. As prophylaxis is everything in those cases in which there is a strong hereditary predisposition, it would be of great value to determine accurately in each case what are the excitants of the morbid state derived from without, and to take measures to prevent their action, either by their removal, or by setting up an antagonistic and therefore restraining process. There are revolutions in the functions of the various organs, especially of those subservient to reproduction, and to the excretion of urea and bile, which have a very important influence on the cerebral functions. It is much to be wished that some competent hand would take up the prophylaxis of hereditary insanity in a comprehensive spirit, and point out the precise relations of the various forms of the disease to the antecedents, whether consisting in a poisoning of the blood, in a direct action on the nervous centres from diseased organs, or in the *irritamenta malorum* that act through the senses or feelings directly upon the brain.

The increase of insanity concurrently with an increase in wealth and civilization, is significant of one, at least, of the exciting causes of the disease. If the passions *per se* would favour its development, then savage people, in whom certainly some of the passions are more violent than with civilized, would display a greater proportion of insane persons. This is, however, far from the fact. Of 135 cases admitted into the Retreat near York, in whom the exciting causes were of a moral or mental character, 112 were referred to sorrow, anxiety, and disappointment of various kinds.* These facts corroborate the statements of Pinel and others, as to the predominating influence of moral causes over the physical; and would lead to the conclusion that a certain refinement of the mind, or, in other words, a certain development of the brain, is an antecedent of some importance in the etiology of insanity.

If we were to multiply pathetic relations of terrible trials, and so illustrate every variety of moral cause by searching into the records of commercial anxieties and catastrophes, conjugal infelicities, religious struggles, or amatory disappointments, and were to go no farther in our inquiry than the ordinary goal of psychological writers, we should still be in ignorance of the mode in which the passions effect those delicate changes in the cerebral tissues, upon which the various shades and forms of unsound mind are dependent, from simple irritability, or eccentricities of temper, to incurable chronic delirium, or furious mania. Inquiries so conducted are mere ink-shed, or at best serve only to the purposes of a mental stimulation like that which novelists seek to excite.

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* Dr. Thurnam's Statistics of Insanity.

It is manifest that before we can even theorize upon the mode in which the pathemata act in inducing cerebral disease, we must know something of the nature of those changes which accompany their development or manifestation. According to the principles of inquiry laid down, we must extend our observation and contemplation of their phenomena far into the depths of creation, and minutely trace their analogies from grade to grade of organisms.

Our countryman Willis may be read even in the present day with wonder and delight; although his doctrines are obscure, and his cerebral anatomy imperfect, there are from time to time such wonderful scintillations of intellect seen shining through the gloom, that the reader cannot but feel himself in communion with a mind of the highest order. These remarks apply more especially to his 'Two Discourses concerning the Soul of Brutes, which is that of the Vital and Sensitive Man.' In his ninth chapter, he thus pithily defines the passions:

> "The corporeal soul, therefore, affecting pleasure as the greatest height of its felicity, in which it would acquiesce, [we quote from the folio English Edition of Willis's Works] is moved at the appearance of any good: if it be to come and contrary to opinion, by and for the getting it, desire or love arises; if with opinion, hope and boldness; if opinion esteems fruition hopeless, desperation is raised up; if this good be past, or should be lost by our default, shamefacedness or repentance is brought in; if it be possessed by others, emulation and envy; love is busied about it being taken absolute, without respect to time or possession. Besides also there are other respects and habitudes of appearing good, able to excite many other affections with ease. In like manner, on the contrary side, grief or trouble is a sickness of the sensitive soul, and a disposition very much ingrateful to it; wherefore all the objects apparently threatening its induction, the soul variously contracts herself, and is inclined hither and thither that she might shun the approaches of the threatening evils. . . . . Hatred is busied about evil taken absolutely, that being absent we prosecute with aversion; by and by about to come, with fear; and unworthily brought, with anger; falling upon ourselves, we sustain it with sadness; inflicted on our friends, with pity."

Discoursing on the passions of Brutes, Willis takes the same line of argument:

> "As to what regards natural instincts, it is a great and most ancient notion that there is in all living creatures an innate conservation of themselves, to wit that every individual might preserve itself as long as it can; this is a law of Divine Providence, inbred in all creatures which gather together the principles of life like a bond, otherways apt to be dissipated, and to depart one from another, and *on which*, as the basis, *the duration or continuation of the whole world stands.* This being supposed, it necessarily follows that all animals ordained for this end, are furnished also with certain fit means for following the same; wherefore they ought to know by natural instinct whatsoever things are congruous and benign, and what are incongruous or hurtful to them, and that they should follow these with hatred and aversion, and those with love and delight."

The relation here indicated between the instincts of lower animals and the emotions and passions of man is of considerable importance. We have already remarked, however, that we can only *infer* the existence of consciousness, even in our fellow-men, either from their testimony or their acts. But as to the lower animals, we can infer the existence of consciousness in them from their acts alone; and consequently such a statement as that just made by Willis, and which is the popular opinion, is one that must be received with a careful reference to the phenomena of life, and especially to those phenomena belonging to the nervous system termed reflex, the most striking characteristic of which is the adaptiveness to circumstances manifested in them, especially in repelling what things "are incongruous or hurtful" to the organism. We feel assured that, on the most careful consideration of the phenomena of life, the good principle enunciated by Willis will be found universally applicable, and must modify our opinion as to the presence of consciousness or feeling in the numerous classes of organisms. Willis asserts, with the grandeur of a true philosopher, with reference to the identical faculty of conservation of the animal, that it is the basis on which THE DURATION OR CONTINUANCE OF THE WHOLE WORLD STANDS. This faculty of conservation is manifested in lower organisms by the selection of what is benign and congruous, and by the neglect or repulsion of what is hurtful or incongruous; and this under circumstances in which we can conceive neither love nor hatred, aversion nor desire.

This analysis then of instinctive phenomena has separated consciousness from them, as not being always necessary to their performance, and as being frequently a coincident, but not an agent. This deduction widens our field of observation and contemplation of the order of nature, and opens to our view a new world of mind. In vegetables and vital organisms of the lowest and most microscopic forms, we find the most exquisite adaptation of the individual to surrounding influences, without any act of reason or volition on its part, and the most striking manifestation of acts with which we cannot without an outrage on common sense connect the idea of consciousness, and yet which would be termed instinctive and emotional, if we could connect such idea with them. Seeing then that such are the facts, are we warranted in concluding that all the instinctive acts of animals to whom we grant sensation, that is to say, the consciousness of impressions, are induced or excited by sensations? May not the consciousness be one thing, the acts themselves another? If highly adaptive acts, or acts closely analogous to the emotional and instinctive in ourselves, or the higher vertebrata, could be received as indubitable proofs of consciousness, then we might freely grant that endowment to reptiles and insects. But inasmuch as acts of these kinds occur under circumstances such that we cannot grant either reason, volition, or sensation, we are bound at least to suspend our judgment, until we have inquired more deeply into the nature of that force which developes these vital actions and processes, and considered its relations to that other power or faculty of animated beings, and which, termed consciousness in the abstract, is individual mind in the aggregate.

It will elucidate these relations, if we consider the nature and correlations of pleasure and pain. As regards the sensations of pleasure and pain, that is to say, the consciousness of pleasing or painful impressions, we know no more than of consciousness itself. We are certain of their existence, and of their external relations, first, to the excitants (or impressions), and secondly, to our motive and other apparatuses. We know that after such an act of consciousness, certain material changes in the body necessarily result. If we investigate these changes, we find that they have a certain relationship to the impressions that excited them, and that if the cause of the impression be benign and congruous (to use Willis's terms), the acts are even so, and the sensation is pleasurable; but if hurtful and incongruous, the acts are even so again, and repel or cause the organism to flee from the hurtful incongruous thing; therewith there is the sensation of pain. Now the only phenomena that we can observe or contemplate, are the acts and their motive or exciting power; the consciousness remains an indivisible invisible thing, known really only to and by itself. But the acts and their exciting powers or forces, are not, as we have seen, limited to conscious animals, since they may be observed and contemplated in those creatures respecting which we are in doubt whether they have consciousness or not, and in those respecting which we feel assured there is no consciousness whatever. Hence we infer that consciousness is not always necessary to the development of the visible phenomena of the passions, and that we must look into the vital structures for the material changes with which those phenomena are coincident and connected. Perhaps the vegetable kingdom would give the fairest opportunity for investigating the nature of those material changes in the intimate structures of vital organisms, which are dependent upon impressions made by what is benign or incongruous with their life and well-being. In many of these we see the principle of conservation in vigorous action, seeking, repelling, adapting. Or we might take up some of the simpler forms of animal life, and watch analogous changes in them, as being the analogues of those connected and coincident with the passions and instincts. It is to be regretted that no naturalist has as yet taken up this part of the question, and given a comprehensive glance of the phenomena with special relation to the higher manifestations, whether connected with consciousness and the accompanying sensations of pleasure and pain, or not. In the absence of such researches, we can only revert to the emotional phenomena as displayed in man and the vertebrata.

The emotions and passions may be divided into two very distinct classes: in the one, the causes are corporeal, in the other intellectual. When a person having a diseased heart awakes from sleep in vague terror, the cessation or obstruction of the circulation through the central force-pump is the true antecedent, and the emotion is probably analogous in its nature to its simpler form in brutes. But the terror excited by the fear of exposure of some act of moral turpitude or moral evil, is a different form of the passion, and is connected with the operations of the intellect, and consequently with the cerebral hemispheres. It does not appear that the first form is ordinarily, or even ever, a cause of insanity, whereas the latter is notoriously such. What then are the morbid changes induced in the hemispheres sufficient to cause this result? It may be inferred that the changes induced by the painful passions in the cerebro-spinal axis, and the whole system, in fact, differ only in degree; and so also with the pleasurable. As to the general effect of either class, if violent or intense, there is perhaps also a like difference. Anger and joy will both kill if excessive, but with a different sensation, and with a different action on the organism.

Although the passions act throughout the whole system, they are found individually to act on certain organs and structures in preference to others, and to excite results widely different from each other. Thus the passion of fear may only stimulate the motor system to more vigorous efforts in carrying the individual away from the threatening danger, and add wings to his flight. But the same passion in a greater degree, namely, terror, will fix the motor system with spasm, or relax it by paralysis. The individual is rooted to the spot, his limbs tremble, his sphincters relax. But should the passion be that of anger, and therewith a desire to repel the threatening evil be excited, we have the muscular system preternaturally active, and, in addition, the secretions of certain structures rendered morbid and injurious to life. In some of the lower animals, this change of secretion is permanent, and hence poisonous wounds are inflicted. In man, the secretions are thus altered, as when in a wet-nurse the lacteal secretion is rendered by anger hurtful to the child. Anger acts also on the hepatic secretion, and may induce chronic disease of the liver.

"————væ meim
Fervens difficili bile tumet jecur;
Tunc nec mens mihi nec color
Certâ sede manet"————

We have then another mode in which the passions may act in inducing insanity, not directly upon the central axis of the nervous system, but indirectly by vitiating the secretions and impairing the functions of organs necessary to health. How this happens can only be explained when we have learned the true nature of the changes excited by congruous or incongruous impressions; we know, however, that parts supplied exclusively by the sympathetic system, suffer very often.

"Danger, long travel, want, or woe,
Soon change the form that best we know—
For deadly fear can time outgo,
And blanch at once the hair.

. . . .
. . . .

Nor does old age a wrinkle trace,
More deeply than despair."

The action of the passions on special portions of the system, is an extremely curious subject, and well worthy of a detailed investigation. The changes that the colour of animals, as fishes and reptiles, will undergo under the influence of emotion, are well known. It is curious also to observe varying groups of muscles brought into action with the varying emotions. The buffalo or bison (bos taurus) is described by one well acquainted with its habits, as carrying its tail when in flight like ordinary cattle; when at bay, or wounded, he often lashes it, or carries it over his back; but when *mad*, or, in other words, in that mood which induces him to attack anything within his reach, he carries it nearly horizontally, with a slight curve in the middle, like some of the lions in the coats-of-arms. Numerous similar examples might be adduced; thus, the canine and feline races express different passions by almost similar sets of muscles: the cat or tiger lashes his tail horizontally, or moves the tip only when angry: the dog *wags* his tail horizontally when moved by joy. If the cat is terrified, it erects its tail; the erected tail of the dog expresses confidence and courage. Amongst the more frequent moral causes of insanity, grief and its modifications hold a prominent place. The action of grief is principally directed to the precordial region. The respiration is slow and oppressed, the action of the heart impeded, frequently peculiar spasmodic affections of the respiratory muscles are developed, as sobbing, globus, and even immoderate laughter, if the grief be excessive. Again, not only have the passions their own proper course of action through the cerebro-spinal axis, but there are excitants proper to them. Thus, various colours, odours, and sounds will develope emotional phenomena. The action of red upon bulls is well known—

> "Haud secus exarsit quam circo taurus aperto,
> Cum sua terribili petit irritamenta cornu,
> Pœniceas vestes, elusaque vulnera sentit."

The action of colours, odours, and musical sounds in developing the sexual instinct, and exciting the passion of love, is another example. Excitants of this kind require careful observation, as the emotional changes are not always very obvious. The colour and songs of birds, the sexual odours of insects and vertebrates, the action of music on the female especially, all show that, while there is a course in the motory system, along which the passions act, there is also a course in the sensory system along which they are excited, and that there is in this portion a special adaptation to external stimuli. In this respect they resemble the mere instinctive acts of lower animals, in which there is no reason to believe in the uniform existence of consciousness, as they often simply imply an arrangement of *vital* matter presided over by that mysterious force on which, as a basis, the duration or continuance of the whole world stands; a force that not only adapts actions to the effectuation of results, but arranges and adapts vital matter to the effectuation of actions. So wonderful is the mystery of animated nature, so profound the mystery of mind!

When we pass from these, the lower phenomena of the passions, to a consideration of the higher, and view them in their relation to mind, we are more deeply struck still, with the profundity of our subject. The expression of the passions by an individual, or, in other words, their visible phenomena, act as an excitant upon other individuals, and develope analogous and sometimes similar phenomena in them. The glance of anger, of pride, of contempt—one glance only, made up of some indescribably rapid combination of the muscles of the eyes,—will sometimes rouse the individual upon whom it falls to fury; so angry, so proudly contemptuous is the feeling excited. On the other hand, the sweet tones of love, the kindly look, the merry ring of joyous laughter, will call up kindred feelings; and we love, regard kindly, nay laugh even involuntarily with our fellow-men.

This would be marvellous if occurring between two human beings only, because the instinctive readiness of infancy in detecting the play of the feelings shows the same law to be in operation as in the preceding forms, and that there are writ on the organism some corresponding lines—some adaptation—of whose nature it is, in the present state of our knowledge, a vain thing to hypothesize, but of which we may say it is surely there, and as transmissible from parent to offspring as any other corporeal peculiarities. But we cannot fail to notice that this intimate relation is not confined to the human race. His humble brute companions watch his countenance often with intense solicitude and the most manifest anxiety, hoping to see there, and very often detecting there, with a superhuman rapidity, the expression of the feelings by which their master is actuated or about to be actuated towards them. Dogs are specially skilful in reading the human countenance; any one who will treat an intelligent dog as a reasonable being, will soon find that many a man or woman is surpassed by him in physiognomical skill. At a smile, however slight, he will look cheerful; at a frown scarcely perceptible, he will look sorrowful.

Nor is this permanency of feeling confined to the companions of man; it exists throughout creation; it awakens a touch of sympathy throughout the vast chain of animated beings. The cry of anguish from the dying brute in the depths of the forest, or near our own houses, is equally powerful in its action. We understand, and lower creatures understand, the hiss of the serpent, the roar of the lion, the yell of the hyæna, the battle-cries of the dog.

With each passion there is connected a machinery subservient to its expressions or its impulses; and this machinery, although at rest, will often speak loudly to us and to our fellow-creatures amongst the brutes. The eye of the carnivora terrifies even in repose. The uncouth appearance of the arachnida is instinctively our abhorrence, and we know not why, until we find that the creatures are crafty, cruel, rapacious, poisonous. There is the same instinctive dislike in man towards the reptile tribe. In the lower animals, these external indicants of passions and feelings vividly excite their instincts from the moment of birth, and without the possibility of their having been taught. It is to this community of sensitive, instinctive, and adaptive nature, existing between man and the lower animals, that we must look for an explanation of those various appetites and propensities developed in disease, or during pregnancy, of which Dr. Millingen has made a collection. We should apprehend, *à priori*, that every adaptive act, and every form of external phenomena of the instincts, emotions, and passions of lower animals, might, under some chain of circumstances or other, occur in the human organism.

The general doctrines we have brought forward, have some important practical bearings with reference to the prevention and treatment of insanity. The passions that act injuriously on the brain, are those which are accompanied by pain, and ordinarily a sense of depression. There is a desire excited for that which will relieve this sense of depression; and hence the readiness with which individuals suffering from grief, anxiety, or fear, from whatever source arising, become habitual drunkards or opium eaters; the alcohol and opium counteracting the cerebral changes on which the painful sensations depend. As a temporary resource, such means may be admissible; but it is evident that if the cause of the injurious changes continue in operation, no permanent benefit, but rather increased morbid change, will result. All pain that man suffers is for his conservation; it is a part of that great plan of ineffable benevolence which fills creation; but the mere instinct errs. The true remedy, and that to which the anguish and pain arising from the passions points, is to be found in man's moral life, namely, in a trust and confidence in Divine wisdom and goodness. Such a state of mind is doubtless not to be always attained, either because the individual, from previous habits and training, is incapable of experiencing it, or because the material changes have already deeply affected the sensorium; but where attainable, it is the best curative means, and is in perfect accordance with man's moral nature.

As auxiliary to this, but by no means of inferior importance, is the removal of or from those excitants on which the injurious passions depend. Upon this principle hangs the whole of the so-called non-restraint or soothing system, and the practice of seclusion. It cannot be doubted but that a more minute analysis of the excitants of the instincts and emotions would furnish us with a more discriminating therapeutics.

In those unfortunate examples in which, from an hereditary defect in the cerebral matter, or in which, from the superinduction of such defect, the ordinary circumstances of life become powerful excitants, and at last such changes are induced, as to involve not only that arrangement of the microscopic elements with which these mental phenomena are connected, but also the capillary system and the investing membranes,—a merely negative plan of treatment is not sufficient. Medication and such a hygienic treatment as will at once relieve the morbid condition, and prevent its recurrence, as well by attacking the predisposing causes, as by preventing the access of the exciting, is manifestly indicated.

ART. IX.

1. *Handbuch der pathologischen Anatomie.* Von CARL ROKITANSKY, Med. Dr. kk. o. ö. Professor der kk. pathologischen Anatomie an der Universität zu Wien, &c. I Band.—*Wien,* 1846.

Manual of Morbid Anatomy. By CARL ROKITANSKY, M.D., Professor of Pathological Anatomy in the University of Vienna. Vol. I.—*Vienna,* 1846. pp. 578.

2. *Entwurf einer pathologisch-anatomischen Propädeutik.* Von Dr. Jos. ENGEL.—*Wien,* 1835.

A Sketch of a System of Pathologico-Anatomical Instruction. By Dr. Jos. ENGEL.—*Vienna,* 1845. pp. 132.

3. *Anleitung zur Beurtheilung des Leichenbefundes.* Von Dr. Jos. ENGEL.—*Wien,* 1846.

An Introduction to the Critical Examination of the Appearances presented after Death. By Dr. Jos. ENGEL.—*Vienna,* 1846. pp. 448.

IT is nearly five years ago since the third volume (although the first in order of publication) of Rokitansky's 'Morbid Anatomy' was noticed in the pages of one of our predecessors. (See Brit. and For. Med. Rev. vol. xv, p. 83.) The work is now complete, and fully sustains in every respect (atrocity of style not excepted) the character which was then given to the portion that fell under review on that occasion. Never in the course of our reviewing labours has it been our lot to meet with an author more successful in concealing his true meaning by obscurity of style and complexity of language than Rokitansky; and were it not for the sterling value of the information that we knew, from our past experience of his former labours, must be contained in the present volume, we should long ago have thrown up our task in almost unutterable despair. His very name is a terror to translators. Dr. Peters, an American physician, and an old friend and pupil of Rokitansky's, in his preface to an abortive translation of the work, writes as follows: "We are scarcely ashamed to own that the present is the fourth version which we have made: twice we essayed the task unaided, and, although we had read the work repeatedly and carefully, and were familiar with Rokitansky's style, from hearing his lectures and enjoying familiar private intercourse with him, we could not succeed in making even a passable translation."* Even his own assistant and ardent admirer, Dr. Engel, was induced to write the book that stands third at the head of this article, because he felt that, in order to understand the volumes of Rokitansky (the first volume, it must be observed, was not then published), "some preparation is required, partly because the key to the whole—the general part without which it is impossible properly to comprehend the details—is still wanting; and partly because his language is so figurative, and, for that reason, often so obscure, as in some cases to give rise to misconceptions." When we further add, that the first volume is more unintelligible in language, and more difficult of comprehension from the general nature of the subjects discussed in it, than either of the preceding volumes to which Drs. Peters and Engel refer, we trust we have offered a satisfactory apology to our readers for the bald harshness of the translated extracts in the present article.

The volume now under our consideration is divided into an Introduction and ten Chapters; and we purpose giving an analysis of the most important of its contents, with occasional illustrations from the treatises of Engel.

The Introduction, which extends over fifty pages, contains an outline of the history of morbid anatomy, and a sketch of certain general principles essential to the clear understanding of the work. From the pages devoted to the consideration of these general principles we have condensed the following remarks:

1. Since anomalies in size, number, form, colour, texture, &c., are nothing more than deviations from the normal state of the organism and of its parts, we must regard them as *abnormal conditions*, and exclude the idea of an independent parasitic organism of disease.

2. There is no organ insusceptible of disease in one or more ways. When several anomalies occur simultaneously in an organ, they usually bear to one another the relation of cause and effect. Thus alterations in texture often induce alteration of size and form, and these again lead to alterations of position.

3. Although morbid anatomy is principally concerned with *local anomalies*, it often gives rise to the detection of *general diseases*, either directly by the eye, or by a process of reasoning based on facts; and elucidates the relations subsisting between them. There is no doubt that the seat of general diseases must be sought for in the fluids of the body, especially in the blood.

4. Although the recognition of the existence of general diseases by the aid of pathological anatomy be a great advance, yet it renders us liable to fall into the error of exclusive humoralism, and to deny the existence of all local diseases; whereas we should always attempt to derive and construct them from a corresponding general morbid process, since many local diseases are nothing more than the localization of a (pre-existing) general disease.

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* A Treatise on Pathological Anatomy. By Carl Rokitansky, M.D., &c. Part I. Translated from the German by Dr. J. C. Peters.—New York, 1845. The translation does not extend beyond the first 152 pages of the volume previously reviewed by us; and we cannot ascertain that any more parts are expected.

5. The existence of purely local disease is founded (α) on the individual vitality of the organs, and their independent relation to the external world; and (β) on the local action of direct or indirect irritation, producing, either directly or through the influence of the nervous system, a local disturbance in the vital process, or in the activity of the assimilating and secreting processes, and in the metamorphosis of tissue, occasioning an anomalous reciprocating action between the vessels (together with their contents) and the parenchyma, and thus giving rise to quantitative and qualitative anomalous products.

Their existence is further shown (γ) by the non-occurrence of a diseased condition of the blood in such cases; and (δ) by their curability by simply local means.

6. Local disease extends itself, for the most part, in the following way:

a. *Per contiguum*—a mode of distribution favoured by,

α. Homogeneity of structure.

β. Increased intensity of the disease.

γ. The nature of the disease. While some diseases, as tuberculosis and cancer, may attack every organ and tissue in the body, others, as for instance, the typhous process, show a predilection to certain textures.

b. *In distans*, to similar or dissimilar organs, through the agency of the nervous system, without the induction of general disease.

7. A disease, at first purely local, may pass into a general disease of a similar or different nature; the former being effected through the influence of the nervous system; and the latter occurring when, in consequence of the products of the local disease, the blood becomes impoverished in fibrin, albumen, or salts; or when venosity or cyanosis arises through the mechanical impediments presented to the proper current of the blood in diseases of the heart and lungs.

8. General disease is likely to be induced,

α. In proportion as a local disease has spread over many similar or dissimilar structures, and occurs with great intensity.

β. In proportion as the products of the local process are heterogeneous, as compared with the constitution of the normal structures.

γ. When the characters of the local disease are not in themselves sufficient to explain the symptoms during life, or the appearances after death.

δ. When, in addition to the alienation of the solids, the secretions and excretions are anomalous, and—

ε. In proportion as the whole organism suffers in the form of cachexia, without the occurrence of any peculiar anatomical change.

ζ. In proportion as the blood manifests an anomaly in the quantity or quality of its constituents.

9. The termination of disease is an object of anatomical investigation.

α. A local disease may terminate in a perfect restoration to a normal state, or there may be certain residua or consequences of the disease not incompatible with a state of comparative health.

β. The change of one general disease into another (metaschematismus) is frequent. Thus we have dropsy after defibrination and the elimination of albumen, and cancer after tubercle.

γ. Metastatic transitions are a frequent object of anatomical investigation. We shall return to the metastasis presently.

δ. The disease may terminate in death. Diseases are generally fatal—

αα. Through exhaustion of power and of organic matter—tabescence.

ββ. Through disturbance of the functions of vital organs, and their paralysis—certain dislocations, hypertrophies, atrophies, &c.

γγ. And through an abnormal state of the blood, and a consequent paralysis of the nervous centres.

10. The disposition to various diseases differs with age, sex, climate, &c.

Thus aneurisms occur chiefly in adult or advanced age, while rachitis is limited to the period of childhood. Tuberculosis in early life principally attacks the lymphatic glands and brain, while, after the age of puberty, the lungs are generally affected by it. The sexual system of the female favours the more frequent occurrence of cystoids, cystosarcomatous growths, and cancer. There are climates in which tuberculosis is rare, while intermittent fever and hypertrophied spleen are common. Under the tropics, the so-called ossification of arteries is of extremely rare occurrence.

The first chapter of Engel is devoted to a similar object: it treats of the anatomical causes of disease, the course of diseases, the conversion of one disease into another, the combination and complication of diseases, and the products of disease. In the section on the course of disease, we find him complaining of the lax acceptation of the term *metastasis*. He observes that many of what are commonly termed metastases, are nothing more than diseases of the same nature, propagated in accordance to the laws of continuity or contiguity, as, for instance, the inflammation of the subcutaneous cellular tissue in the exanthemata.

In other cases, the metastasis and the primary disease arise from the same cause, in which case it does not necessarily follow that there should be any similarity in the nature of the two. If erysipelas of the face and meningitis are simultaneously developed, we term the latter a metastasis. When pleuritis occurs simultaneously with an exanthema, or appears soon afterwards, we frequently term it a metastasis. The endocarditis occurring at the same time with inflammation of the joints, is apt to receive a similar denomination.

There is a separate class of metastases, which are in reality nothing more than exaggerated symptoms; as, for instance, the pulmonary hyperæmia accompanying the exanthemata, and the catarrhal inflammation of the bronchial tubes in the same diseases elevating itself to croupous pneumonia, which is then named a metastasis.

There is yet another class in which, through the action of new causes, a perfectly independent disease developes itself during the progress of another general disease, without standing in any essential connexion with it. Suppressed perspiration sometimes gives rise to pulmonary œdema; if this occurs during the eruption of an exanthema, the affection of the lungs is regarded as metastatic. Extreme vexation and terror occasionally give rise to inflammation of the brain, and to suppression, to some alteration, or to an increase, in a normal or morbid secretion; the same causes may give rise to suppression of the lochial discharge, and to cerebral inflammation; and yet the latter disease is not a consequence of the former.

A similar interpretation must be put on meningitis, or pulmonary œdema, occurring after suppression of the flow of milk. When pulmonary tuberculosis is developed after the suppression of normal or anomalous secretions, it must be regarded in the light of a new disease springing from the same cause that gave rise to the suppression, not as dependent on the suppression itself.

In the section on the conversion of one disease into another, we find the following questions propounded by Engel:—

1. What are the means used on the part of the organism for the distribution of diseases over all or certain systems, and how does their distribution follow in accordance with these means, both in relation to space and time? What means on the part of the organism hinder the further extension of disease?

2. What consequences of disease are *necessary*, occurring in every organism, under every condition? What consequences follow only in those cases where, on the part of the organism, certain conditions are presented which are altogether independent of the original disease?

3. What consequences follow when an external influence is superadded?

4. Is it within the power of the organism to limit these consequences? or, in other words, is a natural cure possible or impossible?

Engel gives a few illustrations, with a view of elucidating these points.

In relation to the *first question* he observes, that an inflammation established at [illegible]ngle spot may give rise to a general inflammatory crisis, and that a very limited amount of suppuration may rapidly decompose the whole mass of the blood. An affection of a nervous centre is sufficient to extend a disease over all the tissues into which nerves enter.

The direct distribution of diseases by the vascular and nervous systems is usually very rapid, and extends without hinderance to the most remote parts; the extension of disease is, on the other hand, extremely slow, if it progresses steadily in space, and it is easily impeded by dissimilar tissues.

The extension of disease is impeded by relations of space (räumliche verhältnisse). Similar tissues, as, for instance, two neighbouring cartilages, separated either by mere space, or by a different tissue, do not communicate their diseases to one another. If two portions of the same tissue are supplied with blood from different sources, they not only present nothing common in the diseases to which they are liable, but also can only with difficulty communicate to each other those diseases which emanate from the blood. We frequently observe this in the diseases of the visceral and parietal surfaces of a serous membrane. The venous stases in the vascular system, which everywhere occur in cases of valvular insufficiency and dilatation of the heart, do not generally extend beyond the capillaries; that is to say, they give rise to no dilatation of the arterial system. Pathological epigeneses* are also of great importance in isolating morbid processes; every pathologist must have witnessed cases in which inflammation has been stopped at a spot where areolar tissue, produced by previous inflammation, begins. Further, it is known that a disease, pre-existing in an organ, frequently incapacitates it from the influence of a new disease; thus, for instance, an organ that is hypertrophied, or atrophied, is seldom affected by a disease spreading over most of the other organs, and a nerve in a state of inflammation seems incapable of communicating diseased conditions.

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* We adopt this word as an equivalent to the *Neubildungen* in the text, because several German pathologists have used it when writing Latin. The German word is literally *new formations*.

In reply to the second question he observes, that a mechanical contraction of a canal produces stagnation of the fluid occurring above the contraction, and a consecutive dilatation of the tube, together with hypertrophy of the muscular fibres in the vicinity and paralysis of the contractile tissue. These are necessary and persistent consequences, even in cases where the impediment, after lasting a considerable time, has been removed. Insufficiency of the mitral valve invariably gives rise to dropsy; it depends, however, on other causes, whether or not a venosity of the blood ensue. The consequences of a well-marked granular state of the liver are inevitable.

An hepatic abscess may, in some cases, give rise to acute poisoning of the blood, but it does not necessarily follow that it should do so.

Obesity induces relaxation of the venous system; but in order that varicosity may also ensue, a condition on the part of the organism is also requisite, namely, that there should be a certain amount of blood. Similarly, bronchial catarrh leads to pulmonary emphysema; but a saccular bronchial dilatation is only induced, when the catarrh has existed for a long time, and is seated at the extremities of the bronchial tubes.

In relation to the third question he observes, that hypertrophy renders a muscle very fragile, but that it only becomes rent when an external irritant gives rise to an augmented contraction. Suppurative inflammation of a bone gives rise to a destruction of the osseous tissue, but it is only through the action of the air that ichorous discharge is thrown off.

The observations of Engel in relation to the fourth question extend over several pages. The following paragraphs afford a very condensed sketch of his views on this point.

α. It is only in very few cases that the peculiar morbific cause can be removed by the natural passages, and a spontaneous cure thus effected. The following are the means employed by Nature to attain this object:—The expulsion of the morbid product by reflex motions, excited by the morbid product itself, as is observed in the case of gall-stones urged forward by the contractions to which they themselves give rise. Another means is afforded by the inflammation, which the morbid product excites, giving rise to a communication with the external skin, or with one of the canals which open externally, and thus leading to the ultimate removal of the product itself. There are again certain conditions under which a morbid product is entirely destroyed by putrefaction; and sometimes even in these and similar cases, which will readily suggest themselves to the mind of every pathologist, the mere removal of the morbid product is obviously but the first indication of cure.

β. When morbid products cannot be removed, the organism may present certain means of rendering them innocuous, as by induration, cretification (verkreidung), and ossification. These metamorphoses are, however, often not unattended with danger, as in atheroma and ossification of arteries.

γ. There are some diseases which only heal by the formation of certain products. There are, however, very few chronic diseases in which these products occur in organs destined for excretion or secretion; and hence, in such a case, the cure depends on the possibility of neutralizing the injurious effect of such a product in the organism, or of reducing it to a minimum.

δ. Another and not less successful means of cure on the part of the organism, is the production of a new disease, approximating in intensity to the original.

ε. The injurious consequences of material changes may also be entirely obviated, and that without the assistance of the physician,—

1. By an active compensating development of the part of the organ or of the organic system still unaffected.

2. By an active development of one of a pair of organs, when the other can no longer act; as in hypertrophy of one kidney from atrophy of the other.

3. By an active development of heterogeneous organs, whose function is connected with that of the diseased or inert organ. This is seen in the hypertrophied condition of the muscular fibres posterior to a urethral stricture, in the thickened walls of the stomach arising from stricture of the pylorus, and in numerous other instances.

ζ. Finally, injurious consequences may be very much lessened, and the distribution of the disease limited,—

1. By the formation of an isolating tissue checking its further progress, as may be seen in the callosities developed by chronic inflammation around the basis of ulcers.

2. By the production of a new morbid condition, as in the formation of a new anus, when a stricture in the intestinal canal cannot be overcome.

With these remarks on Engel's questions, condensed from his own observations, we conclude our notice of the introductory chapters of our authors, and proceed to a systematic consideration of Rokitansky's volume.

THE FIRST CHAPTER is devoted to *Anomalies in relation to the number of parts.*

In connexion with the subject of congenital deficiencies, our author points out, more clearly than we recollect to have seen in the works of any preceding writer, the sympathetic relations of absent parts; that is to say, how the absence of one part or organ is almost necessarily associated with the absence of certain other parts or organs. "Thus in acephalia, the heart is almost always wanting, and indeed the thoracic viscera generally, likewise the liver, spleen, and pancreas—an instance of regularity which at present does not admit of explanation, since these organs are not developed from an homologous germ-structure, and there is no dependence of functions between them." (p. 51.) As we regard this chapter, however, as much inferior to the corresponding chapter in Vogel, a work now accessible to all our readers, we pass on to

THE SECOND CHAPTER, on *Anomalies of size*, from which we extract the following remarks on hypertrophy:—

> "There appears to be no doubt of the existence of hypertrophy of areolar tissue, fibrous tissue, fatty tissue, the general investments of the body, including the corium and the papillæ, the sebaceous glands and the epidermoid structures, the mucous membrane and its follicles, and the osseous system.
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> "Hypertrophy cannot be ascertained by a comparative enumeration of the elementary structures, the size of which, even in their physiological condition, presents many differences; but as an increase of bulk is apparent, and there is no simultaneous formation of heterogeneous elements, the sum of these elementary structures must actually be increased, in order to occasion the hypertrophy. We find the elements of the textures affected to be very frequently in an embryonic stage, as, for instance, in areolar tissue.

"The recognition of hypertrophy of the muscles presents the greatest difficulties, simple as, at first sight, it appears. The augmentation of mass and volume of an hypertrophied muscle *appears* to arise from an augmentation of its fleshy mass, but hitherto no stringent proof has been obtained, at least for the striated muscles; for it is impracticable to think of counting the primitive fibres, and we have hitherto failed in detecting elements which would appear to be passing through embryonic stages of the formation of new muscular fibre; nor have we been more successful in recognising an enlargement of the primitive muscular fibre, depending on an increase of the contents of the primitive fibrillæ.

"That the hypertrophy consists solely in an increased formation of the areolar investment [in verstärkter Scheidenbildung; this does not refer to the sarcolemma, REV.] is opposed by the circumstance that in an hypertrophied muscle, as, for instance, the biceps brachii, we observe a deep fleshy tint, an eminent degree of resistance, and great functional capacity; and just as little (in point of fact, far less) could an increased quantity of fat explain this hypertrophy, since an excessive formation of that substance retards in every form the growth of muscular flesh; and the two last assumptions altogether fail in explaining the hypertrophy of organic muscles.

"The examination of hypertrophied hearts* affords results for the solution of this problem, that seem to lessen in value in proportion to the size of the hypertrophied organ. We cannot distinguish any new formation of muscular fibres; the existing fibres being, on the other hand, in a state of disintegration, and their fibrillæ partially breaking up into dark-coloured molecules—a condition corresponding with the diminished energy of the organ. That which appears to be an increase, is actually nothing more than an irregular aggregation of an amorphous, flaky (scholligen), or flaky-fibrous blastema, copiously interspersed with nuclei of areolar tissue in different stages of development, and of areolar tissue itself, together with a large quantity of free fat, and of adipose tissue.

"Hypertrophy of the organic muscles is more easily recognised; for here, besides nuclei, we find minute, flattened, nucleated bodies growing lengthwise, and serving as the rudiments of new fibres. The uterus in the pregnant state affords not only a striking illustration of this form of hypertrophy, but also of the retrograde metamorphosis of the fibres to a residuum of countless nuclei, which finally become resorbed." (pp. 68–9.)

In his description of the hypertrophied heart, Engel agrees pretty closely with Rokitansky. In addition to mechanical impediments to the blood's motion and to previous peri- or endo-carditis, as causes of cardiac hypertrophy, he observes, that "in many cases the heightened irritation excited by blood rich in plastic constituents appears sufficient in itself to induce hypertrophy."

Engel enumerates the following involuntary muscles as especially liable to hypertrophy:—

α. The muscular fibres of the œsophagus and stomach, and much more rarely those of the small intestine.

β. The muscular fibres of the large intestines, especially of the rectum.

γ. The muscular coat of the bladder.

Hypertrophy occurring on the nervous system is, according to Rokitansky, very doubtful.

"The formation of new nervous fibres is in itself highly improbable, and has never yet been proved; neither is there any evidence of an enlargement of the nervous tubuli by an augmentation of their contents. In the central organs—certainly in the brain—the enlargement depends on the aggregation of finely granular, areolar tissue between the nervous tubuli. In the periphery it can only depend on an augmented amount of neurilema.

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* When this article was written, the admirable Lectures of Professor Paget on Hypertrophy and Atrophy, had not been delivered. They have since appeared in the 'Medical Gazette,' and we most strongly recommend their perusal to all who take an interest in such subjects.

"In the case of the ganglia, the formation of new ganglionic cells is certainly not proved, but the reproduction of extirpated ganglia is not improbable." (pp. 68-70.)

There are few organs more subject to hypertrophy than the glandular organs. If we duly consider the very frequent cases of spurious hypertrophy occurring in the liver, spleen, and kidneys, we find, in the first place, that an unessential constituent—areolar or fatty tissue—which enters into the composition of these organs, becomes hypertrophied, as, for instance, in the mammary or salivary glands; or, on the other hand, the hypertrophy attacks the true parenchyma; and, with reference to this subject, the following points must be borne in mind. An examination with the naked eye shows—as may be best seen in an enlarged prostate—an increase of bulk in the parenchyma, which may originate either in the formation of new acini (lobules), or in the enlargement of existing ones, new primitive enchyma-cells being formed in them, or possibly in both ways simultaneously. A new formation of lobules and lobes most probably takes place, judging from what we observe in hypertrophied conditions of the prostate; at any rate it is undoubted, from the observations of Henle regarding the existence of solitary enchyma-cells in the vicinity of the lobules of the lachrymal glands of a calf, and still more so from what may be readily observed in hypertrophied thymus glands, that there may be a new formation of such glandular cells, which, by the resorption of the intervening walls, unite with the existing lobules, and thus cause their enlargement. There is a peculiar form of hypertrophy which concurs with the dilatation of the cavities of the acini, and which is a very frequent groundwork of the enlargement of glandular structures. This dilatation is the consequence of the augmentation of the contents of the acini (i. e. of the secretions) occurring with the hypertrophy, and produced by the same local or general causes. The hypertrophy consists in an increase of the mass of the investing fibrous structure of the acini. With this condition there is closely associated the development of degeneration of the acinus into a dilated cyst-like cell with a strong investing fibrous layer, which may finally grow into a cyst filled with contents entirely different from the original secretion of the gland; these secretions varying in quantity and quality proportionally to the alteration of structure. Cases of this nature have been observed in the acini of the thyroid gland, in the malpighian bodies of the kidney, in the graafian follicles, and in the mucous follicles, especially those of the cervix uteri, which are very frequently developed in their physiological condition into apparently thick-walled cysts discharging their contents by dehiscence.

There is great difficulty in accurately establishing hypertrophy of the liver, spleen, and lymphatic glands.

Hypertrophy of the liver, by the formation of new hepatic cells, would in no way be opposed to our views regarding its objects and function, but an anatomical demonstration of the fact is altogether impossible. An apparent objection to this view is found in the turgescence of the hepatic cells, consequent on an increased amount of fat or bile within them, combined with capillary hyperæmia, a condition which influences the degree of the appearance of the so-called secreting substance of the liver,—a partial hypertrophy—in the form of nutmeg liver.

Hypertrophy of the spleen, together with a thickening of its fibrous structure, must be chiefly sought for in the increase of bulk, or, in other words, in the increase of constituent elements of the pulpy matter of that organ.

Moreover, hypertrophy of the lymphatic glands cannot be explained by the formation of new lymphatic vessels in the ganglia, but rather by the augmentation of the parenchyma between the vessels. At any rate, in cases of atrophy these glands appear deficient in it.

Hypertrophy of the lungs does not consist in the formation of new pulmonary cells, but in the increased thickness of the cell-walls. Increased (vicarious) energy in the function of a lung in this condition would at first sight lead to the conjecture that the capillary system was augmented by the formation of new vessels, which, however, has not been proved.

In the same way, hypertrophy of the corpora cavernosa may depend, not on the formation of new cells, or on an increase of their numbers by the development of new septa, but on an increase of their bulk, namely, on thickening of the cell-walls, and simultaneous dilatation of the cell-cavities.

We proceed to the consideration of the causes of hypertrophy. These are:

"1. An abnormal increase of blood in the capillaries of the organ, and retardation of the circulation in it,—in short, repeated and prolonged hyperæmia. Examples of this nature are presented to us in the hypertrophies frequently occurring in the abdominal viscera from mechanical hyperæmia, in that of the mucous membrane in organic heart-disease, in that of the areolar (cellular) tissue of the lower extremities accompanying varicose veins, and, finally, in those (especially affecting mucous membranes) resulting from the hyperæmia remaining after repeated inflammations.

"2. Increased activity consequent on the most varying species of irritation, either direct or reflected (übertragenen). This is specially exemplified in hypertrophies of the voluntary muscles, the heart, and the organic muscles.

"3. A larger number of hypertrophies are dependent on an alienation of the general nutrition (gesammt-vegetation) and on an anomalous blood-crasis; the hypertrophy appearing as the expression, or as a symptom of general disturbance. ......Under this head we rank hyperostosis, polysarcia, goitre, the hypertrophy of the brain and of the lymphatic glands occurring in rickets, the excessive formation of morbid fat in the alcoholic dyscrasia, fatty and waxy liver, lardaceous infiltration of the liver, spleen, and kidneys in tuberculosis, rickets, inveterate syphilis, &c.

"4. Inflammation giving rise to the so-called inflammatory hypertrophy. In areolar (cellular) structures and bones, it induces true hypertrophy; in all others it gives rise to false hypertrophies, by the formation of areolar (cellular) and fibroid tissues." (pp. 75-6.)

We must mention, in reference to the concluding sentence, that Rokitansky applies the term *false hypertrophy* to those cases in which the increased nutrition directly causing the enlarged growth is anomalous; that is to say, when textural elements, distinct from those of the affected organ, are deposited. They approximate to, if they are not identical with, the adventitious products or pseudo-plasmata of other authors.

We give, in the following remarks, a summary of his views regarding the causes of local atrophy. Atrophy may be dependent on,—

1. A diminished supply of blood to the affected organ, consequent on

pressure on, or contraction or obliteration of, the afferent vessels. Thus we not unfrequently have partial atrophy of the liver associated with adhesive phlebitis of the branches of the portal vein, and atrophy of the cartilaginous investments of the joints from sclerosis of the spongy ends of the bones. It is not impossible that some forms of atrophy, as, for instance, that of the brain, may depend on an intrinsic thickening of the walls of the smaller vessels, thus hindering the passage of the nutrient fluid through the vascular parietes.

2. Exhausting processes of disease and cure, as, for instance, atrophy of the bones (and adjacent soft textures), consequent on caries from destructive suppurations during the healing of wounds. Atrophy of the uterus after delivery and exhausting puerperal diseases.

3. Diminution of innervation, paralysis, or hinderance of the activity of an organ, consequent on mechanical malposition, as in the case of the muscles in anchylosis and dislocation.

4. Compression, by inducing increased resorption, may give rise to atrophy; so also may

5. Anomalies of the general nutrition, and especially of the blood-crasis, under which we may place the atrophy of the testicles and ovaries in advanced life, the atrophy of the thyroid gland when the blood is impregnated with iodine, &c.

Rokitansky makes a separate class of *secondary atrophies;* these are preceded by a change of texture: as illustrations of this form of atrophy, he mentions cellular infiltration into the medullary cerebral substance consequent on encephalitis, the atrophied condition of the kidneys resulting from nephritis and Bright's disease, and the conversion of muscular fibre into fat.

The THIRD and FOURTH CHAPTERS, devoted to the consideration of *anomalies of form* (*deformitates, formæ alienæ*) *and of position* (*ectopia*), may be passed over without comment.

From the FIFTH CHAPTER, *on anomalies of combination*, we extract the observations on that form of fissure (spaltbildung) which is dependent on the persistence of certain communicating openings between parts that should be afterwards separated, and on the permanent patency of certain canals.

"*Deficient development of the partition-wall of the ventricles and auricles of the heart giving rise to patency of the foramen ovale.* These partition-walls are at first gradually formed in the heart, those between the auricles not being fully developed till after birth. The deficient development of the partition between the ventricles presents a similarity to the actual and perfect heart of fishes and reptiles (with the exception of crocodiles), especially to that of snakes and tortoises; whilst the deficient state of the wall separating the auricle is especially suggestive of the heart of fishes.

"The arrest of structure is often palpably the result of endocarditic metamorphosis of the valves, the residuum of valvular inflammation during fœtal life.

"*Patency of the ductus arteriosus, and its injurious dilatation.*

"*Patency of the ductus venosus Arantii*, in consequence of which a portion of the blood of the portal vein finds its way into the inferior vena cava.

"*Patency of the processus vaginalis peritonæi*, frequently giving rise to a hernia or congenital hydrocele. As a general rule, the inguinal canal closes, when, in the seventh month of fœtal existence, the testicles have descended into the scrotum, carrying with them a prolongation of peritoneum. Sometimes, in consequence of an arrest of structure, no closure ensues.

"*Patency of the urachus* permitting of the escape of urine from the navel. In the embryo, both the urachus and urinary bladder are parts of the allantois, which has conveyed the umbilical vessels from the embryo to the external membrane of the ovum, to form the placenta. In the human embryo, it usually closes at a very early period, and the parts external to the umbilicus disappear, whilst the internal portion is developed inferiorly into the urinary bladder, and the part lying between the two aforesaid parts (the bladder and the umbilicus) contracts into a sort of cord, and forms the urachus; being, in fact, an arrest of development." (pp. 99-100.)

The SIXTH, SEVENTH, and EIGHTH CHAPTERS are devoted to *anomalies of colour*, *anomalies of consistence*, and *separations of continuity*. Collectively they occupy only twelve pages, and contain nothing worthy of special remark.

The NINTH CHAPTER, on *anomalies of texture*, is the most important in the whole volume—we might add, in the whole work. It occupies a space of nearly 350 pages, or considerably more than half the volume, and embraces the whole subject of new formations or epigeneses. As there are comparatively few points in this rich fund of pathological information that our limited space will allow us to touch upon, we have deemed it advisable to give a brief tabular sketch of the titles of the most important subjects, with the view of more forcibly directing the attention of our readers to the nature of the contents of this most admirable chapter.

I.—ORGANIZED EPIGENESES.

A. *On the organized epigeneses generally.*
  On the blastema and its metamorphoses, with especial reference to the fibrin.
  Solidified fibrin.
  Metamorphoses of the blastema.
  Hyperæmia, congestion.
  Hemorrhage.
  Anæmia.
  Inflammation.
  The inflammatory process.
  Different kinds of inflammation.
  Relation of the inflammatory process to a crasis.
  Exudations.
    1. Fibrinous exudations.
      a. Simple or plastic fibrinous exudation.
      b. Croupous exudation.
      c. Tubercular exudation.
    2. Albuminous exudation.
    3. Serous or dropsical exudation.
    4. Purulent and ichorous exudation. Pus, ichor.
  Dissolving (schmelzenden) exudation.
  Hemorrhagic exudation.
  Terminations of inflammation.
  Gangrene and necrosis considered generally.
  Characteristics of inflamed tissues, and diagnosis of inflammation in the dead body.
  Deposits and metastases.

B. *On the organized epigeneses in particular.*

1. Formation of areolar tissue.
2. Formation of fibroid tissue.
   Gelatigenous fibroid tumours.
3. Elastic tissue, and that of the circular fibres of the arteries.
4. Formation of cartilage. Enchondroma.
5. Formation of bone. Osteoid, ossified enchondroma, concretions, cretefactions.
6. Formation of vessels.
7. Formation of fat.
   - A. Fatty tissue.
     - 1. Normal. 2. Anomalous.
   - B. Free fat.
8. Formation of epidermis and hair.
9. Formation of pigment.
10. Colloid.
11. Formation of cysts.
12. Sarcoma and carcinoma.
    - A. Sarcoma.
      - 1. Gelatinous sarcoma.
      - 2. The albuminous fibrous tumour, fibrous sarcoma.
      - 3. The albuminoid fibrous tumour (Müller).
        Cystosarcoma.
    - B. Cancer. Carcinoma.
      - 1. Gelatinous cancer, colloid cancer, alveolar cancer.
        Cancer aréolaire, gélatiniforme.
      - 2. Fibrous cancer, Cancer simplex.
      - 3. Medullary sarcoma, Cancer medullare.
        - *a.* Cancer melanodes.
        - *b.* Typhus matter.
        - *c.* Villous cancer (zottenkrebs).
        - *d.* Epithelial cancer.
      - 4. Carcinoma fasciculatum (Müller).
        Cystocarcinoma.
13. Tubercle. Tuberculosis.
    - *a.* Simple fibrinous tubercle.
    - *b.* Croupo-fibrous tubercle.
    - *c.* Albuminous tubercle.
      Acute tuberculosis.
14. Crude albuminous blastemata.

II.—UNORGANIZED EPIGENESES.

A. *On the unorganized epigeneses generally.*

B. *On the unorganized epigeneses in particular.*

In his remarks on *the organized epigeneses generally*, Rokitansky maintains the view, which we have more than once insisted on, that the cell-theory is being carried out too exclusively. He observes that:—

> "The formation and development of new structures present great and important differences; they, undoubtedly, often follow the laws of the cell-theory, cytoblastema, elementary granules, nucleus, cell and fibre; but, at the same time, the

formation of fibres from nuclei and granules, and especially the independent development of fibre directly from a solid blastema, from intercellular substance, primitive structureless membranes, and membranous coagula, is at least equally general." (p. 123.)

After some general remarks on the blastema and its metamorphoses, we meet with the following remarks on solidified fibrin, the only solid blastema ever known to occur. The following are its most important forms:

"1. The fibrin taken from healthy subjects after death forms tolerably compact, tough, and somewhat adhesive coagula, of a yellowish-white colour, which are separable into membranous layers, having a soft felt-like surface. The microscope shows a clear (hyaline) membranous stroma, over the margin of which there is a thick projecting film of very fine, elastic, dark-edged, ramifying fibres, readily soluble in acetic acid. Moreover, we find numerous round, shining nuclei, which stand out more clearly on the addition of acetic acid, and amongst them, some sparingly scattered, delicate, granular, pale, round and oval nuclei, and cells of the size of pus-corpuscles; colourless blood-corpuscles, lymph-corpuscles (the fibrinous globules of Mandl); the structures occurring in exudations, and termed plastic corpuscles (Bennett) or exudation-cells (Henle), also occur.

"The same composition is manifested by the soft, brine-like (sulzeartig) coagula of the so-called *parafibrin*, which occur either together with the above-mentioned firm coagula, or, under different circumstances, constitute the whole of the small amount of fibrin; and, in our opinion, form a transition-structure from albumen to fibrin, of high importance in the normal process of nutrition.

"2. Fibrin, whose coagula with the same external characters present a more highly-developed adhesive property, and frequently inclose a considerable quantity of serum; the microscope exhibits a discoid stroma, separable into flat or roundish, rough and solid fibres, or fibres similar to those of organic muscles; or else a membranous stroma very delicately fibrillated with undulating lines, and which, besides containing elementary granules, likewise presents numerous round, dark-edged, and sometimes rod-shaped or fibre-like nuclei; and after the expulsion of the moisture, granular, pale nuclei and corresponding nucleated cells. This fibrin, often associated with the rudiments of the previous sort, frequently enters into combination with those we have yet to consider.

"These two kinds of fibrin form the stroma of numerous pure or mixed areolar (cellular) or fibroid epigeneses, both within and external to the vascular system. The formation of tissue has its actual commencement in the process of coagulation. The fibrin (No. 2) is frequently effused in considerable quantity, in consequence of morbid processes, as for instance of inflammation. We might, therefore, distinguish it as a diseased form from the following forms, by the term plastic or organizable fibrin.

"3. Fibrin, whose coagula are distinguished by opacity, and by a pale white colour, merging into a yellow or yellowish-green. These coagula frequently contain not only serum, but likewise a considerable quantity of blood-corpuscles; from which an augmented capacity for coagulation may be inferred; they are then of various shades of red, and are opaque. The microscopic examination shows the coagulum to consist of a discoid or disco-fibrous stroma, or of a faintly-striped membrane; both are, however, opaque, in consequence of a large number of minute spots. In the serum we find a large number of nuclear structures and faint granular nuclei, and cells of a corresponding character in various stages of development. The whole coagulum frequently appears to be made up of the two last-named elements, with some fine granular matter. All the nuclei exhibit the ordinary indifference towards the action of acetic acid. The fibrin has a less adhesive character than the former.

"4. Fibrin exhibiting a greater degree of disease than that already considered. The coagula are in the highest degree opaque, and when they do not inclose any blood-corpuscles they have a yellowish-green tint. They frequently, however, contain a large quantity of corpuscles, and are then of a grayish-red or brownish colour—indications of a rapid coagulation. On more careful examination, they are found to consist of a thick, fine, granular mass of nuclei and cells, more or less approximating to the nuclei and cells of pus, held together by a tough, amorphous, connecting mass, without any fibrinous network or other species of fibrillation. They are more deficient in adhesiveness than even the third variety.

"The last two forms are named croupous fibrin.

"The fibrin here approximates to that occurring in pyæmia; it has the croupous constitution, and the nuclei and cells inclosed in the coagulum are true pus-nuclei and pus-cells.

"Other diseases of the fibrin are of no importance in relation to our present subject. They will be noticed in our remarks on the crases; as, for instance, the milk-white, opaque fibrin in medullary dyscrasia.

"On the one side, there is a tendency to the formation of tissue originally inherent in these forms of fibrin; on the other side, a tendency to (molecular) disintegration; each transformation having in fact already commenced. The fibrin (1) and (2) is organizable, while the fibrin (3) and (4) undergoes disintegration, and is only capable of transformation into tissue when mixed with a portion of the fibrin (1) and (2), as we frequently observe external to the vascular system, in exudations of this nature. In fibrin (4) there is no plastic coagulation.

"These forms of fibrin correspond in some measure with the different stages of oxidation of protein described by Mulder. Chemical analysis, in reference to these points is, however, as yet far behind anatomical facts." (pp. 142–5.)

The metamorphoses of the blastema are considered under two heads, according as it is solid or fluid.

The solid blastema originally appears either as a solid mass, or, if it incloses much moisture, as a lattice-work, with variously-shaped intervening spaces (Balkenwerk mit verschiedenartig gestalteten Lücken), a system of bars proceeding from a central mass, sometimes isolated, and sometimes anastomosing with another system so as to form a network, with meshes of an oval shape. This formation is seen, according to Rokitansky, in the opaque depositions in the inner coats of arteries, and in different fibrous tumours, especially such as occur on the dura mater.

The blastema may be amorphous or discoid, or it may be streaked in parallel lines. It may appear opaque, from the presence of elementary granules in various stages of development, or it may seem granular, or transparent. It may contain nuclei and cells in varying quantities; or it may be entirely free from them.

These forms of solid blastema become directly developed by a kind of cleavage (1) into fibres or fibrillæ of areolar tissue, or (2) into riband-formed, flat, rough, and for the most part felt-like, fibres, varying in breadth from the 100th to the 85th of a millimetre, and either dentated or rounded off at the edges; or (3) into fibres perfectly similar to those of organic muscle. The two last forms, by further cleavage, become converted into the fibres of areolar tissue.

After noticing some other cases, in which tissues or thin fibres are formed by cleavage of the blastema without the intervention of cells, he proceeds to add, that "fluid blastemata in their development into tissues follow Schwann's cell-theory. The perfectly-formed nucleated cell is, however," he observes, "formed in two ways:

"1. A nucleus is formed by the union of several elementary granules, and round this the cell is produced, with the nucleus imbedded in the wall (the ordinary mode); or, "2. The cell is first formed, and within it, from its originally clear contents, there are produced endogenous nuclei; as in blood, exudations, colloid and encephaloid." (p. 153.)

We have found considerable amount of original matter in our author's remarks on the formation of *fat* in the different tissues and organs of the body. There are certain parts of the body that seem to have a peculiar tendency to collect and accumulate fat.

1. Accumulations of fat appear around diseased kidneys, both such as contain calculi and such as are in a condition of secondary atrophy from Bright's disease; in the vicinity of anomalous osseous formations (being analogous to the medulla), as, for instance, around ossified arteries; around cancerous matter, &c.

2. In lipoma or fatty tumour we have an accumulation of adipose tissue, in the form of a roundish or compressed tumour, invested with a delicate cellular covering, which sends certain prolongations inwards; the size varying from that of hempseed or a pea to that of a man's head, or even larger.

These tumours consist of fatty tissue, made up of spherical fat-cells, in which we sometimes meet with star-like crystals of margarin or margaric acid. When the investing membrane is very strongly developed, the lipoma is said to be encysted. Encysted portions have been occasionally found in the interior of loosely-formed lipomata. Lipomata most commonly occur in the subcutaneous areolar tissue, at those parts where, in a normal condition, there is commonly most fat: as, for instance, on the buttocks, the thigh, the back and neck, and the region of the shoulder, and occasionally beneath the scalp. They are also met with in the submucous tissue of the stomach, the intestines, and even the bronchi; on both the parietal and visceral surfaces of serous and synovial membranes, most commonly, however, on the former surface; thus they are found beneath the pleura and peritoneum, on the inner surface of the dura mater, and on the lining membrane of the ventricles; also in the synovial sac of the knee-joint.

In the above cases, and likewise in that form of tumour to which the term steatoma is applied (and which, in reality, is merely a modification of lipoma), the fatty tissue is normal; there is no peculiarity in the fat-cells, and the chemical characters are those of ordinary fat. But there are also cases in which the contents of the fat-cells are abnormal in their constitution, and in which the structure of the cells deviates from the ordinary type.

These are arranged by Rokitansky in the following manner:—

"A. There are certain conditions under which adipose tissue presents anomalies that specially affect the constitution of the fat contained therein; thus,

"*a.* It sometimes occurs as a very thin fluid similar to olein. It is very greasy, oozes forth in large drops on being pressed or cut, and on examining the neighbouring organs they are often found to be infiltrated with fluid fat; it is usually of a deep yellow colour, similar to marrow.

"This character is very often noticed in the fat of aged persons, especially of those suffering from incipient osteoporosis (ex atrophia), accompanied with extensive arterial ossification [we use the term in its popular but incorrect signification], in cases of osteomalacia, and in persons of less advanced age affected with cancer. The fatty accumulations which supplant muscular flesh and the texture of the pancreas present a similar character.

"*β*. The fat contained in the cells is sometimes very firm, resembling stearin or mutton-suet, and clings to the blade of the knife. The whole fat of the body is sometimes of this character, especially that of the panniculus subcutaneus: in these cases the rete Malpighii generally secretes a considerable amount of pigment, and the secretion from the sebaceous glands is more abundant than usual. This character is particularly observed in the fat of young spirit-drinkers, and is almost invariably associated with fatty liver.

"This change may be dependent on an increased proportion of margarin in the fat, and it is very probable that stearin may also be developed. The use of alcohol is very strongly conducive to this morbid alteration.

"B. There are sometimes anomalies in the fat-cells, besides alterations in their contents.

"Here we must place Müller's cholesteatoma, which is always a locally circumscribed formation. The fatty mass, which is formed of cholesterin, consists of thin (sometimes concentrically-deposited), glittering, nacreous leaves or scales, which show, on closer examination, that they are composed partly of round and oval cells, but for the most part of polyhedric cells, resembling those occurring in plants, and with a diameter varying from the 80th to the 16th of a millimetre,—a texture corresponding with the cells in the fat of the sheep, which contain stearin, except that the cells are smaller and more delicate. Most of the cells exhibit no nucleus; but in a considerable number, especially of those that are round and appear young, it is distinctly visible.

"Between the layers of this polyhedric cell-tissue, we find crystalline depositions of fatty matters, chiefly in the form of rectangular tablets. Barruel has shown that they consist of cholesterin and a fat resembling stearin.

"Cholesteatoma is usually found inclosed in a fibrous cyst, or in a thin structureless or streaky membrane, lined internally by a delicate epithelium. In this encysted form we, in common with other observers, have noticed it in the subcutaneous areolar tissue, in bone (the femur), in the pia mater, and in the brain. Müller found it in cystosarcoma.

"It has likewise been observed free, in the form of a stratum, in urinary fistulæ (by Cruveilhier), and on the surface of an ulcerating mammary cancer by Müller and ourselves. In our case it occurred on the ulcerated surface of a cancer, combining the characters of the fibrous (scirrhous) and epithelial varieties." (pp. 284-5.)

The above remarks apply only to fatty tissues; our author now proceeds to the consideration of the morbid deposition of free fat.

Free fat is deposited under two separate and distinct conditions:

1. The fat is directly separated *as fat;* and

2. The fat is liberated, in consequence of the formation of new compounds, or is formed during the retrograde metamorphosis of the protein-compounds.

To the first class belongs the fatty degeneration of the liver noticed in the bodies of confirmed spirit-drinkers, and frequently associated with tuberculosis. In the case of spirit-drinkers, we very often find that the fat of the adipose tissue generally presents more or less of the character of stearin.

Here, also, we must place the fatty contents of encysted tumours.

These fats vary in their nature; thus in the common fatty liver a preponderance of olein seems the only peculiarity; but in some of its varieties, as, for instance, the waxy liver, the fat is more firm and consistent, and probably contains stearin and cholesterin.

In cysts, the fats often differ from those ordinarily occurring in the human body.

We regard the second class as very important, and we shall notice it at some length, because the subject is one which has not hitherto been in any degree sufficiently considered.

We may conclude that an actual conversion or metamorphosis of other tissues into fat is in the highest degree probable, we might almost say, certain, for the following reasons:

"*a.* Because we sometimes find that fat has supplanted structures, into whose composition it could not have entered in the injuriously large quantities in which it is actually found to occur.

"*b.* Because large quantities of fat occur at places and under circumstances, in which it is impossible that they can have been deposited there by any portion of the vascular system, since they are found in structures, which, on the one hand, possess no vessels, and, on the other, are too far removed from vascular structures (as, for instance, in exudations inclosed in dense, non-vascular, shrivelled sacs, and in depositions in arteries); and, finally, because they occur quite as frequently in the central portions of the structures in question, those furthest removed from the vascular system (for instance, in the coagula within the blood-vessels, or in tubercular granulations), as in the surrounding masses of crude fibrin.

"*c.* Because, lastly, the occurrence of the fat is accompanied by a complete metamorphosis (usually appearing as a disintegration) of the structures in question." (p. 287.)

The appearance of fat is, as Rokitansky justly remarks, a subject of so much interest, in the case of blastemata in the process of development, and in that of tissues already formed, that we shall offer no apology to our readers for extracting 'his remarks on this point, and noticing the leading instances in which it occurs. He arranges this appearance or occurrence of fat into no less than twelve subdivisions. These are—

"1. The occurrence of fat in the fibrinous clots of blood found in the veins, a consequence of a diseased condition of the serum of the blood, arising either spontaneously, or from infection with inflammatory products.

"2. The fatty metamorphosis of the coagula similarly developed in the capillary system—what is termed deposition or metastasis.

"3. The occurrence of fat in fibrinous and albuminous inflammatory products; as, for instance, in exudations, and especially in pus. This may be best observed in the exudations occurring on serous membranes.

"4. The fat occurring in the albuminous and fibrino-albuminous products, formed in the kidneys in Bright's disease. The spots at which the actual process of fatty transformation are proceeding, are generally perceptible to the naked eye as faintly white, fatty, shining, and generally rather turgescent points.

"5. The occurrence of fat in lardaceous infiltration of the liver. The lardaceous blastema appears at points to be opaque, and of a faint white, or whitish-yellow tint.

"6. The occurrence of fat in tubercle, during its incipient softening, as well as in the surrounding masses of crude fibrin as they also soften.

"7. The occurrence of fat in colloid substance, which is especially observable in colloid of the thyroid gland. In the more consistent forms of colloid it may be remarked by the naked eye, as forming opaque, pale white, or whitish-yellow spots.

"8. The occurrence of fat in cancer, where it forms emulsive compounds with albumen, and soapy, glutinous compounds with bases—the saponifying process of cancer. The points from which the process commences, are very frequently the yellow fibrinous masses, which either penetrate the mass in the form of a reticulum, or constitute enveloping accumulations.

"9. The occurrence of fat in which is commonly designated the atheromatous process, in the depositions on the inner surface of arteries, and in the soft groundwork of the concretions sometimes found in veins.

"10. The fatty conversion of the fibrous blastema and tissue in fibrous tumours and fibrous exudations.

"11. The fatty degeneration of the circular fibrous coat of the arteries, partly pure, and partly combined with, and dependent on, the deposition and its metamorphoses—atheroma and ossification.

"12. The occurrence of fat in the muscles of animal life, for instance in the heart, manifesting itself by an apparent conversion of the muscular fibrillæ into molecular fat, and accompanied by the disappearance of the transverse striæ, and by the swelling of the sheaths.

"Whether milky-looking blood, so far as this appearance depends on fat, and whether the secretion of fat from the intestinal canal, should find a place in this class, are points which cannot be decided without more accurate observation and investigation.

"The metamorphosis into fat affects (as must be understood from the remarks we have already made) crude fluid and solid blastemata, such as are in various stages of development into tissue, and tissues that are perfectly formed........ .... It must be regarded as a favorable event, since it reduces many kinds of tissue to a condition in which they can be more readily resorbed and reassimilated; further, it is the basis of the processes of involution, destruction, and death of the structure in question.

"In regard to the last property, it is very frequently combined with the so-called processes of ossification and cretefaction occurring in blastemata and tissues —processes which present a striking analogy.

"The fats occurring as the consequence of these metamorphoses may present many differences. In most cases, we find fluid fat in a finely-divided molecular condition, or in drops, which are sometimes comparatively large, clear, and strongly refracting, and sometimes more opaque, yellow, and glutinous. Cholesterin is very frequently found in a crystalline state in exudations, tubercle, colloid, cancer, and especially in atheroma of the arteries." (pp. 287–90.)

We proceed to the section on *cancer*, and regret that our limits will not permit us to extract more than the following brief remarks on the modes by which cancer kills, and on its occasional spontaneous natural cure.

"Cancer usually kills, at an earlier or later period, by exhaustion, which, however, may be brought about in various ways. Anæmia, tabescence, and, finally, inanition, are consequences of the exorbitant vegetation of the adventitious product in the form of the excessive growth of a solitary cancerous formation or of several such formations, of hemorrhage, or of ichorous discharge. Moreover cancer, like other adventitious structures seated in vital organs, may prove fatal by mechanically impeding their functions, as, for instance, in cancer of the brain. When cancer is very acutely and rapidly developed, it speedily kills by the hyperæmia of the vital organs which it induces; or, in the form of ichorous discharging cancer, it may cause death by infecting the mass of the blood, and giving rise to pyæmia.

"Rare as is the cure of cancer effected by extirpation, much more rare is its spontaneous cure. Amongst the modes of spontaneous cure, we must first notice that which depends on ulcerative destruction and necrosis, whereby the cancerous structure is sometimes gradually consumed, and the separate dead portions are thrown off, and sometimes rapidly killed and separated, in consequence of suppuration occurring around it, and separating it from the adjacent healthy structures; as, for instance, in cancers of the breast and uterus.

"But there are other modes of cure of higher interest than this, which give rise to an involution or destruction of cancer. These are—

"1. The saponification of cancer, a metamorphosis which is usually induced by the disintegration of the substance constituting the reticulum, and in part consists in the liberation of fat in the form of granular cells, or in the conversion of the protein-compounds of the cancer into fat, with the emulsive and soapy compounds arising from these changes........................It especially occurs in encephaloid medullary carcinoma, which is remarkable for the quantity of fat and of crude convertible albumen which it contains; and is most common in the liver and uterus.

"2. Corrugation or obsolescence of cancer, with condensation and solidity of its blastema, and liberation of the calcareous salts in the form of free molecules and incrustations on the cells. It affects, for the most part, the more dense forms of cancer, those with a solid blastema (or intercellular substance), as solid medullary and fibrous cancer.

"Ossification and cretefaction often give the first impulse to inflammatory products in cancer. Here also the ossification and cretefaction (of the reticulum) are associated with the former change, namely, with the saponification." (pp. 351-3.)

The next point to which we shall allude is *tubercle;* and, from the high authority of our author, and the deep interest attached to the subject, we shall endeavour to give Rokitansky's principal views, in as concise a form as is compatible with perspicuity.

There is tubercle, he observes, with very few exceptions, where there is an endogenous separation within the vascular system,—in short, an exudation in the widest sense of the word; an exudation of solidified protein-compounds—fibrin and albumen—which remains as the blastema in the lowest stage of development; that is to say, in its primitive condition, as yielded by the solidification of the so-called crudity. It forms a sort of transition to the unorganized new formations.

This exudation is for the most part produced in the form of either isolated or accumulated nodules; or, if it occur in very large quantity, it appears in the form of granulations or gland-like masses; hence, at its very first appearance, it may be recognised as tubercle. In making that general statement regarding the primary form of tubercle, we must not forget, on the one hand, that other structures—as, for instance, gelatinous and fibrous cancer—sometimes occur in this form; nor, on the other, that tubercle sometimes appears in irregular masses of considerable size. He divides tubercle into three species:

1. Simple fibrinous tubercle.
2. Croupo-fibrinous tubercle.
3. Albuminous tubercle.

The simple fibrinous tubercle occurs in the form of granulations, about the size of a millet-seed, which are either separate and distinct, or accumulated in gland-like mass; or else it appears on serous membranes as a product of inflammation, in the form of smooth pseudo-membranous exudations.

In the first of these two forms it constitutes the gray semi-transparent granulations of Laennec.

A tubercle, at first sight, appears as a little roundish substance, but, on closer examination, is seen to send out ramifications from its periphery. It has no other connexion with the surrounding tissues than that it is deposited between their histological elements, occasionally inclosing some of them; as, for instance, when it occurs on serous surfaces. In density and colour it is tolerably uniform; when rubbed between the fingers it sometimes communicates a granular or fibro-granular sensation to the hand, while in other cases it breaks down into a soft uniform substance of a more or less deep gray colour.

On instituting a microscopic investigation, it is found to consist principally of a more or less transparent groundwork (blastema), which forms the medium of connexion for certain histological elements.

The *groundwork* is a discoid or disco-fibrous, solid, gray blastema, which swells and becomes transparent on the addition of acetic acid.

The *histological elements* are—

a. Elementary granules of the most varying sizes.

b. Nuclei, black at the edges, round, or oval, or even prolonged into the form of a rod; also delicate granular combinations of nuclei.

c. Nucleated cells of anomalous irregular form, but frequently present in great number.

Besides these, we usually observe the histological elements of the tissue in which the tubercle is situated.

Tubercle on serous membranes is the purest and best adapted for these investigations.

Whether tubercle is vascular or non-vascular, is a question into which our limited space will not allow us to enter. The conclusion at which Rokitansky arrives is, that the purity of tubercle is proportional to its freedom from vascular structures.

> "The metamorphosis which this form of tubercle undergoes is limited to obsolescence. It takes place in this manner: after the tubercle has passed through its condition of crudity, it loses its shining appearance, and increases in density, becomes converted into a small hard lump, and then shrivels into a tough, amorphous, or slightly fibrous, horny mass—cornification. It forms the basis of a complete destruction or death of the tubercle, and no further metamorphosis can take place. This process is sometimes accompanied with more or less of ossification, in which case the remains of the tubercle consist partly of a horny mass, and partly of bone-earth." (p. 397.)

This form of tubercle is incapable, according to Rokitansky, of undergoing any form of metamorphosis.

If softening has commenced, we may be sure that, instead of examining a specimen of pure fibrinous tubercle, we are inspecting a combination of it with croupo-fibrinous tubercle. After this statement, it is unnecessary to remind our readers that he looks upon Laennec's description of the softening of gray tubercular granulations as altogether erroneous.

Croupo-fibrinous tubercle generally occurs in the form of roundish nodules,—in the form of irregular, nodulated, ramifying masses of considerable size,—and, when it is present on free surfaces, in the form of glandular nodulated layers of varying thickness.

On examining a portion we may describe it as opaque, and of a yellow tint; a section presents, according to circumstances, either a fibrous or a granular appearance; it may be tough or friable, and has a lardaceous or caseo-lardaceous character. To distinguish it from the former, we term this *yellow tubercle.* Its microscopic characters hardly differ from those of gray tubercle, except that the stroma is interspersed with an enormous number of the finest conceivable specks of matter (punctmasse). Its metamorphoses embrace *softening* and *cretefaction.*

> "1. The softening or suppuration proceeds in the following manner. After the tubercle has existed for some time in the state of crudity, it becomes, as it were, loosened in its texture, and usually increases in volume; it breaks up on slight pressure, and becomes more moist; then changes into a yellowish, dissolving, casein-like, fatty, and viscid matter, and finally breaks up into a thin,

whey-like, acid-fluid, in which flocks and shreds—the remains of the imperfectly disintegrated tubercle—are observed swimming. This is tubercular pus......

"The softening consists in the solution and disintegration of the solid groundwork of the tubercle into a fluid containing an abundance of the minutest molecules. This change is followed by a separation and isolation of the above-named histological elements of tubercle, which undergo more or less marked alterations by their immersion in the fluid; the cells become distended, corroded, and are finally dissolved; the nuclei shrivel, and assume irregular forms, becoming angular and indented. Finally, in softened tubercle we meet with free fat.

"The dissolved tubercle consists—

"*a.* Of a fluid with minute molecules.

"*b.* Of altered nuclei and cells isolated in the manner we have described.

"*c.* Of free fat in the form of elementary granules, and distinct globules of a larger size.

"2. The other metamorphosis of this form of tubercle is cretefaction. It occurs as a secondary change, never attacking tubercle in its original form, but confining itself to the dissolving or dissolved tubercular blastema.

"In softening or softened tubercle, the calcareous salts and fat occur in the form of free, distinct, or aggregated elementary molecules, or in granular cells; the fat also not unfrequently occurs in the form of large drops or of crystals of cholesterin. In this process the softened tubercular mass gradually thickens into a moist, fatty, viscid kind of plaster, and, finally diminishing in volume, is converted into a mortar-like concretion." (pp. 398–400.)

The following varieties of these cardinal forms deserve notice:

There are several varieties of croupous tubercle analogous to those of croupous fibrin, and the exudations which are formed by it. In this form of tubercle they are shown by the varying degrees of opacity, coloration, consistence, and solubility, the corrosive power of the ichor, and the quantity of histological elements, minute molecules, nuclei, and cells contained in it.

Again, the exudation forming the blastema for tubercle is very commonly not altogether pure and unmixed. The uniting of the two cardinal blastemata in every degree of commixture, the combinations of the varieties of blastemata of croupous tubercle, and, finally, the combinations of all these with the blastema capable of undergoing organization (fibrin), are capable of giving rise to innumerable varieties of tubercle.

Moreover, gray tubercular granulations may present great differences in relation to transparency, colour, &c.

With regard to the seat of tubercle, it may be any point of any tissue external to vessels; in short, wherever there is a capillary system, there tubercle may be separated. It must be deposited at, or very near to, the place of exudation, in consequence of the highly coagulable nature of the blastemata; hence it cannot occur in such tissues as cartilage, which are merely nourished by a prolonged saturation of their substance by plasma.

Rokitansky admits the possibility of the formation of tubercular deposits within the blood-vessels, but regards such occurrences as extremely rare.

The apparent causes of the development of tubercle in an exudation are:

1. A deficiency of vivifying influence received by the blastema from the surrounding tissues and the system generally.

2. The want of sufficient moisture in the blastema.

The next subject to which we shall advert is *the relation of tubercle to other morbid processes*. We shall follow Rokitansky's arrangement, but, as far as possible, condense his remarks.

1. Cysts are very rarely found in the same organ with tubercle, or even in the same individual. When exuberant cyst-formations occur in the abdominal cavity, as, for instance, in the ovaries, they tend in a secondary but very important way, to secure immunity from phthisis by diminishing the thoracic cavity. When one process has entirely ceased, the other may develope itself; and, in opposition to the hydatid theory of tubercle, the formation of cysts much more frequently follows the extinction of tubercles than conversely.

A comparison of the occurrence of these two processes in different organs shows the extraordinary frequency of tubercles, and the extreme rarity of cysts, in the lungs; while in the ovaries, and, in a less degree, in the salivary glands, the opposite relation exists.

2. A similar, although less preponderating, relation exists between tubercle and carcinoma. When both have occurred together, we generally find that the cancer has followed the tuberculosis; and it seems much rarer for tubercle, or the crasis that leads to its development, to supervene on exterminated cancer. Rokitansky observes that, simultaneously with inflammatory and ichorous cancer, there are often developed in the lungs tubercles of a whitish colour and soft gelatinous consistence, which break up into a whitish creamy ichor, indicating a croupo-tuberculous and cancerous state of the fibrin.

The following table indicates the relative degrees of cancer and tubercle in the different parts of the body:

FREQUENT.	RARE.
Pulmonary tuberculosis . . .	Pulmonary cancer.
Ovarian cancer	Ovarian tubercle.
Cancer of the salivary glands .	Tuberculosis of the salivary glands.
Cancer of the stomach . . .	Tuberculosis of the stomach.
Cancer of the œsophagus . .	Tuberculosis of the œsophagus.
Cancer of the rectum	Tuberculosis of the rectum.
Tuberculosis of the ileum . .	Cancer of the ileum.
&c.	&c.

Amongst other facts connected with the favorite or special localities of diseases, Rokitansky refers to the uterus, in which the vaginal portion and cervix are liable to attacks of cancer, while tuberculosis invades the mucous membrane of the body of the uterus, and, as a general rule, never extends beyond the orificium; to the circumstances that, while tuberculosis attacks the epididymis, the testicles themselves are liable to cancer; and that, while the upper parts of the lung suffer principally from tuberculosis, cancer attacks equally all portions of the parenchyma. Again, when cancer and tubercle both affect the same organ, it is important to recognise the primary and the secondary affection; thus in the liver, cancer is not unfrequently primary, the tuberculosis being merely one of the phenomena of that affection generally pervading the body.

3. *Typhus and tuberculosis.* It is only when typhus is raging as a very severe epidemic, that it attacks persons labouring under tuberculosis; but, on the other hand, croupo-fibrinous tubercle is no unfrequent consequence of typhus. Thus, as the disease is abating, pneumonia and inflammation of the follicles of the small intestine, which have escaped injury from the typhus-process, yield foci for tubercular depositions; and we often meet

in these cases with intestinal ulcers presenting the combined characters of typhus and tuberculosis.

The occurrence of tuberculosis as a result of typhus, is undoubtedly dependent upon the change (which, by the way, is a very frequent one) from the typhous to the croupo-fibrinous crasis.

There is a similar relation between tuberculosis and acute exanthemata,—scarlet fever, and measles. The supervening tuberculosis is generally croupo-fibrinous, and is based on a corresponding change of the exanthematous process.

4. Tuberculosis and intermittent fever are known to exclude one another.

5. Bronchocele and tuberculosis exert an excluding influence, which seems due in part to the close affinity between the diseased thyroid gland and cysts, sarcoma, and cancer, and in part due to the mechanical impediment which the tumour opposes to free respiration. We are apparently justified in concluding that the same primary anomaly of crasis which may give rise to bronchocele, checks the development of tubercle; for in the countries where bronchocele is endemic, tuberculosis does not exhibit itself even in those who are unaffected with the bronchocele.

6. Rachitis and tuberculosis are very rarely associated; in fact, when we have rachitic malformation and contraction of the thorax, tuberculosis is scarcely ever found to exist. Whether, or to what extent, rachitis has in itself the power of excluding tuberculosis, or whether the immunity is dependent only on secondary causes, to wit, the malformation and contraction of the chest, are points on which our author can give no decided opinion.

7. Arterial disease, giving rise to spontaneous aneurism, is, in its higher degrees, very rarely combined with tubercle. The disease consists in the endogenous separation and deposition of a fibrinous structure on the inner wall of the artery; and it is probable that the immunity from tuberculosis is dependent on the circumstance that, after this separation of fibrin, there is a lack of material in the blood for the formation of tubercle. A more certain immunity is afforded by such as tend, by promoting the coagulation of large clots, to defibrinate the blood, and to give rise to hydræmia; or by a large aneurism in the neighbourhood of the heart, which may produce a mechanical action.

8. In a practical point of view, the excluding influences of venosity and cyanosis, arising from mechanical obstacles in the heart and lungs, are of the highest importance. The peculiar immunity which they afford against tuberculosis has led Rokitansky to devote several pages to this subject. We shall endeavour to give the pith of his remarks.

Venosity hinders the arterialization of a sufficient quantity of blood, while cyanosis is a check to the emptying of the venous system into the right side of the heart, so that venous blood is retained in the veins and capillaries. The facts bearing on this subject are arranged in two classes, according as the venosity and cyanosis are dependent on the heart or the lungs.

a. In the first place, it is well known that persons suffering either from primary enlargement of the heart (dilatation, hypertrophy, or their combination), or from secondary enlargement, induced from disease of the valves, do not suffer from tuberculosis.

b. In like manner tuberculosis does not appear to attack those who have secondary enlargement of the heart, consequent on congenital deficiency of the heart or larger arteries, persistence of the ductus arteriosus, &c.,—changes which rise to venosity and cyanosis.

c. In the same way, an immunity from tuberculosis is conjoined with several acquired anomalies of the large arterial trunks, which are similar to the above-named congenital defects; as, for instance, coarctation from the compression often induced by large aneurisms in the neighbourhood of the heart. Independently of any reference to the observations in the preceding paragraph, the immunity is here based on the mechanical impediment which the augmented column of blood in the distended aorta opposes to the due emptying of the left ventricle, and on the consequent opposition that is presented to the entrance, or rather to the exit, of venous blood into or from the right side of the heart, caused by the congested state of the pulmonary capillaries.

The same immunity is afforded by venosity and cyanosis, induced by impediments to the pulmonary circulation; and the degree of immunity afforded seems proportional to the extent of the dilatation of the right side of the heart.

d. We learn by observation that the increased density of the lungs, owing to a contraction of the thoracic cavity in the higher degrees of lateral curvature of the spine—the pigeon-breast of rachitic patients,—exerts an excluding influence on tuberculosis; and it is an important fact, that when malformation of the spinal column (kyphosis) has been removed, even if it arose from tubercular caries of the vertebræ, the tuberculous crasis is entirely eradicated in consequence of the contraction of the thoracic cavity.

e. Compression, produced by a pleuritic exudation, and a consequent persistent increase of density of one lung, owing to the sinking-in of the thorax, check the tendency to tuberculosis, in a degree corresponding with the abnormality of the relation between the mass of the blood and the amount of mechanical impediment to its entrance into the pulmonary capillaries, and to the deficiency of power possessed by the other lung to effect the arterialization of sufficient blood.

f. The fact that pregnancy not only checks the advance of existing tuberculosis, but also excludes its development, may be thus explained. As the abdomen enlarges, the thoracic cavity becomes encroached upon, and the parenchyma of the lungs being exposed to pressure, a condition of venosity results. This is doubtless the reason why the fœtus is scarcely ever, and the placenta very rarely, tuberculous.

The rapidity with which tuberculosis developes itself after delivery—after the removal of the cause inducing venosity—renders this illustration especially interesting.

g. A similar immunity from a similar cause is afforded by all kinds of abdominal enlargements; as, for instance, by large ovarian cysts.

h. Original smallness of the pleural sacs and of the lungs, when associated with a considerable development of the abdomen and its viscera, acts as a safeguard against tuberculosis.

i. In very early childhood pulmonary tuberculosis is rare, owing to the density of the lungs produced by the comparative magnitude of the abdominal viscera.

k. Persons suffering from chronic catarrh, from vesicular emphysema and bronchial dilatation, were known, in Laennec's time, to enjoy an immunity from tuberculosis; and the knowledge of this fact has led to various proposed methods for curing existing tuberculosis, by the artificial establishment of such conditions. The empirical modes of treatment to which we advert signally failed, because those who employed them were utterly ignorant of the real healing agent, namely the venosity, which in catarrh is the consequence of the deficiency of function, of the collapse and ultimate destruction of numberless pulmonary lobules, arising from the bronchial twigs being closed by blennorrhœal secretion; which, in dilatation of the bronchi, is the result of similar destruction of the lobules; and which, in the case of emphysema, depends on impeded respiration, owing to the diminished contractility of the pulmonary tissue, (in expiration,) and on the destruction of certain portions of the pulmonic capillaries.

9. The dropsical crasis, arising from venosity, in like manner excludes tuberculosis.

There can be no doubt that, although the rules laid down in the preceding pages are generally correct, they admit of exceptions; and we shall now proceed to give Rokitansky's explanation of the usual causes of these exceptions.

These exceptional cases usually occur as instances of tubercle combined with cancer, or of tubercle associated with one or other of the forms of venosity noticed in Par. 8.

The following observations will perhaps elucidate these cases:—

1. The conditions excluding tubercle are essentially dependent on the simple fact of their giving rise to an excess of fibrin. This, however, does not prevent the occurrence of tubercle in an exudation containing only a little fibrin.

2. Tubercle may be the result of a local inflammatory stasis, in which the fibrin becomes tuberculous.

3. A consecutive tuberculous crasis may be developed as an intermediate symptom, after whose exhaustion by the induced depositions the primary crasis recurs: the whole mass of the fibrin being diseased.

4. In relation to the observations made in Par. 8, it must be observed, that it requires a certain degree of venosity to ensure perfect immunity from tuberculosis. What this exact degree is, we have no means of determining. There can, however, be no doubt, that the relation existing between tuberculosis and the venosity induced by mechanical hinderances in the heart and lungs, is of the highest importance, not only in pointing out that tuberculosis is based on a fibrinous crasis, but also from its affording a very valuable guide in connexion with the mode of treatment.

The importance of the subject, and the originality of Rokitansky's views regarding it, induce us to proceed yet further in his company, and to give his observations "on the occurrence of tubercle in the different tissues and organs, and on the natural modes of cure." In order to enter clearly into these points, he deems it expedient to express his views regarding the much-disputed question of the identity or non-identity of tubercle and scrofula. We are glad to find him a powerful advocate for the identity, both of the processes and of the resultant deposits.

The following are the grounds of his belief:—

> "1. They present the same elementary composition, both anatomically, and, as far as we can yet see, chemically. The identity seems specially striking on comparing scrofulous matter with yellow tubercle.

"2. Both undergo the same metamorphoses, namely, softening and cretefaction.

"3. The tuberculous and the so-named scrofulous ulcer are entirely similar, not only when they occur on the same organ, but when they occur on different organs and affect different structures, as in scrofulous ulcers of the skin, and tuberculous ulcers of the intestines; and the same is the case with respect to their cicatrices.

"4. Both occur in the same organ, sometimes with, and at other times without, the symptoms of inflammation, and are very often found associated. . . .

"The same substance will often be called tubercle or scrofula, according as it occurs in the lungs or in the bronchial glands." (pp. 434–5.)

Every one knows that some organs are much more liahle to tubercular deposits than others. From his personal experience (extending, it is reported, to *thirteen or fourteen thousand cases*) he has drawn up the following table, in which each organ or tissue is placed according to its liability to tubercle in adult life:

Lungs.
Intestinal canal.
Lymphatic glands, especially the abdominal and bronchial glands.
Larynx.
Serous membranes, especially the peritoneum and pleura.
Arachnoid membrane.
Brain.
Spleen.
Kidney.
Liver.
Bones and periosteum.
Uterus and fallopian tubes.
Testicles, prostate and seminal vessels.
Spinal cord.
Muscles of animal life.

In children the scale is somewhat modified. The lymphatic glands and the spleen stand first, and are succeeded by the lungs and bronchial mucous membrane, the brain, serous membranes, &c.

"The following remarks," says Rokitansky, "appear of the highest importance, and at the same time show the insufficiency of an ordinary table of relative frequency.

"1. Tubercles can only occur wherever there is a capillary system; hence, in epidermal structures and in cartilage there can be no tuberculosis. And there are some vascular organs in which tubercle scarcely ever occurs, as, for instance, the salivary glands, the ovaries, the inner coats of vessels, the œsophagus, and the vagina.

"Vascular new-formations are occasionally the seat of tubercle.

"2. It must be obvious, that it is highly important to arrange the individual tuberculoses according as they are primary or secondary. There can be no doubt that, in relation to primary tuberculoses, the lungs and lymphatic glands claim the first place; but the tuberculoses of the uropoietic system, the female sexual mucous membrane, the bones, the testicles, with the prostate and seminal vesicles, also rank high, although in the scale we have given above they fall very low; while all the others,—tuberculoses of the intestine, larynx and trachea, serous membranes, spleen and liver,—hold a very secondary rank, since they very seldom, indeed scarcely ever, occur as primary affections.

"3. It must be observed that many of the tuberculoses which are high in the above scale, are of comparatively slight nosological importance, since they are always secondary, and dependent on other tuberculoses, being, in fact, often local phenomena of general tuberculosis; as, for instance, tuberculosis of the liver, spleen, kidneys, and even many tuberculoses of the lymphatic glands.

"4. Tuberculoses of several given organs form an almost constant original or speedily consecutive combination; thus tubercle of the brain is associated with tuberculosis of the lymphatic glands; tuberculosis of the testicles, prostate and seminal vesicles, with tuberculosis of the uropoietic system; tubercle of the spleen and supra-renal capsules with tuberculosis of the lymphatic glands; tuberculosis of the fallopian tubes and uterus with tuberculosis of the peritoneum; and pulmonary tuberculosis with tuberculosis of the intestines and larynx.

"5. Secondary tuberculoses have, as it were, their foundation or starting-point in certain pre-existing tuberculoses, that is to say, they succeed to certain already formed tuberculoses, in accordance with a tolerably constant rule. Thus tuberculosis of the lungs, or of the lymphatic glands, forms a general starting-point for all secondary tuberculoses.

"Tuberculosis of serous membranes is associated with tuberculosis of the corresponding parenchyma, tuberculosis of the male uropoietic system with that of the genital apparatus, and so on.

"6. There are differences in connexion with the mode of production of tubercle in different organs; thus, in serous membranes and in bones, tubercle is generally an inflammatory product; in mucous membranes and in the lungs it is very frequently so; while this is comparatively rarely the case in tubercle of the lymphatic glands or of the brain.

"7. Finally, it is worthy of remark, that in each organ tuberculosis exhibits a tolerably constant preference for attacking a particular point.

"Thus in the lungs, the tuberculous deposit is first separated and concentrates itself in the apices and upper third of the superior lobes; in the pia mater the portion investing the base of the brain, from the optic commissure to the pons and medulla oblongata, is the part attacked; the same is the case in the brain with the gray matter and the structure contiguous to it; in the osseous system with the spongy bones; in the intestinal mucous membrane with the lower part of the ileum; in the laryngeal mucous membrane with that portion which invests the musc. transv. glott.; in the testicles with the epididymis; and in the female sexual apparatus with the mucous membrane lining the tubes and the fundus of the uterus.

"8. On the other hand, tuberculosis in its progress meets, in some few cases, with limits beyond which it cannot pass; thus tuberculosis of the larynx scarcely ever extends to the pharynx, and uterine tuberculosis is limited by the orificium uteri, and leaves the cervix and vagina unaffected." (pp. 436–8.)

Passing over his remarks on the different modes in which this affection may prove fatal, we arrive at *the cure of tubercle.*

"The cure of tubercle may occur in various ways; in fact each of the metamorphoses of tubercle we have described may conduce to a curative process.

"The destruction of tubercle and its elimination by an ulcerative process as a local means of cure, can be of no value whatever, unless the crasis on which the tubercular secretion depends be at the same time overcome.

"The cure of tuberculosis, as a general disease,—as tubercular dyscrasia—sometimes occurs under tolerably palpable conditions, which are included in some of the above-mentioned processes and states that exclude tubercle, although it very frequently happens that the conditions under which it occurs are perfectly unknown.

"We must commence our remarks on the local curative process of tubercle, by considering whether crude tubercle can be resorbed, and thus disappear.

The resorption of tubercle was first denied by Laennec, and since his time pathologists have for the most part held the same opinion; and we must admit that, although there are no grounds for denying the possibility of its resorption, there is, on the other hand, no strong proof in favour of the view, which is, at all events, very improbable.

"The obsolescence and horny induration of gray tubercle present undoubtedly the most rapid process for its destruction, and would be the best method of cure, if it were not that the structure affected might not prove injurious, so long as it remained in its original condition.

"Of the two other metamorphoses of yellow tubercle, cretefaction of the softened mass affords the most desirable process of cure; as we shall see from the following considerations of the other metamorphosis as a healing process, or rather as the groundwork of such a process.

"Softening of tubercle cannot, in itself, be regarded as a curative process; it is further requisite that the softened tubercle should be eliminated by a process of ulceration through the surrounding parts. In relation to this mode of cure it must be observed—

"*a.* That it can only occur by ulcerative destruction of tissue.

"*b.* That the ulcerous process should have a healing tendency; since, in a great number of cases of tubercle, it leads to exhaustion and death.

"*c.* That until the general disease is eradicated, the accompanying inflammation propagates tuberculous products, and in this way is conducive to a further ulceration and destruction of tissue.

"*d.* And finally, that even in favorable crases, the mass of the blood may become infected in tuberculous ulcers (caverns).

"From these observations it appears that this process of healing is far inferior to that by cretefaction; it is more complicated, since it can only be effected by the ulcerative destruction of tissue; and it stands in the same relation to the latter, as the removal of a foreign body imbedded in an organ by an exhausting ulcerative process, stands to its being rendered harmless by becoming encysted.

"The cure of tuberculous ulcers (caverns), of a tuberculo-ulcerous process, or of tuberculous phthisis of an organ, can only be effected when the accompanying inflammation, after the removal of the tuberculous crasis, ceases to produce new tubercles, and yields organizable products in their place.

"Shrivelled cicatrices supply the loss of substance; and if the tubercle in the process of phthisis be not perfectly eliminated, it may become encysted by a kind of cicatrix-tissue, in the condition of pultaceous tubercular pus, and become finally cretefied." (pp. 438-41.)

The length to which this article has already extended almost precludes us from offering any remarks on *Albuminous tubercle, or acute tuberculosis,* —a disease which, although presenting many analogies and points of resemblance to the forms of tuberculosis already described, differs from them in many striking particulars. It is a disease in which tubercle is very rapidly developed, differing in several points from fibrinous tubercle, and in which the symptoms are of a highly typhoid character. Its course is rapid, and invariably fatal. It may attack individuals previously free from tubercles, but generally occurs in the course of a more chronic form of tuberculosis.

The second portion of this chapter, embracing the consideration of "unorganized epigeneses," is inferior to the corresponding portion of Vogel's 'Pathological Anatomy,' a work with which our countrymen are familiar, through the medium of Dr. Day's translation.

The same remark is applicable to the earlier portion of the TENTH CHAPTER, the last of the volume. On the subjects of pneumatosis, dropsy, and parasites, we think that Vogel's work is decidedly the more instructive; but of the concluding portion—the last seventy-two pages—of the volume under our consideration, we cannot speak too strongly. It is entitled "*The diseases of the blood* (*dyscrasiæ*)"; and it is a source of much gratification and rejoicing to us, who have long firmly believed and openly maintained the humoral origin of most diseases, to find that we are supported in these views by so experienced and so distinguished an observer. We cannot, however, enter on this topic in the present article; and we regret this the less, because we shall shortly have an opportunity of noticing it at greater length, in connexion with some other recent works on this subject.

It is needless for us to reiterate our opinion of the value of Rokitansky's labours. That they are duly appreciated is obvious, from the facts that two large editions of the volumes on 'Special Morbid Anatomy' have been exhausted in his own country, and that a translation (as we previously observed) has been commenced in America; and it affords us no slight gratification to be enabled to announce, on undoubted authority, that a translation will shortly appear in this country, under the auspices of the Sydenham Society. We congratulate that excellent and flourishing Society on having selected a work of so high and sterling a character: their choice is equally creditable to themselves, and to the illustrious German pathologist. We congratulate our medical brethren on the prospect of shortly receiving from that Society a work, universally acknowledged to be unrivalled in its own department, at a cost considerably less than the price of the original volumes. But we cannot extend our congratulations to the gentlemen to whom the translation is intrusted. Their task is as difficult as it is important. We are not aware that anything has been heard of Dr. Peters, since the publication of his solitary fasciculus: we feel assured that a better fate will attend our enterprising friends at home; and that, in some two years' time, it will be our pleasing duty to announce the completion of the great work which they have undertaken. We believe that its publication will exert a salutary influence on the studies of many of the active working pathologists of this country. It will afford them a safe standard with which to compare their own observations; it will show them the present and actual state of knowledge that is possessed on this subject; and it may prevent them from subjecting themselves to the annoyance of being taunted by foreign observers, as second-hand discoverers.

If we have referred but slightly to Engel, it must not be presumed that we think disparagingly of his labours. Both his works are clearly written, and contain much valuable matter. But there are few points in which he differs essentially from his great master; and we have preferred drawing our information from the fountain-head.

ART. X.

1. *Rare and Remarkable Animals of Scotland, represented from Living Subjects; with Practical Observations on their Nature.* By Sir JOHN GRAHAM DALYELL, Bart. Vol. I, containing Fifty-three coloured Plates. —*London*, 1847. 4to, pp. 270.
2. *Fauna Littoralis Norvegiæ; oder Beschreibung und Abbildungen neuer oder wenig bekannten Seetheire, nebst Beobachtungen über die Organisation, Lebensweise u. Entwickelung derselben.* Von M. SARS, Doctor der Philosophie, &c. &c. Erstes Heft. Mit 10 Kupfertafeln.—*Christiania*, 1846.

The Littoral Fauna of Norway; or, a Description and Representation of new or little-known Marine Animals, together with Observations upon their Organization, Habits, and Development. By M. SARS, Doctor of Philosophy, &c. &c. First Part. With Ten Copper-plates.—*Christiania*, 1846. Folio, pp. 94.

3. *Mémoire sur le Développement des Méduses et des Polypes Hydraires.* Par M. FELIX DUJARDIN. Extrait des *Annales des Sciences Naturelles*, Novembre, 1845.

Memoir on the Development of the Medusæ and Hydraform Polypes. By M. FELIX DUJARDIN; from the *Ann. des Sci. Nat.*, Nov. 1845.

4. *Mémoire sur les Campanulaires de la Côte d'Ostende, considérés sous le Rapport Physiologique, Embryogénique, et Zoologique.* Par P. J. VAN BENEDEN, Professeur de Zoologie et d'Anatomie Comparée à l'Université Catholique de Louvain, &c. &c. Extrait du Tome XVII des *Mémoires de l'Académie Royale des Bruxelles.* Avec 3 Planches.—*Bruxelles*, 1843.

Memoir on the Campanulariæ of the Coast of Ostend, considered in their Physiological, Embryological, and Zoological Relations. By P. J. VAN BENEDEN, Professor of Zoology and Comparative Anatomy in the Catholic University of Louvain, &c. &c.—*Brussels*, 1843. 4to, pp. 42.

5. *Recherches sur l'Embryogénie des Tubulaires, et l'Histoire Naturelle des différents Genres de cette Famille qui habitent la Côte d'Ostende.* Par P. J. VAN BENEDEN. Extrait du Tome XVII des *Memoires de l'Académie Royale de Bruxelles.* Avec 6 Planches.—*Bruxelles*, 1844.

Researches on the Embryogeny of the Tubulariæ, and on the Natural History of the different Genera of that Family which inhabit the Coast of Ostend. By J. P. VAN BENEDEN.—*Brussels*, 1844. 4to, pp. 72.

THE science of Natural History has taken, within the last few years, a new and most important direction. The Zoologist and Botanist have, until recently, devoted by far too large a proportion of their time and attention to the mere business of collecting species, of defining their external characters, and of framing systematic arrangements based almost exclusively upon these. The Anatomist and Physiologist, who have looked more deeply into the mysteries of organization, who have investigated the internal structure and the living actions of these beings, and who have urged the necessity of basing all classification upon the points of agreement or difference in fundamental characters thus brought into view, have been looked upon too often as well-meaning enthusiasts, who put themselves and others to a great deal of unnecessary trouble, and whose labours will tend to confuse what is already simple, to disturb what has long since been established. Thus a degree of isolation, and even of opposition, has grown up between the votaries of natural history, which has greatly tended to retard the progress of the science. What God has joined, man has foolishly striven to put asunder. The mere systematist has rejected, as long as he could possibly do so, the friendly overtures of the physiologist; and the physiologist has repaid this distrust by a not undisguised contempt for the mere species-collector.

The labours of the Jussieus, De Candolle, Robert Brown, and their followers have, however, effected a complete metamorphosis of Botanical science. The Linnæan system of classification, to which so many have fondly clung, almost as tenaciously as if its existence were necessary to their own, is now universally admitted by scientific men to be what its great author expressly declared *he* intended it to be; namely, a means which might give temporary aid in the arrangement of the genera composing the vegetable kingdom, until the collection and comparison of these should have been sufficiently effected to admit of the formation of a really natural classification —that is, of an arrangement founded upon a comparison, not of one or two external characters only, but of the entire assemblage of characters furnished by internal structure and by the form, number, &c. of the external parts. To the determination of the internal structure, therefore, the labours of the scientific botanist are now directed; and he can scarcely prosecute his inquiries on this subject very far, without finding himself involved in physiological investigations respecting the actions of the several parts, and the history of their growth and development. This is particularly the case in regard to the simplest forms of cryptogamic vegetation, which—consisting entirely of cells, and frequently of cells so independent of each other and complete in all their actions, that each one may be regarded as a distinct and separate plant—present us with the phenomena of cell-life in their least complicated state. We rejoice to know that upon these most interesting groups the attention of many skilful and industrious observers is now fixed; and we look to the results of their researches, with a full confidence that some of the profoundest mysteries of organization will be made plain by their means.

The same change is taking place in the study and pursuit of Zoology. Although he was in many particulars anticipated by our own Hunter, yet it is undoubtedly to Cuvier that we owe the first formal and complete enunciation of the great general truth, that the classification of animals must be founded upon the assemblage of characters furnished by their entire organization, if we would have it bear any approach to a really natural method. Still many eminent zoologists, even of our own day, have continued to uphold the pre-eminent value of external characters as guides in the classification of the animal kingdom; and have repressed, so far as they have been able, the attempts which have been made, from time to time, on the part of the anatomist and physiologist, to introduce methods more accordant with the character of that great plan, which it is the object of zoological study to unveil and comprehend. But various circumstances have lately conspired to force these disciples of the ancient school to abandon, or at least to modify, their position. The vast amount of organic remains brought into view by the researches of the geologist needs to be systematically arranged, and to be compared with the forms of animal life at present existing. In a great majority of cases, external characters are here entirely wanting; and the zoologist is compelled to invoke the assistance of the comparative anatomist, for the determination of the nature and affinities of those beings to which must have belonged the bones and teeth and shells of bygone ages. In the classification of the mollusca, again, it has been increasingly felt that the old method of drawing the primary characters from the shells, rather than from the soft parts, was altogether wrong; the animals being the *essential* and the shells the *accidental* portions of these organisms; and the comparison of the former being, therefore, of primary importance in the determination of the real affinities of the latter. So, too, in entomology, it has been more and more found necessary to introduce not merely an anatomical but a physiological element into the plan on which the vast assemblage of the insect tribes is to be classified; for scientific entomologists are no longer satisfied with the knowledge of an insect in its perfect state, however complete such knowledge may be; but they seek to become acquainted with the entire history of the creature from the time of its emersion from the egg, with the characters which it presents in the two earlier stages of its existence, and with the mode in which these are transformed during the gradual development of the active embryo (for such the larva may be termed) into the complete imago. And in the investigation of the allied group of crustacea, it has been found absolutely necessary to pay close attention to the history of development, in order to ascertain the true nature of many of the animals which it includes, and to determine their affinities; for it has been by no means an unfrequent occurrence that an animal has been described as a new generic form, which is nothing else than the early or larva state of some other species, whose adult form is quite familiar to us; whilst, again, the changes which some of these animals undergo, in the course of their development, in order to adapt them to particular conditions and habits of life, are frequently such as to remove most widely from each other, in their adult state, animals which closely resembled one another at an earlier period, and which must, therefore, be considered as having an intimate fundamental relationship. Thus the young of the common crab is a strange-looking, actively-moving crustacean, with a large head, long, narrow body, ending in a brush-like tail, and very imperfect legs; in one of its states it has been described as a distinct genus, under the name of *zoea*, and has even been ranked in the group of entomostracous crustacea, which is very remote from the decapod order to which the crab belongs; and in a condition somewhat more advanced, it has been described under another generic name, *megalopa*. Hence, if all the forms of crustacea, which an attentive search would reveal in the waters of the ocean, were to be described as distinct and separate, we should have a vastly greater number of species than really exist in nature; the association of these into genera, families, and orders, would be a work of the greatest difficulty; and the classification, when completed, would altogether fail to represent the real affinities of the several beings which it embraces. Here, then, the zoologist can make no progress whatever without the assistance of the physiologist, whose business it is to trace the steps of the metamorphoses which many of the species undergo, to indicate what are adult and what are immature forms, and to point out the resemblances and differences which exist amongst the latter, commencing from the period of their escape from the egg. Thus, again, the true affinities of the cirrhipod class (including barnacles, acorn-shells &c.) have been determined by the labours of the anatomist and physiologist; for whilst their shelly covering has caused conchologists to claim them as members of the molluscous sub-kingdom, the annulose character of their internal organization, and particularly of their nervous system, indicated that their real position should rather be among the articulata; and the study of their development has converted this suspicion into a positive certainty, since in their early condition, they so closely resemble the larvæ of crustacea, that the whole group ought, perhaps, to be regarded as an aberrant subdivision of the latter class.

The importance of this combination of anatomical and physiological investigation with zoological researches, was early apparent to M. Milne-Edwards; who seems to have been strongly impressed with it whilst prosecuting his inquiries on the crustacea,—the first group (we believe) which he endeavoured to work out in detail; and who has since carried the same spirit into other departments of the natural history of the invertebrata, and has impressed it on the rising generation of French zoologists, from whose investigations we are continually receiving a rich harvest of valuable results. In several of his later Memoirs he has even adopted the principle, that embryology affords our best and surest guide in classification; as it is by the study of development that we are enabled most certainly to distinguish between those *essential* characters on which affinity depends, and those *accessory* characters which are engrafted (so to speak) on the original type for some special purpose. This doctrine was first formally enunciated by him in a Memoir on the Principles of the Natural Classification of Animals, published by him in 1844;* in which he points out that the condition of the earliest germ of all animals is the same,—namely, the simple cell;—that the earliest phases of its development differ according to the sub-kingdom to which it belongs, whether radiated, molluscous, articulated, or vertebrated, and that the distinctive characters of these *sub-kingdoms* are consequently those first evolved;—that, in the further progress of development, the characters of the *classes* next present themselves, then those of the *orders*, then those of the *families*, *genera*, and *species* consecutively, and lastly those of the *individual*. We are quite sure that Professor Milne-Edwards could not have been aware that he had been completely anticipated in this doctrine by Dr. Martin Barry; or, with his accustomed candour, he would have alluded to the circumstance. Dr. Barry's views, contained in two papers in the 'Edinburgh New Philosophical Journal' for January and April, 1837, are most clearly expressed; and we can only account for the slight attention they have received, by the somewhat abstract form in which they are enunciated, and by the absence of that kind of practical application of them which the author would have been able to make, if more versed in zoology; and also, perhaps, by the circumstance that, being based upon a very limited induction, they were put forth rather prematurely, although their correctness has been fully borne out by the subsequent discoveries. In the first of these papers, he works out the important principle of Von Baer,—that "a heterogeneous or special structure can only arise out of one more homogeneous or general, and this by a gradual change;" and applies this to the different *directions* of development, which present themselves in the primary subdivisions of the animal kingdom at a very early period of the history of the embryo, pointing out at the same time (as M. Milne-Edwards has subsequently done) that this fact completely negatives the idea that the vertebrated animal ever passes through the conditions which are characteristic of the radiated, the molluscous, or the articulated. He further shows that the order in which the distinctive characters of the germ are evolved, is that of their generality in the animal kingdom. "Thus, in development, the structure characteristic of the *vertebrata* only cannot manifest itself, until there has been assumed essentially a structure common to *animals*, of which the vertebrata are but a part, and to whose type the type of the vertebrata is subordinate. In like manner, structures subordinate to the type of the vertebrata cannot manifest themselves, until after a modified appearance of the *general type*, of which they are but partial metamorphoses. More and more special forms are thus reached in succession, until the one most special is at length attained." In his second paper, he expresses this view still more clearly, in the following table of the history of development of any single organism.

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* Annales des Sciences Naturelles, N. S, Zool., tom. i, p. 65.

1. No *appreciable* difference in the germ of all animals (fundamental unity).
2. The *class* manifest, but the *order* not distinguishable.
3. The *order* manifest, but not the *family*.
4. The *family* manifest, but the *genus* not known.
5. The *genus* obvious, but not the *species*.
6. The *species* manifest, but the *variety* unpronounced.
7. The *variety* obvious, but the *sexual* difference scarcely apparent.
8. The *sexual* character obvious, but the *individual* character obscure.
9. The *individual* character in its most special form.

In both papers Dr. Barry continually puts forth this principle as the groundwork of classification. Thus he says: "the only sure basis for classification is—not structure, as met within the perfect state, when function tends to embarrass, but—the *history of the development*, at that period when structure presents itself alone." And again: "the fact is that naturalists have begun just where they should have ended. They have attended to details, but neglected general principles. Instead of analysing, their process has been one of synthesis. Their attention has been directed to the grouping of the twigs,—as if they were thus to find their natural connexions, without even looking for assistance towards the branches, or the trunk that gave them forth. But the simile is inadequate; the labour lost has been greater than even this supposes. For in the *grown tree* of *animal* structure, parts, once essentially the same, have not only diverged in their development, and become elaborated into very different forms,—but, as before said, perform very different functions also. Hence a positive in addition to a negative source of error. But what other course *could* naturalists have taken? Truly none: their 'circumstance' allowed no other. It is only now that a way is beginning to be opened, by which it may by and by be possible to proceed in an opposite direction; viz. from trunk to branches and to twigs. This, if ever accomplished, must be by means of the *History of Development* or *Embryology*."

We have thought it right to bring forward Dr. Barry's claim as the first distinct enunciator of this doctrine, because we perceive that its truth is being more and more generally recognised, and that it must ultimately become the foundation of all philosophical zoology. The mere systematists are no longer able to maintain their ground against the innovations of the anatomist and physiologist; for they are forced to admit that animals which are closely allied to each other in form and structure at any period of their existence, must have a near relationship; although that relationship may be so obscured by subsequent changes of an *adaptive* character, as apparently to cause the characters of difference to predominate over those of agreement. And recent discoveries have shown that such relationships may exist even between *classes*, which have been hitherto regarded as entirely distinct; and that the occurrence of metamorphoses is so much more common among the lower tribes than among the higher, that we seem scarcely justified in assuming *any* specimen of the former to be a type of a complete and distinct species, until the entire history of its life has been examined. Amongst a large part of the lower invertebrata, indeed, metamorphosis would seem to be the rule and not the exception; the animal, at the time of its emersion from the egg, presenting a form altogether different from that which it is ultimately to attain, and in many instances passing through several intermediate phases before the adult characters are acquired. This fact, we think, may be connected with a physiological principle which seems to prevail in the vegetable as well as in the animal kingdom; namely, that, the higher the condition which the germ is ultimately to assume, the longer is the period during which its development depends upon materials supplied by the parent; whilst, the lower the condition which the germ when fully evolved is to present, the earlier it is thrown upon its own resources. The early state of all animals which undergo a metamorphosis, is essentially embryonic; as is evinced by the character of their component tissues and organs. In their *grade* of development, therefore, they are to be compared with the embryos of higher animals, at a period during which these are being nourished, either by a store of aliment previously prepared and stored up by the parent (as in ovipara), or by aliment more directly supplied from the living fluids of the mother (as in mammalia). But in the lower tribes of invertebrata, this kind of assistance is afforded only for a much shorter proportional time. Their embryos are earlier cast upon the world to get their own living; and the plan of their development is so arranged, that they come forth from the egg in a condition in which they can obtain it by their own movements, and digest and assimilate it by their own nutritive apparatus, whilst, at the same time, undergoing a series of progressive changes which terminate in the evolution of the adult form.

There is no department of Natural History which is more fertile in discoveries, than the one to which we are now directing the attention of our readers; there is none more interesting, from the novel and surprising character of the facts which it brings to light; and there is none more important in its bearings upon the general doctrines of physiology. We therefore propose to direct the attention of our readers, systematically and specially, towards the subject of the "Development and Metamorphoses of the Lower Animals," in a series of articles, of which each shall treat of some one group of phenomena, and be thus in itself so far complete, whilst the whole shall form such a connected history as our present knowledge of the subject admits of. We do not propose entering into those minute details, which are interesting only to the professed zoologist or physiologist; but shall confine ourselves to such a general view of the subject as all our readers, we trust, may be interested in obtaining. It has happened by a fortunate coincidence, that the long-expected work of Sir John G. Dalyell has been published, just as we were beginning to carry this purpose into effect; and we most gladly avail ourselves of the opportunity of expressing the high gratification which an examination of its contents has afforded us, and our strong sense of the value of the researches in which the accomplished author has so long been engaged. We can only regret, for his own sake, and for the credit of British science, that some of its results were not earlier made known; since in the publication of his most remarkable discovery:—that of the development, from a single polypoid animal, of a pile of young medusæ,—he has been anticipated by continental naturalists. Sir John G. Dalyell, however, belongs to a race of naturalists (at present, we are sorry to say, too rare) who pursue the study for its own sake, and not for the honours or rewards to which it may lead; who are consequently not ambitious of exciting attention by new and strange announcements of imperfectly-observed facts or ill-digested hypotheses; who consider that, the more extraordinary a phenomenon appears, the more they are bound to verify it by careful and long-continued examination; and who studiously avoid mingling their observations and deductions, but record exactly what they see, and leave it to others to estimate the value of their facts, and to build upon them such inferences as they may think proper. Since the days of Trembley and Lyonnet, we doubt whether there has been such an example of the patient and consistent devotion of a large portion of a life to one department of Natural History, as has been presented by Sir John G. Dalyell. Well, we feel assured, has he been rewarded by the gratification which he has derived from the study of some of the most beautiful objects in creation, and from the discovery of some of the most secret and wonderful mysteries of their production; and if any other reward were needed, we cannot doubt that it will now be accorded by the hearty expression of pleasure and admiration which an inspection of his work must draw from every lover of Nature, and from every votary of science. From intimations which have reached us from time to time, we have reason to believe that this work has been many years in preparation, and that a large number of its very beautiful illustrations have not merely been drawn, but engraved, long since; and we have even a suspicion that the author hesitated to publish some of the strangest and most novel results of his observations, from the fear lest they should be considered unworthy of credit, and should bring down ridicule upon the supposed ignorance and presumption of their promulgator, from those who can appreciate nothing that passes the bounds of their own experience, and fancy that Nature is tied down to the rules they have framed. So little, however, does he seem to care respecting questions of priority of discovery, that whilst he gives abundance of dates of days and months, that serve to indicate the rate at which the changes of growth and development occur, he scarcely ever mentions the date of the *year* in which his observations were made; and we are thus deprived of all clue to their exact period. The present volume is entirely occupied with the history of a small number of species of zoophytes, all of them natives of the coast of Scotland; and the following quotations from the preface will show the manner in which the history has been studied:

> "In endeavouring to ascertain the history of the animated tribes, it must be prosecuted from their origin to the close. We should behold them under circumstances the nearest possible to their mode of life in their natural abode. This, indeed, may be difficult. But spite of that tenuity of many humbler beings which almost eludes the observer's gaze, of that delicacy and fragility almost precluding his touch, their retreat from the light of day, and feeling the gentlest treatment too rude, patience and perseverance will infallibly reward his diligence.
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> "The more important features to be considered, are form, feeding, breeding, and the habits of animals. The form cannot be otherwise demonstrated than by the living subject. The food being seldom present with the creature, is often to be found only by accident; the breeding may be seen from opportunity, though long sought in vain; and the exhibition of habits is dependent on so many contingencies, as to require the careful preservation of life under the best conditions.
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> "From such indispensable requisites for a treatise on Natural History, it is obvious that the taste and qualifications of the historians of each must be diversified in their respective departments, that their opportunities shall be favorable, and their labours protracted.
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> "The operations of Nature undisturbed, are those which demand our confidence. The real organization and habits of the inferior tribes are never displayed unless in a tranquil, vigorous, and healthy state. When under constraint, placed in an unsuitable medium, or enfeebled by disease, the finest specimens languish; they alter and contract, the relative position of their parts is disturbed, their functions are impaired; the organs most conspicuous or most important during life, often disappear entirely, or they are changed by death, beyond the hope of recognition. Thence, can we do otherwise than reprehend and distrust the cruel operations and assumed results whereon too many modern anatomists have founded theories, from living animals,—rather from animals in the agonies of death!
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> "It is vain for one individual to claim the concentration of so many qualities as are necessary for the illustration of the natural history of even a single subject of the animal world; whence, conscious of the superiority of my more distinguished fellows in the science, I confine myself to a narrow sphere.
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> "My principal aim has been to render the external aspect and the habits of certain species of the lower orders more familiar to others, and especially to those who may not have had equal opportunities of personal observation.
>
> "In doing so I have endeavoured to select the most vigorous living specimens of their kind; and along with a general description to present their resemblance from delineations by the most skilful artists. Nothing is described, nor scarcely alluded to, unless represented; for it is irksome to read of what cannot be otherwise understood.
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> "By suitable precautions, I have been enabled to preserve individual subjects, one, two, nay, even ten or twenty years; and by this long acquaintance, to gain some information of their character." (Preface, p. viii.)

The following statements add greatly to the value of the beautiful delineations with which the volume is embellished; showing that they are to be regarded as actual portraits of living objects and not as *made-up* figures of very questionable truthfulness.

> "The naturalist's earliest care ought to be obtaining an accurate delineation of his subject—always selecting the finest specimen—and taking it in motion, or when the parts are best unfolded. Herein it is preferable to employ another's talent than the naturalist himself, though a skilful artist. Many delusions are ever ready to mislead; but of two observers, each may correct the other. After delineation, the subject can be studied at leisure." (p. viii.)

We fully accord with this principle, provided the artist be sufficiently conversant with the objects before him to know *how to observe* their peculiarities, and be sufficiently conscientious to represent only what he himself sees, and not what his employer wishes him to see. Sir J. G. Dalyell continues :

> "Respecting the representation itself, where indispensable to have figures larger than life, that is, as shown by lenses and microscopes, nothing more is requisite than what will favour distinct inspection. There, the author and the artist should stop. We are now less removed from the reality. Preserving distinct vision, perhaps diminution will be preferable to enlargement, just as a miniature of the human visage is more agreeable than one of colossal size. Besides, certain subjects become indefinite in proportion to the magnifying powers; where these are very high, unequal surfaces cannot be brought within a common focus. In general, the lower the power the better." (p. ix.)

This, too, is a most excellent rule; and many errors result from the neglect of it. Microscopists should always remember that the liability to error rapidly increases with the power employed; and that as soon as ever the characters of the object become indistinct, and the imagination is thus allowed to exercise itself, all trustworthiness ceases. Of course there can be no general rule as to the degree of power to be employed, since this varies with the nature of the objects under examination. We could wish that Sir J. G. Dalyell had occasionally favoured us with representations of the minute structure of the creature he portrays, many of which would have been most interesting objects for elaborate investigation; and we trust that a future volume may present us with information on these and other topics, on which it is much desiderated. But we would not be understood as depreciating, by this remark, the value of the instalment we possess; the information afforded being altogether such as probably no other individual, living or dead, could have placed within our reach. The following is our Author's estimate of the value of his own labours,—an estimate, it will be observed, the modesty of which corresponds with the merit and value of his researches.

> "It is not without diffidence that I venture to offer the following fragments to the public, seeing that there are numbers of the scientific world so much better qualified for the task. My purpose is sufficiently explained, as designing them to be viewed only as some detached memoirs, composed from an accumulation of facts. I profess no more than to speak of the living subjects I myself have beheld, and such as belong to Scotland. I disclaim all pretensions to discovery. I think, indeed, it would be difficult to name the first observer of Nature's offspring, whose race has existed since the beginning of time. If speaking personally of subjects occurring to me, it is by no means to disparage other naturalists, to whom I shall always render the homage due; if omitting quotation of the works of celebrity, it is from my inability to procure them, nor will their candid authors impute it to disrespect. Men gain nothing by depreciating their fellows." (p. x.)

Would that this last maxim guided the conduct of every scientific man! What a vast amount of envy, hatred, and all uncharitableness would be saved, if each would be content to estimate his achievements, not by comparison with those of others, but by the degree of expansion and comprehensiveness they impart to his own views, and by the brightness of the glimpses they afford of yet higher and grander truths! The contrast between moral and intellectual elevation is never more painfully shown, than when those to whom we look up as our scientific leaders descend from their high position, and stoop to the meanness of endeavouring to keep down a rising aspirant by depreciating his merits, or (still worse) of stealing the materials on which he is endeavouring to found his young reputation, in order to add another course to their already lofty pile.

It would be foreign to our plan to attempt anything like an analysis of Sir J. G. Dalyell's work; but we shall draw largely upon the rich store of materials which he has furnished towards the execution of our object; and shall quote from his researches, so far as they serve our purpose, in preference to those of others, for reasons which, after what we have stated, we need hardly specify. In our laudation of the only British naturalist who has contributed in any important degree to the extension of our knowledge of the subject of the present article, we would not be thought to undervalue the merits of those foreign zoologists, who have laboured so successfully in the same field. It is to Sars, Siebold, Lovén, and Van Beneden, that we owe our first acquaintance with the remarkable phenomena which we shall presently describe; and although they have been led on many points into errors, from which a longer and more careful scrutiny would have saved them, yet these errors have been gradually eliminated, and a distant glimpse has been obtained of the beautiful simplicity of the truth, although much research is still requisite to perfect that glimpse into a full and uninterrupted view.

Many of our readers are doubtless acquainted with the remarkable work of Steenstrup, translated and published by the Ray Society, on the so-called 'Alternation of Generations.' A very plausible hypothesis was built up in this work, upon the foundation of those phenomena of metamorphosis presented by the zoophytes and acalephæ which we shall presently detail, together with others derived from different classes; this hypothesis being briefly,—that, in certain groups of the animal kingdom, like does *not* produce like; the offspring never resembling its immediate parent, but corresponding with its grand-parents; and the young which it produces repeating the forms and characters, not of itself, but of its progenitor. Or, to put the case in a simpler form, generation A produces generation B, and generation B reproduces generation A; this alternation continuing constant as long as the race is propagated. Now we regard this as a very premature, erroneous, and limited expression of the real facts; and shall endeavour, in the course of our exposition, to show what is the real truth of the matter. The proposition, in the form in which it is enunciated by Steenstrup, is totally inapplicable to the vegetable kingdom; and a strong suspicion of its incorrectness is suggested by that simple circumstance, inasmuch as it is chiefly based upon the phenomena presented by those tribes of animals, which have most in common with plants in their general structure and history. And we think we shall be able to prove on this occasion, that, so far as relates to the subject of our present inquiry, Professor Steenstrup has laboured under a total misconception of the most important part of the process,—a misconception which (as we shall subsequently show) has extended itself to his views of analogous phenomena elsewhere. Nevertheless the attempt at generalization to which we have alluded, had the great merit of bringing together a number of phenomena of analogous character; placing them in such proximity, that their real connexion can be better appreciated; and thus enabling the naturalist to survey them in their general aspect, or to scrutinize their details, with a much greater probability of attaining an accurate result than if he should examine each example for and by itself. Hence if we are enabled to present a clearer and more philosophic view of this subject than our predecessor has done, we feel that it is to him that we owe the collocation of our materials, and the first attempt at grouping them in any harmonious and systematic method.

In order to build our theory upon a right foundation, it is requisite to consider the two leading modes in which reproduction takes place in the Vegetable kingdom; and we find the simplest types of these among the lower cellular plants,—in the *Confervæ* of our streams, for example. Each one of the filaments of which these are composed originates from a germ, which is first developed into a single cell; this cell gives origin to two which succeed it; these new cells themselves undergo the same increase or subdivision; and thus a long series, attached at one end, free and constantly growing at the other, is at last produced. Now we have here a multiplication of the cells by a process of *continuous growth*, which essentially corresponds with the progressive development of one of the higher plants from seed. As the former produces one cell after another, each of which is a repetition of the rest, and is in great degree independent of it, so does the latter generate those compound aggregations of cells and other tissue called leaf-buds; each of which possesses so much of independent vitality that it will continue to vegetate under favorable circumstances even after its separation from the parent structure. This process is commonly known as that of *gemmiparous* reproduction; and is obviously to be regarded as only a peculiar form of the ordinary nutritive operations. We believe that, when it is rightly understood, it will be found to include the *fissiparous*, or reproduction by the division of the parent structure; since *every* multiplication of cells by continuous growth appears to take place by some kind of subdivision of their cavity.

Now, on the other hand, at a certain stage in the evolution of the parental structure, certain of its cells form within themselves distinct germs, which are at first seen to be attached to their wall, but afterwards lie loosely (often having a curious and active movement) within their cavity, and are finally discharged by the rupture of the parent or germ-producing cell. This process is obviously altogether distinct from the preceding; and the difference becomes still more obvious when we compare the two in the higher members of the vegetable kingdom, in which the special productive apparatus is more completely detached from the nutritive. For we have, in the flowering system, an apparatus destined *only* for the production of the seed, which contains a germ that has been altogether detached from the parent from a very early period, and is destined to be developed in complete dissociation from it, without any further assistance than is afforded by the store of nutriment laid up in its neighbourhood. The seed corresponds in all essential particulars with the ovum of Animals; each containing the germ, and the store of nourishment prepared by the parent for its early development; and therefore we may for convenience distinguish this mode of reproduction as the *oviparous*, although the term is not etymologically applicable to the seed, and is commonly employed in contradistinction to viviparous production, in which the embryo is produced alive. Every physiologist, however, is aware that viviparous production is in its first stage oviparous; and the term is really, therefore, universally applicable to the process as performed by animals. Its least correct application is when it is made to include the simple emission of germs (which, when once set free, are destined to maintain their own existence without further aid) in the lower cellular plants; but as we believe this to be only an earlier and simpler stage of the same process, the term *oviparous* (in default of a better) may be extended to it also; and may be thus employed to designate every variety of that form of the reproductive process in plants and animals, in which the multiplication is effected by means of germs detached from the parent, and not by growth in continuity with it. In all Animals, as in the higher plants, that process is effected by the concurrent agency of two sets of sexual organs, situated on the same individual, or appertaining to two different beings; and it will aid us in distinguishing between the two processes, if we bear this fact constantly in view. For where there is no evidence of such concurrence, the presumption is strong that the reproduction is really carried on upon the gemmiparous plan, however great may be the apparent departure from the usual mode of its operation. We find Steenstrup representing his so-called "nurses," which we shall show to be individuals increasing by gemmation, as of the female sex; which he has no more reason for doing, than the botanist would have in speaking of a budding plant as of the female sex up to the time of the evolution of its flowering system.

It is to be borne in mind that there are not wanting examples in the Vegetable kingdom, in which true buds or *gemmæ* are spontaneously detached from the parent at a certain period of their development, and thenceforth grow quite independently of it. This is the case, for example, with the common *Marchantia polymorpha*, a liverwort, which, besides its regular apparatus of fructification, has a beautiful little basket-shaped receptacle, from the bottom of which a number of little green disc-like bodies are produced by a process of continuous growth; these discs are at first supported upon a footstalk, but they become disconnected in time from their bases, and are then frequently washed out of their receptacle by the rain and scattered upon the ground in the neighbourhood, into which they soon begin to send down their delicate rootlets, gradually developing themselves until they acquire the characters of the parent structure. And it is interesting to remark that in some seasons these receptacles and their contents are abundant, whilst the proper fructification is scarcely to be met with; the reverse being the case in other instances; so that the production of *gemmæ* or of *spores* must be determined by peculiarities in external conditions. In many ferns there is a similar tendency to produce gemmæ from the tissue of the leaves, either on the surface or in the angles of the lobes; these drop off and become independent plants. Although among the Phanerogamia the buds more frequently remain in continuity with the parent structure, yet there are not wanting examples of their spontaneous separation. This is the case, for instance, with the common *Lilium bulbiferum* and with the *Dentaria bulbifera*, which have received their specific names from the bulb-like character of their leaf buds; these, which are technically called *bulbels*, gradually become detached from the axil in which they were produced, and falling upon the soil reproduce the plant. The small bulbs or *cloves*, which are generated beneath the scales of the proper *bulbs* of the bulbous-rooted (or rather bulbous-stemmed) plants, are nothing else than metamorphosed buds, having a similar tendency to spontaneous detachment.

We shall now pursue our inquiries into the history of the reproductive process in the Animal kingdom, commencing with its lowest forms; and we think that we shall be able to explain the greater part of the phenomena which have been regarded as most anomalous, in strict conformity with the general principles now laid down. Some extension of our usual ideas of the gemmiparous process, as carried on amongst animals, will, however, be required; for we have been accustomed to regard this as operating only in those lowest tribes, whose affinities to the vegetable kingdom are the closest. So exclusive, indeed, have been the prevalent notions on this subject, that even so philosophic a zoologist as M. Milne Edwards was strongly inclined to degrade the Tunicata from the position they had been accustomed to occupy, as the lowest class in the Molluscous series, to the group of Zoophytes; because he found that, in common with the latter, certain species propagated by gemmiparous as well as by oviparous production. But the existence of gemmiparous production in many other classes than zoophytes is now so well established, that we need no longer attribute such value to its presence in the tunicata or in any other group, as proving an alliance with them. It is in zoophytes, however, that its phenomena are presented in the most varied and remarkable manner; and to a general account of these we shall now proceed.

We may presume our readers to be sufficiently well acquainted with the remarkable phenomena of the gemmiparous multiplication of the *Hydra* or fresh-water polype,—as long since described by Trembley, and subsequently witnessed by a host of other observers,—for it to be unnecessary that we should dwell upon them here in any detail. The structure of this creature is so simple, and its character so homogeneous, that every part of its body is capable, under favorable circumstances, of regenerating the entire animal; so that it may be artificially propagated by division into numerous parts, just as a plant is propagated by the separation of its component leaf-buds. Now it is not to be supposed that, in the one case more than in the other, there is here anything like oviparous reproduction; the whole process is the result of the simple action of *continuous growth*, which is here adequate—such is the wonderful tenacity of life in the divided fragments—to develope the entire being from even a minute portion of it. It is on the same action, that the ordinary propagation of the hydra depends; for this is not usually effected by the production of ova, but by the development of buds or offshoots, which may arise from any portion of the body of the parent structure. These buds remain connected with it, even after their organization has been so far completed that they can obtain and digest their own food; but the footstalk by which they spring is gradually narrowed, and at last, by a mutual effort, the parent and the bud are detached from each other, and the latter commences its free and independent existence. Numerous buds, in various stages of advancement, may frequently be seen upon the body of a single parent; and it is not uncommon for some of those buds which are most mature to develope secondary buds, even before their own detachment from the parent hydra; so that a single individual may thus bear upon itself from twenty to thirty of the offspring thus strangely produced, constituting a third as well as a second generation. All these, however, are destined to become detached in time; as in the true hydra, each polype is essentially solitary, instead of being a member of a community, as it is in the compound hydraform zoophytes. This detachment of buds bears a close resemblance to the separation of bulbels among plants; in some of the lower tribes of which, as we have just seen, there is a special apparatus for their development, altogether distinct from the germ-preparing organs. A little consideration must render it, we think, quite apparent that the regular gemmiparous process in the hydra, and the extraordinary power of regenerating the entire structure which exists in each part of its body, are two manifestations of one and the same power of continuous growth; and that the former cannot, any more than the latter, be referred to any kind of sexual operation.

But a distinct sexual apparatus does exist in the hydra, under certain circumstances; although it would seem, not merely to act, but even to be developed, only under particular conditions; and it has in consequence been very generally overlooked. The gemmiparous process in this animal, as in plants, is carried on most actively under the influence of a warm temperature and of a plentiful supply of food. On the other hand, the oviparous reproduction only comes into play as the temperature falls on the approach of winter; and it appears to be the destined means of continuing the species through a degree of cold which would be fatal to the parent, although capable of being endured without injury by the eggs. The process was described some years since by Ehrenberg, Dujardin, and Laurent, and very recently also by Dr. Allen Thomson,* whose observations are the more valuable, as he was ignorant at the time of making them of what had been seen by his predecessors, and was, consequently, an independent witness. The male or fecundating apparatus simply consists of a sac which is developed near the oral extremity of the body, in the substance of the wall of the digestive cavity; this sac (probably to be regarded as a simple cell, analogous to the seminal cells formed within the testes of the higher animals) is at first closed; but moving filaments may be discerned in its interior, as it approaches maturity. As the sac enlarges, it projects on the external surface of the body, and at last ruptures and discharges its contents, which are obviously analogous to the spermatozoa of higher animals. Near the other extremity of the body, which is prolonged into the foot, other projections are seen, of a different nature; these are formed by the development of larger bodies in the tissues of the parent, which are afterwards discharged entire, and can scarcely be other than true ova, having a horny investment, and albuminous contents. These eggs did not escape the observation of the acute and persevering Trembley; but he did not satisfy himself of their nature, and was not aware of the formation of the sperm-vesicles. It would appear that sometimes one individual hydra developes only the male cysts or sperm-cells, whilst another developes only the female cysts or ovisacs; but the general rule seems to be, that the same individual forms both organs. Like an annual plant, which withers after flowering and the maturation of the seed,—or like many perfect insects, which perish so soon as they have made the requisite preparations for the continuance of the race,—the parent hydra appears invariably to die after producing a few ova. According to M. Laurent,* its body dissolves away as it were, and forms a glutinous investment to the ova, by which they are attached to aquatic plants.

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* Edinburgh New Philosophical Journal, April 1847.

No germinal vesicle has (we believe) yet been detected in the ovum of the hydra; and it seems to be questionable whether the entire ovum is not really homologous with the vesicle of Purkinje, which may itself contain a store of nutriment, adequate to the development of so low and simple a creature, without the additional supply furnished by the yelk in which this vesicle is elsewhere imbedded. It seems to be clearly established, however, that this separated body contains the essential parts of a true *ovum*, and is altogether distinct in its characters from a *bud*. It consists chiefly of matter not yet organized, though organizable; and only becomes fertile when impregnated by the spermatozoa. The germ is not seen in it until long after its detachment from the parent; this germ is developed by means of the nourishment that has been previously stored up in the ovum, which it has the power of assimilating; and it thus arrives at full maturity, and comes forth from the egg in a state of sufficiently advanced development to enable it to maintain an independent existence. On the other hand, the bud is developed in continuity with the tissues of the parent, of which it is a mere extension; it remains for a considerable period in direct dependence upon the parental organism for its means of growth and development; and it is not until its organs are sufficiently evolved to enable it to obtain, digest, and assimilate food for itself, that its detachment takes place. We are at a loss to conceive, therefore, how so profound a physiologist as Professor Owen could have given utterance to the following speculation: "The most ordinary process of generation is by the development of young polypi, like buds, from the external surface of the old one. It is, however, most probable that, in these cases, the gemmation is preceded by the development and fecundation of the true ovum, beneath the integument." (Hunterian Lectures, vol. i, p. 85.) As well might it be supposed, in our opinion, that the production of every bud of a plant is preceded by the development and fecundation of a true seed, beneath the bark; instead of its being, as is well known, an extension of the original cellular substance of the stem, that preserves its existence in the pith and medullary rays, from one or other of which all buds take their origin. And, as Dr. A. Thomson justly observes, such a supposition is negatived by the large size and palpable characters of the real ovum, some traces of which must surely be observable at the base of the buds, if Professor Owen's theory were correct; no such traces, however, have ever been detected.

The animal produced from [illegible] ovum of the hydra seems to resemble its parent. There does not [illegible]r to be here either metamorphosis or "alternation of generations." But the phenomena of its development have been as yet but insufficiently studied; and nothing like a trustworthy history of them can yet be given. M. Laurent states that he has procured similar ova from individuals of three successive generations of buds; that is, from a parent stock, from her progeny, and from the offspring of the latter. All these went through similar changes, and died after depositing their eggs. Thus, in the common hydra there would seem to be no difference in the product of the gemmiparous and of the oviparous processes; each creature, whatever its origin, resembling its parent in all essential particulars.

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* L'Institut, Nov. 1842.

Far different, however, is the history of another creature, which seems in one of its states to be so nearly allied to the common fresh-water polype, as to be almost entitled to rank as a cognate species of the genus Hydra; but which gives origin to beings that have been regarded until recently as belonging to a class entirely distinct. This is described by Sir J. G. Dalyell under the name of the *Hydra tuba* or trumpet polypus; and he thinks that it may be identified with the *Hydra gelatinosa* of Müller (Zoologia Danica, 1789); but this seems to us very questionable. We think it but just to him to quote the following statement, with which he introduces his account of this singular creature:

> "Let me here premise that some years ago, long after the subject of this paragraph had come under my notice, I submitted a few general observations regarding it to the British Association for the Promotion of Science, during the sittings of that learned body at Edinburgh in 1834. As the study of natural history was advancing but languidly in Scotland, my principal aim was then, as on previous and subsequent occasions, to engage the attention of my countrymen with the interesting phenomena which they might readily discover among our national products. Therefore, selecting only the facts most easily attained, not exacting painful and protracted study, I sedulously abstained from discussing various other important and still more interesting points, though sufficiently acquainted with them. I believe now that it would have been better had I done differently; for it would have prevented certain authors from betraying themselves into very erroneous conclusions of the import of my observations." (p. 77.)

Reference is here probably made, in part at least, to the very harsh comment of Steenstrup upon the observations communicated from time to time to the 'Edinburgh New Philosophical Journal' by Sir J. G. Dalyell; of which he says,—"These observations are not only filled with matters of which Sir J. G. Dalyell has taken a false view, but also contain phenomena which he has misunderstood; and they have consequently been of no utility in science, until now that other fundamental researches have allowed of their being correctly explained." Now it is perfectly apparent to us, that the Scottish observer must have been in possession of the real truth from a period much earlier than that at which it had been deduced from the researches of Sars and Siebold, who have hitherto enjoyed the full credit of first unveiling the remarkable phenomena we shall presently describe; although, with the characteristic caution of his countrymen, he hesitated to make his views fully known until he had most fully satisfied himself of their correctness, and consequently employed, in such descriptions as he put forth from time to time, phraseology which by no means conveyed his whole meaning. And it i[illegible]ther evident to us, that his observations have been far more prolonge[illegible] have attained on many points a far greater degree of exactitude and completeness, than those of either of the authors we have just named; and we shall show that one point of fundamental importance has been clearly established by Sir J. G. Dalyell, although neither Sars nor Siebold was able to make it out; and that he has been thereby led to a much more accurate conception of the whole process, than they or their followers have attained. We are happy to find this conception in entire accordance with views which we had ourselves previously formed and expressed.

The first of Sir J. G. Dalyell's series of observations on the *Hydra tuba* regards it as a perfect animal; and the following is his description of it, by which its points of analogy to, and difference from, the common hydra will be easily seen by any one acquainted with the latter.

"The body of the hydra tuba is a hollow cone, five lines in length, thick and fleshy. Thirty or more very extensile, flexible, fine, slender, muricate tentacula descend twenty-one lines from the margin, collectively forming a beautiful silken-like pencil waving amidst the water. The mouth rises as a conic frustum amongst them, in the centre of the disc, much resembling the closed mouth of the actinia; but there is no analogy to a proboscidal organ, either here, or in any other of the hydraoid race. The natural colour of the animal is universally dingy white, sometimes faint orange, perhaps according to the season; but it is specially affected by the quality of the food. It is affixed by the apex; and is exclusively an inhabitant of the sea.

"Complete development of all the parts of the hydra tuba is best exposed under temporary abstinence; the observer will be disappointed of seeing them if resorting to his specimen in a state of repletion. While in abstinence, the animal remains suspended by the apex, the body lengthens, and the tentacula are extended to the utmost stretch in quest of prey. If sensible of its presence when inaccessible, the hydra does not employ them as instruments of capture in sweeping around; but the mouth widely dilating, projects the edge as a thin flexible lip in much action. Now, the capacious cavity of the stomach indistinctly exposes a kind of columnar range around the internal parietes, possibly corresponding with the external form of the body, at times bearing faint resemblance to a cluster column. Exposure so complete is very rare. The stomach is most capacious; when coloured food gorges the hydra, it is seen nearly at the apex, indeed excessive distension seems to detach the animal from its point of adhesion, when it falls to the bottom of its vessel." (p. 77.)

This creature is far from nice in its selection of food; but preys readily and greedily on most other animal substances; seeming to have an especial fondness for young actiniæ (sea anemones), but being in return itself devoured by the old ones. When sated it remains motionless, with the tentacula closely contracted; in this particular exactly resembling the true hydræ. Sometimes a single voracious meal serves it for ten or twelve days; but more commonly, if a large quantity is taken at once, a portion of it is rejected by the mouth, in a half-digested state, instead of the whole being absorbed into the system, as it is when the food is more sparingly supplied. It can endure long-protracted abstinence, under which its size gradually diminishes; but it is suddenly restored by sustenance, and may survive for many years. Hunger induces the extension of the tentacula; but no searching activity is manifested by these organs, even when food is within their reach; and there is no reason to believe that their prey is ever discovered otherwise than by direct contact. The peculiar habitation of this creature seems to be the inner surface, and especially the upper cavity, of empty oyster or other marine bivalve shells. It seems to have less locomotive power than the fresh-water polype, but continues attached to the spot to which it at first fixed itself. Sometimes it dwells in numerous societies, whilst in other cases it is solitary.

Our author's long-continued observations, commenced at a time when he did not suspect this animal to be anything else than a hydra, and subsequently carried on through a long succession of years, have established its exact resemblance to that polype in all that relates to its ordinary gemmiparous production. It propagates, like the hydra, by gemmation, or the budding of the offspring from its side; and it is extremely prolific, especially under the favorable conditions of genial temperature, copious sustenance, and frequently renovated water. Thus a colony of ten hydræ, occupying the cavity of a fragment of a razor-shell, multiplied to such an extent as to spread over both surfaces within a few months; and in ten months from the first procuring of the group, it consisted of fifty independent animals, besides others which had probably become detached and dropped from their site. Were such a rapid multiplication usual to it, the *hydra tuba* ought to be a very common animal, instead of being rare as it is. But it is probably seldom that all the favorable circumstances above specified are concurrent. Sir J. G. Dalyell further ascertained that this creature has the same kind of power of regenerating parts that have been removed, or of reproducing the complete body and tentacula from a portion of it, as that which is possessed by the common hydra; and that both portions of a bisected polype could not merely be developed into new and perfect individuals, but could produce offspring by gemmation.

So far, then, the *hydra tuba* has been proved to display a complete accordance with the common hydra, in all the essential features of its structure and actions, save only the propagation by ova, which never presented itself in the same fashion. Was, then, the *hydra tuba* propagated solely by gemmation? This was the problem which naturally presented itself for solution; and that solution was fortunately suggested to our observer by one of those accidents which are continually occurring, but of which only sagacious men take advantage. In the course of a series of experiments and observations directed towards the investigation of the characters and properties of the *medusæ*, or jelly-fish, Sir J. G. Dalyell had a number of capacious glass vessels made, by means of which a complete view of their singular formation and habits could be attained, whilst they were still living in the midst of their native element. A medusa belonging to the genus *chrysaora* (Peron), the umbrella-shaped disc of which was about eight inches in diameter, was placed in one of these vessels. Although it seemed at first sufficiently vigorous, it soon began to languish and decay; but when it was removed from the vessel, a quantity of brownish matter, like dust, remained at the bottom. Subjected to the microscope, it proved to consist of a host of animated creatures, in quick and varied motion, although to the naked eye they were barely perceptible. When viewed with a high magnifying power, these bodies were seen to be white opaque discs, of a soft homogeneous texture, and a form which changed with their movement, though tending to the elliptical. Similar discs have been observed as the progeny of the zoophytes, and have been designated by Sir J. G. Dalyell as *planulæ*. After the lapse of about forty hours, a distinction became obvious between the contained and the containing portions of these bodies; the latter being indicated by the lighter margin, and the former by the dark interior. The shape also of some of them had become much modified; certain projections appearing, which were subsequently perceived to be rudimentary tentacula; and an orifice presenting itself in the centre, which afterwards obviously became the mouth. "All this," as our author justly remarks, "was an extraordinary exhibition in the offspring of such parents, so lately issuing as mere and almost invisible dust from compact, massy, ponderous animals, alike singular in habits, in form, and in substance." That the brownish dust was really deposited by the medusa, was fully proved by observations made upon other individuals, which were seen in the very act of discharging it. "I have seen," he states, "a stream of cremacious-like matter absolutely flow from the ovarian sacs of the medusa, proving exclusively incalculable legions of *planulæ*. In no great number of days the planula becomes a regular polype; from a freely-moving disc, without organs or oral aperture, it is converted into a stationary hydra, with a definite mouth, surrounded by a circle of eight tentacula; and the form, size, and proportion of parts of the hydra tuba gradually displayed themselves, the metamorphoses, however, not going on with any regularity or uniformity, but differing in the order of the changes, and in the rate of progress, of the several individuals of the same brood." "Thus was a most perplexing problem solved—the *hydra tuba* proved to have sprung from a *medusa!*" The progeny of other species of medusa were examined, with the same result; the young, in its first development, having always the characters of the hydra. It was found difficult to preserve them, however, for any length of time; and their subsequent metamorphoses were consequently not investigated.

We have now to accompany Sir J. G. Dalyell through another part of the same inquiry into the nature of the extraordinary connexion which is thus indicated between the medusæ and the polypes; and cannot better introduce the subject than by his own apposite prefatory remarks.

> "We have viewed the subject of the first and second paragraphs of this chapter exclusively as a hydra or polypus, an animal apparently perfect in itself, and subsisting in independent life; presenting all the properties of that singular genus; nor exhibiting any such discrepancies as to sanction its removal to be incorporated with some other race. It visibly originates, feeds, breeds, lives, and dies after the same manner as the rest of the species; therefore in as far as we have gone we should associate it with them. A creature which survives for years, which transmits its form, together with all its peculiarities, to its immediate progeny, and to remote descendants, seems at first sight entitled to a distinct position in the *Systema Naturæ*.
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> "But as no observation can be too correct, nor any reasoning, unsustained by direct evidence, too profound, so ought due precaution to warn the naturalist against receiving presumptions for facts.
>
> "We have still to look farther,—to dive deeper into the obscure subject before us.
>
> "From one notable peculiarity of the *Hydra gelatinosa*, *strobila*, *tuba*, or by whatever name it shall be recognised, our notice must now be directed to an animal of altogether an opposite origin, one entirely different in form, in habits, and in permanence, though between the two there be a union or connexion, hitherto insufficiently understood." (p. 111.)

Long ago, and at different periods, Sir J. G. Dalyell had remarked colonies of minute transparent animals, swimming in vessels of sea-water, during the months of February, March, and April. Their general aspect very much resembled that of a flock of birds in distant flight, as represented by landscape-painters, but careful examination showed that they belonged to the *Medusa* tribe. The consist of an umbrella-shaped disc, divided at its margin into a variable number of lobes, each of these lobes again cleft (whence he gave it the name of *Medusa bifida*); at the bottom of each cleft is a cone surmounted by a dark speck, analogous to the supposed *ocelli* of other medusæ and star-fishes. From the centre of the disc hangs down a long proboscis, "like an obtuse-sided cluster-column." The animal moves by jerks or bounds, striking the water by the contraction of the lobes of the disc. Like other medusæ, it shows a constant tendency to seek the surface by an oblique or perpendicular ascent; being apparently attracted by the light, whose influence they seem to court, unless it be too intense. After being transferred to vessels free from other subjects, they continued several days in activity, and then disappeared. He could not account either for their origin or their transience; he noticed, however, that though occasionally found in water taken direct from the sea, these objects were chiefly observed in vessels containing the hydra tuba, and that when removed they were frequently replaced by others; but no visible spawn, fragments, or other elements could be discerned, from which they could be supposed to have originated, and he was consequently at a loss to account for their appearance. He was led to the solution of the mystery, and consequently to the very curious discovery on which it was dependent, by noticing the successive appearance of these medusæ in a vessel in which several specimens of the hydra had been long under observation; and by remarking a sort of convulsive motion among the tentacula of one of the hydræ, which seemed to proceed from the entanglement of a medusa among them. This convulsive motion presenting itself day after day, a feeling of surprise was very naturally created in Sir J. G. Dalyell's mind that the medusa should thus continue to dwell with impunity involved in the very organs of capture; and his close attention being then directed to the specimen, he found that, instead of being an ordinary *hydra* entangling a medusa in its tentacula, it was in reality *a pile of medusæ* springing from a sort of fleshy base; and that the pile gradually broke up into its component medusæ, one being detached from its summit after another, by a sort of convulsive effort, until nothing but the fleshy base remained. What then was the origin of this pile of medusæ? He could attribute it to nothing else than the métamorphosis of a hydra; since the vessel from which this subject was removed contained a colony of about 100 of these polypes, that had been propagated from ten specimens, and had been under observation for more than three years, during which time no other animal had been introduced among them. He was thus, of course, guided to more extensive and minute inquiry; and was rewarded by the discovery of the most important features of this curious metamorphosis. We cannot follow him through his series of observations,—deeply interesting though these are, and highly valuable, as being the faithful records of the actual phenomena witnessed,—but must content ourselves with presenting their general results.

Under certain peculiar circumstances not yet fully understood, the *Hydra tuba* (which is originally, as we have seen, the offspring of a medusa) ceases to propagate individuals like itself by ordinary gemmation, although this process may have gone on with regularity and activity for months or even years previously; and assumes a more elongated cylindrical form than it previously possessed. A constriction or indentation is seen around this cylinder, just below the ring that surrounds the mouth and gives origin to the tentacula; and similar constrictions are soon repeated around the lower parts of the cylinder, so as to give to the whole somewhat the appearance of a *rouleau* of coins. Still, however, a sort of fleshy bulb,—somewhat of the form of the original polype,—is left at the base. The number of the circles is indefinite; and all are not formed at once, new constrictions appearing below, after the upper portions have been detached. As many as 27 have thus been progressively produced in one specimen. The constrictions then gradually deepen, so as almost to divide the cylinder into a pile of distinct saucer-like bodies: the division being most complete above, and the upper discs presenting a considerable increase in their diameter. As the discs thus become more distinct from each other, and of enlarged dimensions, their edges are no longer plain, but become lobed; and the lobes soon present the clefts and ocelli characteristic of the detached medusæ. Up to this period, the tentacula of the original polype surmounted the highest of the discs; and a general contraction and relaxation of the whole cylinder, causing the intervals between the discs to be diminished or increased, might be occasionally seen to take place. But before the detachment of the topmost disc, the circle of tentacula by which it was originally surrounded disappears,—in what precise manner Sir J. G. Dalyell has not been able to ascertain; but meanwhile, *a new circle of tentacula is developed upon the summit of the bulb, that remains at the base of the pile of discs.* This is a fact of great importance, as we shall presently see; and it is one that seems to have escaped the notice of other observers. At last the topmost and largest disc begins to present the convulsive action that precedes its liberation; it becomes detached, and swims freely away; and the same series of changes takes place from above downwards, until the whole pile of discs is detached and converted into free-swimming medusæ. *But the original polypoid body still remains*, and may return to its polype life and gemmiparous production, becoming the progenitor of a new colony of hydræ, every one of which may develope in its turn a pile of medusa-discs. Now this fact, our knowledge of which is entirely due to the persevering researches of Sir J. G. Dalyell, is of fundamental importance in our philosophical interpretation of this wonderful process.

On one point Sir J. G. Dalyell has been less successful than his foreign contemporaries; he has not been able to trace the further development of his *Medusa bifida*, which is evidently an immature form. By no effort could he prolong its life more than 55 or 60 days, a period too short for the attainment of its perfect characters. The eminent Norwegian naturalist, Sars, however, has been more fortunate; and has been able to watch the development of this immature form into the adult medusa, a creature possessed of distinct sexual organs which are contained in separate individuals, and reproducing its race by their conjoint action. The process of development is briefly this.—During their growth, the segments or lobes of the border of the disc increase very little in size, whilst the intervals between them gradually fill up; tubular prolongations of the stomach extend themselves over the disc; and its border becomes furnished with long, pendent, prehensile tentacles. The mouth, which even in the youngest detached animal allows of being greatly extended and protruded, is quadrangular, and presents four extensible angles. These angles grow more rapidly than the four-sided oral tube or proboscis; so that, in the more advanced animals, the mouth appears during the growth to have divided or split into four lobes; and the minute serratures which appear on the edges of these are the commencement of the lobes and fringes which are observed on those organs in the adult animal.

This mode of development has been as yet determined only in two of the commonest medusæ of our seas, the *Medusa aurita*, and the *Cyanæa* *capillata;* but there can be little doubt that it is common to all the pulmonigrade forms of this class. So close is the correspondence between these two during the earlier periods of their evolution, that it seems quite impossible to point out any distinction between their polype-larvæ or even their early medusoid forms; the distinctive characters of their respective genera being only evolved as they approach maturity. Hence it is quite impossible to say whether the *Medusa bifida* of Sir J. G. Dalyell would have been evolved into one of these two forms, or into some other. One at least of the medusæ, from the ova of which his *Hydra tuba* originated, seems to be of a distinct genus; so as to afford us proof that, in three genera of pulmonigrade medusæ, the ovum produces—not a being resembling the parent, but—a polypoid animal, which is in every essential respect conformable in its growth, habits, and method of gemmation with the ordinary hydræ, and which may remain in that condition for an indefinite period; but which, under conditions as yet unknown, may give origin to a pile of medusoid discs, every one of which may be developed into a medusa resembling its original parent.

Now what is the real character of the latter part of this curious process, —the production of the pile of the medusoid discs from the polypoid base? It has been represented by Sars as if the original polypoid animal became *divided* into an aggregation of medusæ: and this view is taken up by Steenstrup, who goes so far as to maintain that the original polypoid animal is really a fixed medusa of the female sex, which serves as a sort of *nurse* to a brood of young ones. How these young ones are developed—whether from ova or from gemmæ,—he does not attempt to explain; nor does he show, if they are produced from ova, by what male influence they are fertilized. Altogether the theory is very obscure, and we believe that we can offer an explanation, which is not only much simpler, but more in accordance with another group of facts which we have presently to notice as beating on the relation of the hydraform polypes and pulmonigrade medusæ; and which is also in harmony with the phenomena of the vegetable kingdom. It is evident to us that the pile of medusa-discs is *not* formed by the constriction of the proper body of the original polypoid animal; but that it is the result of a process of *gemmation* taking place between the body and the oral ring of the parent structure. The buds thus produced, however, differ from the polype-buds previously generated by the same being; but they differ precisely in the same manner that the *flower-buds* of a plant differ from its *leaf-buds,*—that is, in containing a sexual apparatus. As the leaf-buds (so to speak) of these animals become detached and independent creatures, it is not surprising that the flower-buds should do so likewise; the purpose of the latter being to multiply the race in far distant quarters, whilst the polype-buds establish themselves at no great distance from the parent stock. The loss of the original tentacula of the polype is a necessary consequence of the position in which these medusa-buds are formed; since the detachment of the latter involves the separation of the tentacular ring from the body at the base. The production of a new circle of tentacula, ready to come into action when the lowest disc has been detached, and the original body has been left the same solitary creature that it was before, is only an instance of the regenerating power, which, as we well know, the hydraform polypes usually possess.

The following is the interpretation which we should give, on this view of the case, to the series of phenomena we have described. The fertilized ovum of the medusa-parent is like the seed of the plant; and the polype that grows from it resembles the first leaf-bud into which the embryo expands. From this bud are at first produced others, by the process of continuous growth, which are repetitions of itself; these in the plant usually remain connected with each other so as to form a compound structure, and so they do also in the ordinary zoophyte; but in the common hydra, and in the hydraform medusa-larva, they become detached like the bulbels of the marchantia or lily. But under certain conditions, a new and different set of buds, containing a sexual apparatus, are produced; these, too, become detached, and, by their inherent power of movement, they carry the germs of a new generation to a distance from the parent stock. The whole of these phenomena appear to us to constitute but a *single* generation, instead of making *two*, as represented by Steenstrup. We are not in the habit of speaking of the leaf-buds and the flower-buds of a plant as of two distinct generations; nor, if our comparison be correct, have we any ground for giving such a designation to the polypoid larva and the medusa-imago, which are continuous developments from the same germ. Hence the whole doctrine of the "alternation of generations" falls to the ground, so far as this individual case is concerned; the phenomena being simply those of metamorphosis or change of form, attending the evolution of successive products from the same original germ. This metamorphosis is not really so great as that which presents itself in the course of the development of any one of the higher organisms, the several parts of which depart more widely from each other, and from the early embryonic cell-cluster, than do the polype-buds and medusa-buds we have been describing. The chief difference lies in the capacity of the latter to maintain a separate and independent existence; a capacity which is evidently connected with their low type of organization.

The correctness of the view we have taken will be made apparent, we think, by considering the converse series of phenomena, which has lately become known to naturalists; and we shall now pass from the cases, in which animals well known in their free, adult, or medusoid condition have been ascertained to originate in a fixed, larval, or polypoid form, to those in which animals well known in their zoophytic state have been found to give origin to medusæ. Some very interesting examples of this development are given by Sars in the work whose title heads this article; and others are recorded by M. Dujardin in his paper in the 'Annales des Sciences Naturelles.' The polypes described by Sars as producing medusa-buds are the *Syncoryna Sarsii* (enumerated by Dr. Johnson* as a British species), the *Podocoryna carnea* (stated by the same authority† to be identical with the *Hydractinia rosea* of Van Beneden), and the *Perigonimus muscoides*, a new form first described by himself; all these belong to the family *Tubularidæ*, which is nearly allied to the common hydra. They produce polype-buds also; but these, instead of being thrown off from the body of the parent—as in the hydra, and in the hydra-like larva of the medusæ,—remain connected with it, and thus produce a compound plant-like structure. The reproductive buds, whose medusan nature had been obscurely seen by other observers, but is fully demonstrated by the beautiful figures and clear descriptions of Sars, pullulate laterally from the parent polypes, just beneath the tentacula of the latter; and their growth no more interferes with its ordinary form and functions, than does the development of a polype-bud. The development of these medusa-buds has not yet been watched, subsequently to the time of their detachment from the plant-like polype-structure which bears them; but there can be no reasonable doubt, we think, when the absence of any other reproductive organs is taken into account, that a proper sexual apparatus is destined to be developed in them, the operations of which will terminate in the production of true ova; and that from these ova will be evolved polypes of the same character with the originals. The memoir of M. Dujardin contains similar observations upon two other species of *Syncorynæ*, designated as the *S. decipiens* and the *S. glandulosa;* and upon an allied form which he has described as a distinct genus under the name of *Stauridia*. His observations upon this last are of considerable importance; as he has been fortunate enough to complete the entire circle. He has not only watched the evolution of the medusa-bud from the polype-structure; but he has ascertained that certain (at least) of the medusa-buds deposit ova, and that these ova are first developed into polypes, which go through a series of phases identical with those already described. And we should be doing injustice to M. Dujardin were we not to state that, so far as regards this individual case, he clearly states what we believe to be the true relations of the two forms,—speaking of the hydraform polype as the vegetative phase, and the free medusa as the reproductive phase of the same organism. It is worth while to remark that, in the case of the medusa produced from the *stauridia*,—the only case in which *both* phases have been completely made out,—the medusa-buds do not seem to undergo any great alteration in form, subsequently to their detachment; the principal change being noticed in the increased development of parts, which had already begun to manifest themselves.

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* British Zoophytes, vol. i, p. 43.

† Op. cit. p. 463.

The *Tubulariæ* proper have been especially studied by Van Beneden and Sir J. G. Dalyell; the researches of the former being recorded in the first of the elaborate memoirs whose titles we have stated above; and those of the latter in the beautiful work before us. From the latter we shall quote a general *resumé*, which will serve to indicate to such of our readers as are not acquainted with the animal, the general features of its structure and history, and will serve to add to the knowledge which others may possess regarding it, since it embodies the results of Sir J. G. Dalyell's original investigations.

"1. The *Tubularia indivisa* consists of a single living hydra, sustained on a fistulous stem, rooted irreversibly to a solid foundation.

"2. The stem is occupied by a tenacious matter, or pith, essential to the permanent life and the animal functions of the product.

"3. An external ovarium (?), composed of several clusters, is borne by each hydra; each cluster consisting of several cysts, and each cyst containing an ovum or embryo [probably rather a bud or bulbel].

"4. On expulsion of the ovum (?) from the cyst, a hydra is unfolded, which enjoys the faculty of locomotion, and in its earlier stages evinces animal nature exclusively.

"5. After a brief display of the locomotive faculty, the hydra becomes rooted permanently, and thus it flourishes, enlarges, multiplies, and dies.

"6. The original hydra reared on the first elongated stem falls from its summit, after a certain but indefinite interval from its evolution, and perishes below.

"7. One or more hydræ, according to the vigour of the specimen, replace in succession that which has first fallen.

"8. If the luxuriance of the hydra be great when the subject is originally recovered from the sea, that of its successor is generally inferior.

"9. No correspondence appears between the dimensions and the number of regenerated organs of the successor, and those of its immediate precursor.

"10. No uniform length of interval prevails between the regeneration of the successive hydræ, some being evolved much more speedily than others.

"11. No uniform duration prevails among the regenerated hydræ.

"12. Prolongation of the stem is absolutely dependent on the existence of the hydra, and the rate of increment generally corresponds to its duration.

"13. Six successive hydræ may be generated from the summit of the same stem in six months.

"14. The germ or elements of each hydra probably reside at some distance from the summit of the stem.

"15. A greater number of hydræ than are apparently allotted by nature to a single stem, may be obtained by artificial sections or subdivisions of it.

"16. By such subdivisions, 22 hydræ have been generated in 550 days from three sections of a single stem.

"17. Monstrosities from external injury to succeeding hydræ before evolution, are gradually effaced in each successor; and symmetry, to which there appears a constant tendency, is restored, in remote generations." (p. 37.)

This continual exuviation and renewal of the polype-head, which terminates the stem, is a very curious feature in the history of the Tubularia. It strongly reminds us of the fall of the leaf in plants; and the analogy really proves to be a close one, if the correspondence between the polype and the leaf-bud be kept in mind. But the phenomenon is not confined to plants and zoophytes; for the processes of decay and exuviation are going on yet more constantly and remarkably in ourselves, even in our most important organs; although they are not manifested in the same obvious manner. Compared with that of plants, the regenerating power of the *Tubularia* would seem to be of extraordinary activity. "None of my observations," says Sir J. G. Dalyell, "have been sufficiently protracted to determine its utmost extent. But is there any vegetable product to rival it in these climates? Any one which blossoms and fructifies so often? We admire their exuberance if they flourish twice within a year. How infinitely more fertile must this be deemed, if its most essential organization be renewed repeatedly within a month!" But how much more active would that of the higher animals prove to be, if we could only watch their changes as closely as we can those of a plant or zoophyte. We should find the most important organs of the body undergoing a daily disintegration, not *en masse*, but as regards their component elements; and we should see the regeneration, within a few hours, of all that had been so changed by the operations in which it had participated, as no longer to be adapted to form part of the organism.

The propagation of the *Tubularia* has been made the particular object of study by M. Van Beneden; and he enumerates three ways in which it may take place:—1. By continuous gemmation; 2, by free gemmations; and, 3, by ova. The *continuous gemmation* is the branching of the parent structure, which takes place in most species of tubularia, although it is not common in the *T. indivisa*, as its name implies. The *free gemmation* of the tubularia is the production of medusa-buds, of which he gives a particular and very interesting description; taking them all the while, however, for young polypes, although he says that any one might suppose them, from their appearance, to be true acalephæ. He did not trace their development subsequently to their detachment from the parent structure, but *supposes* them to become fixed after a time, and to shoot up a stem analogous to that of the parent. This idea is altogether inadmissible. That which he (in common with Sir J. G. Dalyell) terms propagation *by ova*, appears to us to be only another form of gemmation, terminating in the production of polypes, which do not quit their attachment until they are able to maintain their own existence. The essential parts of an ovum are wanting, nothing like a fertilizing apparatus can be detected, and the germ is evolved in organic connexion with the parent. We have little hesitation, therefore, in regarding these as free polype-buds, analogous to those of the hydra; and in believing that, as in other cases, the medusa-buds are destined to produce the real ova from which the next generation is to spring. The bud is inclosed within a pear-shaped capsule, and its form and character are not discernible until it is set free by the rupture of this. Its expulsion takes place slowly and gradually; and at the time of its emersion its organs are not fully developed. These are fully evolved, however, after a few days of inactive life; and the nascent polype becomes permanently and firmly rooted on the plane supporting it. Sir J. G. Dalyell has observed the development of medusa-buds from an allied zoophyte, *Tabularia ramosa*, or *Eudendrium ramosum;* but restricting himself, with laudable caution, to what he actually sees, he does not speculate upon the manner in which the medusa returns to the polype form. This genus also produces free polype-buds or bulbels, like those of the tubularia; but the young animal comes forth from them in a different state, having a minute oblong flattened body, destitute of visible organs, very much like that of a minute *Planaria*, and, like that animal, crawling upon its under surface with considerable activity, and changing its direction if it meets with an obstacle. Bodies of this kind, which Sir J. G. Dalyell has frequently met with in the course of his extensive researches, are designated by him as *Planulæ*, and the term is a good one, as expressing their general aspect and kind of movement. The planula of the eudendrium, after being discharged from the vesicle of its parent, continues thus to move for some days; but it at last attaches itself, and sends up a stem, terminated by a polype-head, from which the whole branching structure may be regenerated. Sars also figures and describes capsules in his *Podocoryna carnea*, from which proceed bodies corresponding with Sir J. G. Dalyell's planulæ.

We have now to proceed to the third leading type of the hydraform polypes, the *Sertularidæ*, in which there is a compound branching polype-structure, of horny texture, furnished not only with cells for the protection of the polypes, but also with deciduous "ovarian vesicles," within which the reproductive bodies are developed, and from which they are set free. Considerable discussion has taken place among naturalists, with respect to the nature of these reproductive bodies; and it appears to us that we are only just now beginning to obtain a glimpse of the truth in regard to them, and that this truth can only be elucidated by observations of a much more careful, minute, and comprehensive nature than have yet been made. One source of error has bee[illegible]s we think we can presently show, that naturalists have confounded two sets of objects entirely different; and that there has consequently been a great deal of good discussion wasted. The observations of Sir J. G. Dalyell tend to clear up the mystery, but they do not altogether dissipate it.

The family *Sertularidæ* may be divided into two subordinate tribes, the *Sertularinæ* and the *Campanularinæ;* in the former the cells of the polypes and the so-called ovarian vesicles are sessile on the stems and branches, projecting from them laterally; whilst in the latter the polype-cells are developed at the ends of long stalks, as are also the ovarian vesicles. This would seem to be a comparatively trifling structural difference; but the physiological differences which present themselves in the observation of the reproductive process are far more important. We have seen that several polypes of the family *Tubularidæ* produce two kinds of detached buds or bulbels; those which come forth as *planulæ,* and are at once developed into polypes; and those which come forth as *medusæ,* and which can only give origin to polypes by producing ova from which they shall spring. Now Sir J. G. Dalyell's observations, carried on for a series of years upon numerous members of the *sertularian* tribe, have never led him to detect any other product from the "ovarian vesicles," than *planulæ* which at once go on to lay the foundation for a new stem. Whether these planulæ are to be regarded as buds or bulbels early detached, or as the product of real ova developed within ovigerous buds which never themselves expand, cannot yet be stated with confidence; in the latter case the "ovarian vesicle" (which has been shown by Professor E. Forbes to be an undeveloped branch) might be likened to the "cone" of the coniferæ.

On the other hand, Sir J. G. Dalyell observed, many years since, that the "ovarian vesicles" in the *Campanularinæ* set free true medusæ; and this statement is fully borne out by the descriptions and figures given by Van Beneden; although that able observer made the same mistake as in the case of the Tubularia,—considering these bodies as really young polypes, though bearing a very strong resemblance to medusæ, and as destined gradually to become fixed, and to be converted into the parental form. For this notion there is no foundation whatever in any observed phenomenon; and it is most obviously contrary to all analogy; for *there is no known instance of a medusa itself returning to the polypoid form, or reproducing that form in any other way than by true ova;* whilst, on the other hand, *the polypoid forms give rise to medusæ by the process of gemmation, or continuous growth.* That this last statement is as true of the medusæ developed within the vesicles of the Campanularia, as it is of those which bud off from the exterior of a Coryne, appears from the history of their production as recorded by Van Beneden; although, with the same want of discrimination, and somewhat prejudiced attachment to former views, which he has elsewhere manifested, he constantly terms them ova. No male apparatus whatever, even in that simple condition manifested in the hydra, has been detected in these compound polypes; and the mass, termed by Van Beneden the vitellus, is expressly stated by him to be in continuity with the pith-like substance of the interior of the stem and branches of the zoophyte; which at once appears to us to demonstrate that it is a peculiar form of bud, and has no claim to the title of an ovum.

Other excellent observers, however, have given very diverse accounts of the condition and development of the embryos of the *Campanularinæ.* Thus, whilst the descriptions of Ellis bear enough resemblance to those of Dalyell and Van Beneden, to make us believe that they witnessed similar phenomena, Cavolini, Grant, Lister and the eminent Swedish naturalist Lovén, have described appearances that would seem irreconcileable with these. For, according to them, the embryo comes forth from the ovarian vesicle in the condition of a ciliated "gemmule," destitute of external organs; this body, after moving freely for a certain time, attaches itself to a flat surface, on which it spreads itself out, and then shoots up a stem that bears the first polype-cell;—closely resembling, in its whole history, the "planula" of the *sertularinæ* as described by Sir J. G. Dalyell.

It would appear at first sight impossible to reconcile these very contradictory statements; but we think that it is not difficult to show that they may both be true,—the observers in question not having seen the same objects. For although many of them may have watched the beautiful zoophytes they were studying for long consecutive periods, yet it is by no means impossible that, of two modes of propagation, one only may have presented itself during their researches. Let it be borne in mind that the *Hydra tuba* was kept by Sir J. G. Dalyell for *many years*, without the manifestation of any other than its polypoid method of reproduction; and that the development of medusa-buds takes place only under certain conditions not yet fully understood,—temperature appearing to have the chief influence, since they are found only in the spring of the year. Now, according to the observations of the last-named investigator, certain species of campanularia propagate in two ways;—namely, by free *medusa-buds*, and also by *planulæ*, which at once develope themselves into polype-buds; and it is pretty evident that whilst the former are the bodies described by Van Beneden and some former observers, the latter are the bodies which have presented themselves to Cavolini, Grant, Lister, and Lovén. In what light they are to be considered, we do not at present feel justified in positively asserting. We should have been disposed to regard them as detached polype-buds or bulbels, were it not for the statements of Lister* and Lovén† as to the history of their development. These observers represent them as developed from within medusa-like bodies that are generated in the interior of the "ovarian vesicles;" and describe these bodies as successively expanding themselves at the top of the "ovarian vesicles," setting free two or three contained ova, and then withering or dying off, to be replaced by others. Now if this account be correct,—and the high character of Lovén and Lister, as observers, together with its general agreement with the previous statements of Lœfling and Grant, would lead us to attach great weight to it,—the histories of the process so variously described may be thus harmonized and interpreted. The so-called "ovarian vesicle" is nothing else than a common receptacle (a contracted branch according to Prof. E. Forbes), within which a number of ovigerous buds—answering to the flower-buds of plants—are developed; and within these are formed, by a truly sexual process, the real ova. In some cases these ovigerous buds become altogether detached, escape from the ovarian vesicle as medusæ, and are carried by their spontaneous movements to a distance, even previously to the development of their ova (as observed by Dalyell and Van Beneden). In other cases, the ovigerous buds are not detached, but successively expand at the summit of the ovarian vesicle, liberate their ova, and then wither, precisely after the manner of the blossoms of a plant (as seen by Lœfling, Lister, and Lovén). And it would seem by no means impossible that, in other cases again, the ciliated gemmules or *planulæ* may be really produced within ovigerous buds, although these never expand at the summit of the ovarian vesicle or make their exit from it; and this method, if it have a real existence, is that which we should expect to find in the *Sertularinæ*, in which (as we have seen) no trace of medusa-buds has yet been detected, the young issuing from the "ovarian vesicles" in the condition of *planulæ*, each of which is developed into the formation of a new polype-structure.

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* Philos. Trans., 1834. † Wiegman's Archiv, 1837.

We would beg to call the particular attention of marine observers to the solution of the various interesting questions here indicated,—namely, the relation of the two modes of reproduction in the Campanularinæ; the ultimate history of the detached medusa-buds, which at present can only be judged of analogically; and the real nature of the early development of the ova within the "ovarian vesicles" of the *Sertularinæ*. When these points shall have been fully elucidated, we shall have a tolerable clear notion of the relation between the hydraform polypes and the medusæ, so far as this can be determined by observation of the former group. But it will still remain to be determined whether all, or what particular tribes, of the latter pass through the polypoid condition; and this, owing to the extreme delicacy of the subjects, and the difficulty of preserving them in artificial *vivaria*, must be long in being accomplished. At present we feel much disposed to regard the hydraform polypes and the pulmonigrade acalephæ as together constituting one group; distinct alike, on the one hand, from the helianthoid or actiniform zoophytes, as well as from the bryozoa or ciliobrachiate polypes (which are more probably degraded molluscs); whilst, on the other hand, they must be equally removed from the beroidæ, physalidæ, and other orders with which they have been associated, but with which they seem to have little in common, save their softness of texture and their locomotive powers. This view of the identity of the two groups above named seems to be that taken by Sars in the most recent observations on the subject (op. cit. pp. 13-16); for in commenting on the statement of Steenstrup, that the polypoid larva of the medusa (the Hydra tuba of Sir J. G. Dalyell) is still a medusa though fixed—since its body is distinctly quadrangular, and four canals pass along these angles to terminate in a circular canal that surrounds the mouth (a statement confirmed by Dr. J. Reid),—he remarks that it is not the less a polype; these two groups, although widely separated by systematists, being in reality one and the same, although in different states. He points out that the medusæ are nothing else than free polypes, having peculiar locomotive powers, no other valid distinction having ever been show to exist between them; and he states, as an additional indication of their relationship, the very remarkable fact which he has been (we believe) the first to observe, that even certain medusæ propagate by gemmation. He describes and figures this curious process in the *Cytæis octopunctata;* and he states that it takes place also in the *Thaumantias multicirrata.*

Briefly, then, to recapitulate.—The true *Hydra*, which may be regarded as uniting the general form and structure of the polype with the locomotive powers and dispositions of the medusa, propagates in both the modes characteristic of the Vegetable kingdom; namely by *gemmation*, and by the production of *ova*. The *buds* are not destined to remain in continuity with the parent, but are thrown off like the bulbels of certain plants; having previously acquired, however, the form of the parent.* The *ova* also are developed into polypes resembling the parent. The usual mode of propagation is here by bulbels; the ova being destined, apparently, to continue the race through the winter season, the cold of which might be fatal to the parent.

In other cases, however, we find a greater specialization of characters; the locomotive and proper generative apparatus being especially developed in the *medusæ;* whilst the true polypoid condition presents its most complete evolution in the plant-like *sertularidæ.* Yet these two groups are not to be dissociated from one another; for each of them, in one of its stages of development, presents the characters of the other. The *medusa* begins life as a polype. As a polype it is attached. As a polype it grasps and digests its food. At a polype it reproduces parts that have been removed. And as a polype it propagates by gemmation; the buds being detached from the parent as soon as they have acquired the form of the latter, and are capable of maintaining an independent existence. But in this condition it forms no ova. A new and distinct series of buds (flower-buds) is produced for this purpose; these buds are detached like the preceding; they become developed into perfect medusæ, in which state alone they have been known until recently; and from these medusæ are produced ova by a true sexual process, which are first evolved into the polypoid form, and go through the series of changes just enumerated. It is very important, for the right comprehension of the relation of this process to the development of medusa-buds from the compound polypes, to bear in mind that this production of medusæ by a polypoid larva is *not* effected (as represented by Sars and Steenstrup) by the subdivision of the latter; but, as clearly proved by Sir J. G. Dalyell, by the development of medusa-buds from a polype which continues to retain its original character.

In the *compound hydraform polypes,* on the other hand, the early or zoophytic form may be said to predominate; being the one under which alone the group has until recently been known. Here we have the ovum first developing itself into a single polype; but this polype becomes (like the *plumula* of the germinating plant) the foundation of a ramifying structure more or less extensive, the buds remaining in continuity with the parent, instead of being detached from it. In certain forms of these, we see distinct medusa-buds developing themselves from the exterior of the stems or polype-heads of the zoophyte; these become detached, float freely away, and deposit elsewhere their ova, from which a new generation of zoophytes is to spring; whilst the parent structure continues to live and grow as a zoophyte, like the hydra tuba that has detached a whole brood of medusæ, or like a tree whose period of flowering is over. The chief question yet remaining to be solved, is the real origin of those gemmules of the Sertularinæ, and of those also occasionally produced from the Campanularia and Tubularia, which have the character of *planulæ,* rather resembling the offspring of medusa-buds than the medusa-buds themselves. Are these to be regarded as polype-buds or *bulbels* detached in an earlier state than usual; or are they the products of *ova*, which have been generated within ovigerous buds that have never advanced to their full development? This is a point of great interest; which will not, we hope, long remain undetermined.

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* The following list of plants propagating by bulbels has been furnished by M. Decaisne; the first two reproducing themselves by this method exclusively: *Lunularia vulgaris, Lemna gibba, Dentaria, Dioscorea, Globba amarantina, Gagea villosa, Ornithogalum umbellatum,* and *Lilium bulbiferum.* Other instances are mentioned, in which the true fructification of certain species is invariably wanting in particular localities. See M. Dujardin's Memoir, in Ann. des Sci. Nat. N. S. Zool., tom. ix, p. 379, note.

And now it may be asked, why have we expatiated at such length on a subject that is so remotely connected with practical medicine,—if indeed any connexion whatever can be admitted to exist between the history of the metamorphoses of polypes and medusæ, and the cure of disease in man. Our answer involves numerous considerations, on which we might enlarge at length, if we were not afraid of extending our article beyond the patience of our readers. But our reasons are briefly these. The investigations whose results we have been examining have opened to us an entirely new page in the great book of Nature; of an outline, at least, of whose contents, no well-informed man ought to be unpossessed. Further, the group of zoophytes occupies a position so curiously intermediate, in regard to the manner in which its reproductive operations are carried on, between the Vegetable kingdom on the one hand, and the higher forms of the Animal kingdom on the other, and the correct interpretation of the process as carried on in the latter is so dependent upon the due appreciation of its phenomena in the former, that this alone would constitute a sufficient reason for thus enlarging upon the curious phenomena which it presents. We shall not have occasion to trespass upon the patience of our readers to anything like the same extent in other instances.

Again, we are desirous of enlisting such of our readers as may have the good fortune (for such we deem it) to reside in the neighbourhood of the coast, in the rank of observers in this department of natural history; which has received, we are sorry to say, very little extension in this county, excepting from the valuable researches of Sir J. G. Dalyell. We had hoped much from the labours of Dr. Johnston, of Berwick, in this field; but it appears that he has found it necessary to restrict his attention to the collection, determination, and classification of species; the number of which known to exist on our shores has been vastly increased in the interval that elapsed between the publication of the first and of the (recently-completed) second edition of his beautiful work on the 'British Zoophytes;' this accession being mainly due to the impetus which his labours have given to the study. We are proud to be able to point to a member of our profession, actively and successfully engaged in practice, who has yet found time, not merely to occupy and amuse himself with the observation of Nature, but also to contribute so much to the extension of our knowledge of her wondrous operations. We trust that he may find many imitators; and that the curious physiological questions, which we have adverted to as being yet unsolved, may receive their elucidation (as they ought to do) from members of our own profession. A good microscope is an indispensable requisite for the successful prosecution of such researches; and a previous knowledge of "how to use it," and of the general appearances presented by this class of objects, is at least as essential, as a safeguard against error.

Another reason for the extent and minuteness of the survey which we have taken of this curious subject, is to be found in the useful lessons that may be drawn from the history of the investigation. We have seen how much care and patience have been required in observation; how much greater care in deduction; how easy it would have been, how just it would have seemed, for one observer to give a direct negative to the statements of another, because *he* had not seen the phenomena in question, though he had long and patiently looked for them! And we have also seen how beautiful and perfect is the uniformity of Nature; how phenomena, which at first seemed exceptional, are found, when fully understood, to fall in most admirably with principles that have been already established on a broad induction; whilst crude and hasty generalizations, which were founded upon a limited view of these phenomena, and which seemed to remove them into a new and isolated position, are found not to give a real expression of their nature when this is closely scrutinised, and may be consequently put aside as valueless for the future, although they have had a certain degree of utility in promoting the collection and comparison of analogous facts.

And last, though not least, we are desirous of holding up Sir John G. Dalyell as an example to our readers of every class—as a fine example of a patient, laborious, discriminating, and unprejudiced observer; perseveringly carrying out his researches, not merely day after day, and month after month, but year after year, and, we might almost say, ten years after ten years, for the mere love of truth, without the remotest particle of that self-seeking disposition, which, in these days, tempts almost every young investigator to rush into publicity with the crude results of his inquiries, rather shunning notoriety than courting it, and modestly expressing simply the facts which he has himself witnessed, without throwing doubt or discredit upon the statements of others. We need not say that we shall anxiously look for a continuation of the present publication; and that we trust, alike for the sake of science and for Sir J. G. Dalyell's own reputation, that it will not be long delayed.

ART. XI.

On the Causes and Treatment of Abortion and Sterility: being the Result of an extended Practical Inquiry into the Physiological and Morbid Conditions of the Uterus, with reference especially to Leucorrhœal Affections and the Diseases of Menstruation. By JAMES WHITEHEAD, F.R.C.S., Surgeon to the Manchester and Salford Lying-in Hospial.—*London*, 1847. 8vo, pp. 426.

MR. WHITEHEAD's book affords a gratifying proof of the increased attention which is being directed to the investigation of the diseases of the uterus; and we acknowledge at once that he has written a valuable and original work. We should hardly have been prepared, from the title, for so lengthened and discursive an inquiry into the function of menstruation and its diseases, which takes up full half the book; and, although we think the author would have done wisely to have somewhat abridged this part, yet, with occasional exceptions, there is so much good sense and careful observation in it, that we willingly accept it as it is, and forget its prolixity. We must fully acquit so respectable a practitioner of any intention to give an unfairly alluring designation to his publication; but we should have been very glad to have been spared the necessity of remarking *in limine* upon the striking discordance which exists between the title of the book and its contents.

Mr. Whitehead's opportunities for observing the diseases of which he treats have been derived, in addition to the ordinary run of them in private practice, from the patients of the Manchester and Salford Lying-in Charity, of which he is one of the surgeons. These women, although professedly only attended in their confinement, really apply for and receive relief during gestation and after delivery; and many cases of the functional and organic diseases of the womb are examined and prescribed for. From this public source Mr. Whitehead appears, at great personal toil, to have derived an extended field for observation; and that he has availed himself of it, his numerous well-reported cases and practical remarks supply conclusive evidence. It strikes us that he has assumed rather too independent a character as a writer, for he rarely notices any of the authors who have gone before him; and, although we are quite disposed to limit this form of reference, yet an almost complete rejection of it, in a work like Mr. Whitehead's, is hardly becoming.

The first four chapters are devoted to the subject of menstruation, in the following order: 1st, on menstruation, 2d, the conditions which principally influence menstruation; 3d, the diseases of menstruation; 4th, the last menstrual crisis.

The First Chapter is physiological, and treats a subject which has of late years received much attention from authors, both here and abroad. The appearance of the menses marks the period of puberty; and the *signs of puberty* are first described by Mr. Whitehead. The menstrual flow is periodical; and although a lunar mouth is the normal interval between two periods, yet, like all other functions, this is liable to occasional variations. Mr. Whitehead, with the object of ascertaining how far such variations were compatible with good health, took great pains to learn accurately the times of the recurrence of the flow in "five hundred and twenty intelligent and many of them educated women, in whom menstruation had commenced favorably, and been continued regularly at the periods peculiar to each, for a sufficient length of time together to afford a fair average." Of these, 359 had always been regular, with an interval in the majority of a lunar mouth. In some, however, the interval was shorter, but then it was shorter as a habit; it was, in fact, the natural and regular interval of such persons.

> "Of the remaining one hundred and sixty-one cases, which may be called *irregular*, in fifty-one, menstruation recurred every lunar month, but every third or fourth return a difference of three or more days was observed in the duration of the discharge, and often a difference, also, in the quantity thrown off in a given time; thirty-eight menstruated every lunar month generally, but every third or fourth time from four to seven days earlier; and these deviations were so marked and constant in most instances as to be anticipated at particular times; the amount of excreted fluid was also variable in these cases; fifteen had the menses every three weeks generally, but every third or fourth return from four to seven days later; fourteen every twenty-four days, but occasionally the interval was twenty-eight days; five every five or six weeks, but having occasionally an interval of only a month; two every eighteen days, of whom one had the discharge four days and a free interval of only fourteen—the interval now and then, however, being two or three days longer, and the duration of the discharge cor-

> respondingly abbreviated; the other having, every third or fourth time, an interval of a month. One menstruated every lunar month, but at the middle of every third interval she had an additional discharge every way similar to the catamenial, which continued thirty or forty hours, making no perceptible difference in the regular periods. Two menstruated every fourteen days, in one of whom the discharge generally continued seven days, leaving a free interval of seven,—but occasionally the interval was ten days, and the active period only three or four; in the other case the discharge generally continued two days, but sometimes only a few hours. One individual had the menses monthly, but every third time she missed the period, having a free interval of two months, her health never suffering in consequence. And thirty-two menstruated so irregularly as to afford no means of calculating the periods of recurrence or their duration with any degree of exactness." (pp. 8, 9.)

Mr. Whitehead, in describing the properties of the menstrual blood, has quoted the chemical analysis of it by Dr. Letheby, and the microscopic one of Donné; and he has made some careful observations himself. He concludes that it is similar, if not identical, with the systemic blood, until it becomes mixed with the mucus from the cervix and the vagina, the acid of which acts as a solvent to the coagulating part of it, and retains the blood in a fluid state.

Source of the menstrual secretion. Under this head Mr. Whitehead considers, not only the part whence the menstrual blood exudes, but also the influence which the ovaries have in exciting the flow. In natural menstruation, he believes that the blood proceeds from the whole of the inner surface of the uterus and the upper portion of the cervix. In proof of this he relates the appearance of the uterus of a girl who died from exhaustion, the consequence of menorrhagia. A clot accurately filled the cavities of the body of the uterus and the cervix.

> "The inner surface of the uterus presented numerous openings scattered over every part of it, obvious to the naked view, some being sufficiently large to admit a good-sized bristle, or the end of a lachrymal probe. The largest and most numerous were at each side of the fundus, near the horns of the uterus, and at the contracted part of its body, near the commencement of the cervix. The openings had a valvular arrangement, a great number passing downwards, towards the cervix, while those at the upper part of the organ appeared to pass towards the Fallopian orifices." (p. 35.)

Mr. Whitehead concludes from this case that the process by which the menstrual product is separated from the blood, is by simple exudation from the arterial capillaries in communication with the valvular orifices naturally existing upon the inner surface of the uterus. For our own parts, we regard the determination of this question as possessing much practical interest. The womb is more liable in its morbid states to hemorrhages than any other organ; and these bleedings, in the unimpregnated state, very frequently begin as exaggerations of the menstrual flow. The ordinary monthly flux, as in Mr. Whitehead's case, runs into a profuse and even fatal bleeding; and as Mr. Whitehead has conjectured, the source of the one is the source and seat of the other. Whence then does the menstrual blood come? We venture to object altogether to the term secretion, as applied to it. If it be a secretion, where and what is the secreting structure? The uterine glands have obviously nothing to do with it; and we know of no other structure in the mucous membrane of the uterus, in which such a secretion can be elaborated. But a careful examination of the inner surface of the uterus, in women who have died menstruating, indicates the source of the flow. We have had several opportunities of seeing this; and it appears to us to be beyond dispute that the veins open on the surface of the womb, and that the blood proceeds directly from them. If the womb, when laid open, is gently squeezed, the small veins become visibly filled, and blood comes out from them, first as so many scattered points of extravasation, then as a small trickling stream. It is not often that the veins are seen with valvular orifices, such as Mr. Whitehead has described them; but in one case which we witnessed, where a woman affected with jaundice died, in consequence of hemorrhage from several of the mucous surfaces, we noticed the same appearance in the cavity of the uterus. A triangular clot was moulded to the inside of the womb, and the lining membrane was slightly raised; and numerous patent orifices, from which blood flowed when the uterus was pressed, showed the open state of the veins. The fact of the menses when taken from the uterus having all the chemical and microscopical elements of blood, and the sensible properties of venous rather than arterial blood, is an obvious confirmation of this view. It is these veins, so small when giving out the menstrual flux, which become more and more open as the womb increases during gestation. Those of them which are covered over by the placenta, supply that prolongation of the venous tissue which forms the containing structure of the mother's blood in the placenta; whilst those which are not engaged in this office can be traced, with their valvular orifices surrounded and guarded by the superficial layer of muscular fibres, and their blood prevented from escaping by the uterine decidua. If the placenta be slightly separated, or the decidua loosened, hemorrhage takes place from the open orifices of these veins. The fluid of the lochia, which by some again is called a secretion, is truly a flux of blood from the contracted mouths of these same veins; which are additionally secured at this time by firm clots within them. It is only by appreciating rightly this peculiar organization of the venous system of the womb,—first, during the catamenial flow,—and then in its amplification during gestation,—that we can understand the mechanism of the bleedings in some of the diseases of the unimpregnated womb, and the accidental and unavoidable hemorrhages which complicate parturition.

We might illustrate this subject still further, by referring to the part which these veins take in the organization of some forms of polypi, and in the bleedings which result from them. If a fibrous tumour is developed near the peritoneal surface, and stretches out the serous membrane, very little beyond the mechanical inconvenience is experienced from it. If it be developed in the centre of the uterine wall, and it enlarges evenly so as not to encroach on the cavity of the uterus, the rule is that bleeding is not a symptom. But let the fibrous tumour be directed in its growth inwards, so as to press out the lining membrane of the uterus; and during the whole of the process, its most important characteristic is, that hemorrhage more or less profuse, accompanies it. And why? simply because the veins enlarge as in pregnancy, assuming their peculiar plexiform arrangement; and when the tumour bulges inwards, the veins on the surface, as in pregnancy, become developed, and expose their open valvular orifices, which are not guarded and shut up, as they are during gestation, by the uterine decidua. Hence it is, that copious venous hemorrhage is the dangerous symptom of polypi; and the source of the bleeding is from the veins, which in some polypi collect in the stalk, and in others pervade the entire growth, ending on the surface with their characteristic open mouths. Mr. Whitehead has not alluded at any length to the theory of menstruation which has lately been so fully discussed; the prominent feature of which is, that one or more mature ova escape from the Graafian follicle and ovarium into the oviduct, and, if not impregnated, are eventually passed off through the vagina. We fancy, from the general tenor of his observations, that Mr. Whitehead hesitates to receive this theory as proved —and we have ourselves always entertained great doubts of its correctness, as far at least as regards the *constancy* of the phenomenon alluded to.*

Mr. Whitehead thinks that the menstrual blood proceeds entirely from the inner surface of the uterus, including the upper part of the cervix. Why he thinks that any portion of the cervix is engaged in the performance of this function, he does not explain; and we have never seen any appearance which confirms this supposition. Indeed, we feel convinced that not one drop of the menstrual flux is yielded by the cervix; and that in this function, as in pregnancy, the body of the uterus and the cervix are quite independent of one another, and do not yield the same products or perform similar functions. The first effect of pregnancy on the womb is to increase its vascularity, and to change its mucous membrane into a decidua. This decidua, however, is not formed by the cervix; but is strictly limited to the proper cavity of the uterus. The glands of the cervix yield their peculiar mucus, which blocks up this inlet to the womb. So, again, in menstruation, as we have ourselves had convincing proof, the blood exudes from the cavity of the womb; but there is no similar flow from the cervix. The latter secretes at this time its thick, tenacious mucus, the salts from which assist in preventing the coagulation of the menstrual blood.

Spurious menstruation. We must here notice a remark of Mr. Whitehead's, with reference to what he terms spurious menstruation; a term, he says, "which may be appropriately used to denote a class of symptoms consisting in the development of the menstrual phenomena during pregnancy and lactation, and in certain states of morbid plethora of the uterus and surrounding organs." He further says, that "under whichever of these conditions it occurs, it is invariably associated with a morbid state of parts situated external to the uterine cavity, generally of its cervix and labia, sometimes of a portion of the vaginal mucous membrane." In another chapter, Mr. Whitehead has entered more fully into the subject of menstruation during pregnancy; and he has given the results of a specular examination of the lower portion of the uterus and vagina during the spurious menstrual flow. That many females have a periodical discharge from the sexual organs during the first few months of pregnancy, in every notable respect similar to their ordinary catamenia, is well known, and is fully admitted by our author. The cause of it, however, has hitherto been involved in some obscurity. We have always entertained the conviction that it was an exudation of blood from the upper part of the vagina and the exposed surface of the cervix, and that it was a salutary effort to relieve a morbidly congested state of the sexual organs. So far as our experience goes, it usually occurs in plethoric women, or in those who have fed too well, and have indulged in luxurious and inactive habits during their pregnancy; and it certainly has not been associated, as a rule, with any general ill-health, or to our knowledge, with uterine disorder. Mr. Whitehead's statement,—that "the blood discharged in these cases is furnished, not by the lining membrane of the uterus, nor by any healthy secreting surface, except sometimes, perhaps, the inferior part of the inner cervix; but by the lower extremity of the uterus external to its cavity, or by the contiguous vaginal reflection, being in a state of suppurative inflammation,"—has quite taken us by surprise. Still more formidable is the catalogue of organic changes which the speculum has revealed.

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* Vide Medico-Chirurgical Review, 1845.

> "On examination with the speculum, inflammation or ulceration of one or both labia, or of the cervix uteri, complicated, in some instances, with warty excrescences growing from the cervix, or from some part of the vaginal membrane, vaginitis, &c., was met with in every case, without an exception." (p. 222.)

So that, according to Mr. Whitehead, women who have these periodical discharges during pregnancy, are to be looked upon as presumptively the subjects of one or more of these diseases. Surely there is some mistake here. We feel persuaded that there can be no one of any extensive practical experience, who has not seen many cases of menstruation during pregnancy and lactation, and yet without any complaint on the part of the patients, indicative of disease of the uterus or vagina. It is their habit to menstruate at these times; and they may have done so during several successive pregnancies. Is it to be supposed that such diseased surfaces are a necessary accompaniment of gestation in such persons? and are they to be exposed to an examination by the speculum, that these may be detected and treated? We cannot but express our conviction that, invaluable as the speculum is in investigating and treating diseases of the uterus and vagina, the facility with which it may be used has suggested its too frequent employment; and we should decidedly object to propose such an examination in the cases under consideration. Is it not just possible that the appearance of a highly-congested vaginal mucous membrane, from which blood is oozing, with its usual accompaniment leucorrhœa, may have misled even so good an observer as Mr. Whitehead?

If diseases, such as he has described, are the cause of these discharges, why do they occur periodically only, and observe so just a relation to the ordinary menstrual flux? We admit that we have never examined by speculum a woman during pregnancy, when in the act of menstruating? but we have very frequently applied remedies, by the speculum, in cases of ulceration of the cervix, of vascular granulations and vegetations from one or both margins of the os, in diseases of the vagina, too, *occurring in pregnant women;* and, so far as our memory carries us, not one of these women has been subject to spurious menstruation. This, no doubt, may be a coincidence; but it goes, at any rate, to prove that the presence of such diseases does not entail sanguineous discharges.

We cannot follow our author through the chapter on the "Conditions which principally influence menstruation, at its commencement;" although it is full of interesting facts, which have been collected and recorded with the most praiseworthy care.

The average age of the first menstrual crises in 4000 persons, taken indiscriminately from the poor and the rich, was found to be fifteen years and nearly seven months; and the greatest number, 967, occurred at 16 years of age; there being 761 at 15 years of age. This, we believe, does not tally with Mr. Robertson's account of 450 women at Manchester: the respective numbers for the ages 15 and 16 being 97 and 76. Statistics of this kind are sadly open to fluctuations; and the average of 400 differs from the average of 4000, as this may from a still greater number.

Mr. Whitehead has discussed fully and ably the influence of temperament and habit of body, in determining the character of disease in difficult menstruation. The subject is interesting; but, practically, very difficult to follow out. When a temperament is well marked, there is, of course, no difficulty in detecting it; but the combinations of temperament in particular individuals are extremely embarrassing, and prevent uniformity in estimating them. The influence of temperament in relation to menstruation has particularly been noticed by M. Brierre de Boismont; but, as usual, Mr. Whitehead does not refer to him.

The effects of different occupations, in retarding or anticipating the period for the establishment of puberty, have generally been supposed to measure their healthful or deleterious character. We have been accustomed to look upon factory employment as very pernicious in this respect; and it is with much pleasure that we learn from Mr. Whitehead that it is not so. The chapter "On employment as influencing the crisis of puberty," is replete with facts concerning the workers in the Manchester mills, which prove that in them puberty is established at a fair average age, and with less difficulty and attendant disease than in the more refined and educated classes. The general results are concisely stated in an interesting table (p. 82).

Mr. Whitehead was, until lately, of opinion that factory labour had the tendency prematurely to excite the sexual organs, and to induce a precocious womanhood; but he now regards this opinion as a popular fallacy; and, as the table just referred to shows, mill employment has not this pernicious influence. It is to moral rather than to physical agency, that we are to look for the cause of a too early development of the sexual organs. That forcing of the mind and the imagination, which is so injudiciously practised in the education of females at schools, just when the body needs sustenance and care, is infinitely more likely to interfere with the healthy establishment of puberty, than the heavy drudgery and daily toil of a factory life. Our author insists on this point with much decision; and there is no judicious practitioner who has not to deplore the fearful price at which educational accomplishments are too frequently purchased.

Influence of climate. A very general impression has prevailed that females, like fruit and vegetables, may be forced into a premature development by the warmth of climate; and that the early menstruation in the inhabitants of southern and tropical regions is thus to be accounted for. Mr. Robertson's researches appear to us to have effectually proved this analogy to be unsound; and to have shown that a lax morality and early marriages are the principal agents in causing it. Our author is disposed, however, to lay stress on the "influence of climate in determining an early sexual development, although not perhaps to the extent that it was formerly believed to do."

Diseases of menstruation. A cursory and, in some respects, a faulty and incomplete notice of the several functional diseases is included under this title. Retention and suppression of the menses, dysmenorrhœa, vicarious and menorrhagic menstruation, are successively described. Some half dozen cases are detailed of various diseases occurring in females in whom the menses were retarded; which were all greatly mitigated or cured by the establishment of the flux, and for their diversity appear to be well and aptly selected. They all present symptoms of congestion in different parts of the body. A case of jaundice, in a young lady, æt. 14, yields to a mercurial aperient, which unexpectedly produces salivation, and the sudden accession of the menstrual discharge. A girl, æt. 15, has an attack of pulmonary congestion, for which she is bled and purged. The abdomen, which was tumid and painful, is covered with a hot poultice, and the following night menstruation comes on. A sempstress, æt. 18, had suffered for three years from a chronic affection of the knee-joint, which was looked upon as white-swelling: after exposure to the rain, she had an attack of peritonitis, which was subdued by leeches, &c.; and during convalescence the menses came on: after this the white-swelling subsided. A case of chronic strumous abscess, and another of hysterical epilepsy, which were cured on the menses appearing, are recorded. The retention of the menses from some obstacle to their passage through the sexual organs, is not described, as Mr. Whitehead has never seen a case of the kind.

Amenorrhœa suddenly induced, is the only form of *suppression of the menses* which is treated of; and several cases, illustrating the various and dangerous diseases which are set up in distant organs on this abrupt arrest of the flux, are related. Peritonitis and congestion of the brain and lungs, are severally alluded to; and the only fault we venture to notice with reference to them, is a too free use of depletory measures. We allude to cases xii and xiv, especially the latter; and our conviction is that Mr. Whitehead's patient would have had a better chance of relief and safety had she been bled more sparingly.

Dysmenorrhœa is most imperfectly described. There is no attempt at dividing it according to the different morbid states which accompany and define it; and the cases are but poor illustrations of so important a subject. A neuralgic or irritable state of the uterus is cursorily noticed; and so also the occasional membranous products; but Mr. Whitehead seems to know nothing more of the latter, than that they were first noticed by Morgagni, and described by Denman; and he appears to connect them with an inflamed state of the lining membrane of the uterus. The views which have lately been promulgated by Dr. Oldham, Dr. Simpson, and M. Coste, and which appear to clear up the confusion in which the subject of membranous dysmenorrhœa was placed, are not noticed, and we suppose not known. We are the less surprised at this, as our author seems sadly behind in his physiological knowledge of the change which the lining membrane of the womb undergoes in consequence of pregnancy. "At the commencement of pregnancy," he says, "the secretion thrown out by the lining membrane of the uterus, differing at all times in its properties from ordinary mucus, presents some important peculiarities which it does not possess at other times. It is more thick and plastic, and is furnished, for the time, in greater abundance; none of this product, however, escapes from the cavity of the uterus, but becomes adherent to its inner surface, appearing to incorporate itself with the proper mucous tissue, and is ultimately changed into a new organized structure." (p. 228.) This is Mr. Whitehead's description of the decidua vera. He appears never to have studied the changes in the uterine glands, which have been so accurately described by Weber, Sharpey, and Goodsir. The way in which these glands swell out into small sacs, lined by an actively-secreting epithelium, and covered by a vascular network—the first maternal element in the newly-formed placenta—is now clearly made out, and generally taught. And not only is this most important to know, so far as the composition of the placenta is concerned, but it will be found to have a direct bearing on the pathology of membranous dysmenorrhœa, when it is remembered that the decidua vera is formed in consequence of the action going on in the ovary and tube, before the ovum has reached the uterus. The proof of this is clearly seen in the fact, that the decidua is never so exuberant as when the ovum is developed in the tube, or interstitially. We possess two preparations, one of tubal gestation, and the other of a parietal extra-uterine gestation, both having terminated fatally by the bursting of the cyst and the extravasation of blood into the peritoneum; and in both the uterine decidua is highly developed. Now it appears that some morbid conditions of the ovary, connected with its function in exciting the menstrual flow, may call forth a sympathetic swelling and growth of the uterine glands; so that the mucous membrane is changed into a decidua, which is cast off by the painful contractions of the womb, producing the membranous dysmenorrhœa. We have at this moment before us a specimen of the membrane, which was passed very recently by a patient of our own. It is an irregular flap, with a smooth, free surface, and a rough attached one; and strikingly resembles a portion of uterine decidua in an ovum aborted about the fifth or sixth week. But the characteristic feature, which proves the identity of the two, is the number of small apertures which pervade it; these being perfectly visible when the membrane is put upon a black ground. We need scarcely observe that this view of the formation of this product in dysmenorrhœa affords a clue to its proper pathology and rational treatment; and upsets the theory of an inflammatory state of the lining membrane of the uterus (the endo-uteritis of Mr. Whitehead), just as Weber's and Sharpey's discovery of the formation of the decidua vera has refuted William Hunter's view of its origin from lymph.

Mr. Whitehead has not noticed the mechanical form of dysmenorrhœa first described by Dr. Macintosh, and the mode of overcoming it by incision, which originated with Dr. Simpson, and has been practised successfully by others. Our author classes vicarious menstruation as a variety of dysmenorrhœa: and he justly remarks, that it is from the mucous membranes and the skin that compensating discharges usually take place:

> "But any other organ may occasionally be the seat of vicarious disease; and besides periodical hemorrhage from the nose, mouth, bronchial tubes, alimentary canal, or the skin; catarrhs, diarrhœa, or eruptive disorders; an attack of rheumatism, asthma, or cellular inflammation; abscesses, dropsical effusion, or vicarious ulcers, may be the means of relieving the system of, what the Hippocratic doctrine styles, its peccant humors." (pp. 133-4.)

Last menstrual crisis. Under this title our author describes the various diseases which are apt to attack the mouth and neck of the womb, at or after the final cessation of the menses. The tendency to disease, not only of the womb but of other organs of the body, particularly the digestive organs and the heart, at this critical period of female life is well known; but our author confines his observations to those only which implicate the lower extremity of the uterus and vagina, and he does not include affections of the ovaries and the organic diseases of the uterus. As a practical arrangement of the forms of disease which he describes, he has characterized them by the discharges which accompany them, in the following manner:

> "The first, and most common of these affections, is characterized by a muco-purulent discharge from the vagina, generally denominated leucorrhœa, or the whites, but which differs from simple leucorrhœa in several important particulars; the second is often accompanied by vaginal hemorrhage, the discharge differing, both as to its properties and its source, from the menstrual product; the third form is characterized by a watery, sanious, serous, or ichorous discharge, which is sometimes mixed with blood, sometimes with pus, mucus, or albumen-like shreds, and occasionally with small portions of fleshy matter, the product of the organic change upon which it depends. It generally emits an offensive odour." (p. 155.)

The particular affection which the author describes and illustrates as being attended with muco-purulent leucorrhœa, is hypertrophy and induration of the cervix, with ulceration of one or both lips. He notices also the tendency of the anterior lip alone to be enlarged and projecting, with a superficial granulating surface on its posterior aspect. Besides this disease of the cervix, he enters more fully into the description of endo-uteritis, or, in other words, inflammation of the lining membrane of the womb; to which we shall refer immediately. Neither of these diseases, we may say, are peculiar to the last menstrual crisis; the latter indeed, in our opinion, is far more commonly an affection of middle life. It is certainly, however, at the change of life, that we frequently meet with the large, tumid, hardened cervix, with a gaping os uteri and a granulating ulcer; and this is attended with a more or less profuse white-of-egg discharge mixed with pus. But we think our author does not insist sufficiently on the attendant swelling and induration of the body of the womb, which, in our experience, most usually accompanies it. It is generally, we believe, this implication of the body of the uterus,—rendering it as an organ, large, massive, and bulky, and oftentimes causing its displacement from over-weight,—which calls forth the sympathetic disturbance of the stomach, bowels, heart, and brain, so painful and trying to the patient. Diseases of the cervix only, such as ulceration in its common forms, and a thickening and hardening of its structures, are not in our observation attended with nearly so great an amount of distant distress and disorder, as are congestions of the body of the womb. The case which Mr. Whitehead has narrated was accompanied with dyspepsia, and yielded after a time to the application of a strong solution of nitrate of silver to the diseased surface, and to attention to the digestive organs. Mr. Whitehead might have insisted on the way in which the brain at this period of life becomes affected; in connexion with, and apparently produced by, the same condition of the uterus. We have ourselves lately had the satisfaction of perfectly relieving a lady who for seven years had been the subject of dyspepsia, with a disposition to mania on religious topics. Her digestive organs had been attended to with the most scrupulous care; and sedatives of various kinds, with change of scene and air, had frequently been had recourse to, but only with partial and temporary benefit. When first we saw her, which is now about eleven months since, our attention was directed to the uterus. It was about the time of the cessation of the menses, which had been interrupted for two or three periods in succession, and had then flowed profusely. She complained of pains in the inguinal regions, particularly on the left side; with lumbar pain, and a frequent heat and pruritus of the external organs, with a scanty muco-purulent discharge. On examination, the cervix was found low down, and directed forward. Both lips of the womb were large and hard; and the anterior one descended so much below its fellow, that it felt more like a small polypus growing from it than the lip itself. The body of the uterus was large and heavy, and had fallen back towards the sacrum. The posterior wall was readily felt projecting abruptly from the cervix, and giving that feel which authors, we believe, erroneously have called retroflexion of the womb. On applying the index-finger to the front part of the lengthened anterior lip, and pressing it backwards, the heavy body of the womb was raised and directed forward; when, by pressing with the hand over the pubes, we were able to catch and fix the fundus, and to hold the womb, as it were, by this co-operation of the two hands. When an examination was made by speculum, one lip only could be admitted into a full-sized instrument; and the anterior lip was seen to be studded over its lowest part with numerous red vascular points, which were obviously the glands of the cervix. A more extended patch of injected granulations covered the posterior surface of this lip. The posterior lip was excoriated; and from the os uteri a quantity of thick, tenacious, glairy mucus was easily pressed out. The treatment of this case consisted in repeatedly leeching the upper part of the vagina close by the cervix posteriorly, and occasionally in scarifying the large anterior lip; these forms of local bleeding being followed by marked relief. A warm hip-bath, with an injection of the extract of opium and the liq. plumb., with the application once a week of the solid nitrate of silver, were the principal local remedies. Drachm doses of the liq. hydrarg. bichlorid., with sarsaparilla and bark, were given internally; and the result of this treatment has been a complete relief to the mental disorder, and a great reduction of the swollen uterus, with a cure of the ulceration of the cervix. This lady wears an abdominal belt with a perineal pad; she walks about with ease and comfort; and her general health is good. Cases more or less marked of this description constantly occur in practice; they really centre in the womb; and by attacking them through this organ they yield sometimes with surprising facility.

We must now refer to Mr. Whitehead's description of endo-uteritis:

> "It consists in inflammation of the lining membrane of the uterus, of which sometimes a portion only, but more frequently the entire surface, is implicated. It almost always extends throughout the cervix to its lower aperture; occasionally it is continued along the Fallopian canals to their outer extremities, and thus, in some instances, probably, it may be a cause of disease, of a still less manageable character, affecting these and the neighbouring appendages. In the acute form, in its first stage, a quantity of glairy or ropy mucus is generally thrown off, which,

after a short continuance, is succeeded by a sanious or purulent fluid of darker hue. There exists, at this stage of the complaint, a peculiar disposition to exudation of plastic lymph, which, under some circumstances, becomes organized; being exfoliated and expelled in a membranous form, accompanied by violent bearing-down pains, like those of labour. This substance, which constitutes the dysmenorrhœal membrane already noticed as a condition connected with disordered menstruation, is most frequently observed in women before the commencement of child-bearing; but it also happens under other circumstances.......

"The body of the uterus in cases of endo-uteritis is somewhat enlarged, and painful under pressure of the finger; the cervix is slightly hypertrophied, but not so painful as the upper part of the organ, and it is generally free from abrasion. The only evidence of the existence of the disease, capable of being revealed by the speculum, is the presence of the bright red ring surrounding the verge of the os uteri, together with the escape therefrom of the characteristic fluid product; or of a small quantity of blood, which, by becoming incorporated with the vaginal mucus in its transit outwards, appears at the os externum in the form of sanies." (pp. 161–2.)

After the remarks which we have made on membranous dysmenorrhœa, we need hardly observe that Mr. Whitehead's endo-uteritis is the self-same disease; and, further, that it is founded on a pathological error. The changes which the body of the uterus undergoes during this disease have been fully described by one of the authors alluded to; although, in keeping with Mr. Whitehead's peculiarity, he has not noticed it.* Our own conviction is, that the lining membrane of the cavity of the uterus is very rarely indeed the exclusive seat of inflammation; and when our eye fell on the word endo-uteritis, we were curious to see what Mr. Whitehead would make of it. That the channel of the cervix, and especially Naboth's glands, become inflamed and ulcerate, is obvious enough to every practical inquirer; but we doubt very much if this inflammation and ulceration extends in such cases beyond the os uteri internum. The presence of an inflamed circle around the os is certainly not peculiar to this disease, nor do we believe it to be *invariable.* We would suggest to Mr. Whitehead that he should accurately examine under water, on a black piece of paper or black wax, a good specimen of dysmenorrhœal membrane, and compare it with a portion of decidua at an early period of pregnancy; and we believe that he would then,—supposing, of course, that he has learned how the decidua is formed,—erase the newly-coined word endo-uteritis. In the treatment of this affection, Mr. Whitehead insists on leeching the sacrum and hypogastrium; and he recommends injecting the uterus with a weak solution of nitrate of silver, in combination with extractum conii, or the introduction of an ointment of the same materials. If the view we have been advocating be correct, that the cast-off membrane is an alteration of the mucous lining, dependent on irritation of the ovary, the application of nitrate of silver to the cavity is not likely materially to benefit the patient. Mr. Whitehead informs us that he has witnessed the instantaneous suspension of pain under its use. But when, we ask, has he injected it? The pain is most severe during the menstrual flow, when the womb is separating and expelling the membrane; but surely it is not then that Mr. Whitehead injects the cavity. And then again, with reference to throwing a fluid into the cavity of the womb, what is to prevent its running directly through the tubes into the peritoneal sac? That it does so, recorded cases sufficiently testify; and we cannot but think that much hazard is braved in adopting this mode of treatment. The introduction of an ointment into the cavity of the uterus, as we have ourselves done on some occasions, is not liable to a similar objection.

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* Dr. Oldham on Dysmenorrhœa, Medical Gazette, Nov. 27, 1846.

Two cases are related in illustration of the diseases attended with hemorrhage; the first occurring in a woman æt. 50, who had ceased to menstruate about two years, and had been troubled with leucorrhœa for several years. An attack of hemorrhage came on, which continued in diminished quantity for four months; and on examination the cervix was found tumid and traversed by a number of prominent venous branches, forming what Mr. Whitehead has called a varicose ulcer. The affection was cured by the use of mercury internally, to the extent of salivation; and the hypogastrium was leeched, and nitrate of silver applied to the ulcer. The patient subsequently took tonics and iodide of potassium. The second case is certainly out of place; being an example of accidental hemorrhage, followed by uterine phlebitis, peritonitis, and death. We think our author has chosen a good term to describe that form of ulceration which is accompanied with venous congestion. Sometimes this congestion appears as a dark blue areola around the ulcerated surface; and we have found much benefit in these cases from dividing the veins with a scarifying knife, instead of leeching the hypogastrium or even cupping on the loins. Less blood need be drawn by this method of depletion; it is a more direct topical bleeding, and is attended with the best effects.

We need not refer to the diseases which are characterized by a watery and offensive discharge; as they are, with the exception of a well-reported case of cauliflower excrescence, neither illustrated nor fully described.

Our author has devoted nearly sixty pages to a description of the signs of pregnancy; but without adding materially to our knowledge on this important subject. We learn from him, however, that he regards Dr. Robert Lee's discovery of the vast supply of nerves to the womb, and their enlargement during pregnancy, as satisfactorily made out; and that he considers quickening to be produced by a sudden extrication of the body of the uterus from the brim of the pelvis. This strange idea of quickening, which we believe to be quite erroneous, originated with Dr. Royston, whose name, however, does not appear.

"*Appearance of the os uteri during pregnancy.* The only test," says Mr. Whitehead, "capable of revealing with certainty the existence of pregnancy during its early stages—from a few days after conception to the middle or end of the fourth month, when auscultation becomes available—is that which the appearance of the os uteri presents to specular examination. It was before stated that, during menstruation, the labia uteri were in a state of high vascular turgescence, and the os tincæ, although elongated, and having its boundaries somewhat relaxed, was nevertheless closed and linear, except during the escape of the small menstrual clots before noticed. At the time of conception, the parts are thrown into a precisely similar condition; but no escape of fluid occurs to relieve the turgescence, which consequently continues to increase. In from ten to twenty days afterwards, the whole organ is found considerably enlarged, and the circulation through it augmented both in force and volume; the labia are thickened and apparently elongated, the commissures less distinct, and the os appears to be sunk in, or dimpled, owing to the distension and consequent projection of the labia below the level of the orifice. In the fourth week, the labia, at the centre of their margins, are permanently separated to the extent of one or two lines; and the os tincæ, which was before a mere chink, with parallel boundaries, is now seen to be an elliptical, or sometimes rounded aperture, which is occupied by a deposition of transparent, gelatinous mucus. At six or eight weeks it becomes decidedly oval, or irregularly circular, with a puckered or indented boundary, having a relaxed and lobulated character." (p. 202.)

There are four woodcuts, showing the changes in form and shape which the os uteri undergoes during gestation; but one only as it appears in the early months. The great practical difficulty in the diagnosis of pregnancy is to get a sign or signs, available in the early months, which shall be independent of the testimony of the woman, and not be simulated by disease. Hence the changes in form and volume of the uterus, the mammary signs, and, perhaps we may add, kiestein in the urine, are those on which most reliance are to be placed. Mr. Whitehead has described faithfully the changes in the cervix and os uteri during the early months; and, undoubtedly, he has extended their appreciation and increased their value, by noticing how they appear to the sight, as well as feel to the touch. We do not agree with him, however, in thinking that the changes seen by the speculum are more distinct and significant than those felt by the finger. To combine the two, is to supply a corrective to the failings of each; but of the two we regard the touch as the most valuable sense. These changes in the cervix and os uteri, whether felt or seen, are, we believe, in uncomplicated cases quite sufficient to justify a strong suspicion of pregnancy; a suspicion amounting to a certainty, if the mammæ are swollen, and the areola and nipple turgid and fresh. But we are bound to say from experience, that cases have occurred to us in which similar changes in the form and structure of the cervix have been noticed as the result of functional disorder of the uterus and *ovaries*. Our own clinical experience on this subject has led us to attach great importance to the appreciation by touch of the even globular enlargement of the anterior wall of the womb, and the gradual rising of this organ within the pelvis. The swelling-out of the cavity of the womb to accommodate the growing ovum, can be detected at the sixth or eighth week of pregnancy, through the anterior and upper part of the vagina; and we think it a more truthful sign of this state, than the changes in the os and cervix. The direction of the body of the uterus forwards, when it is enlarged by congestion or disease, is of rare occurrence; as it is then generally displaced backwards; and the regular globular body, swelling out gradually from the cervix, with its obscurely elastic feel, is very significant of pregnancy.

Mr. Whitehead has related several cases, in which conception occurred before menstruation; a fact which is well known, and which proves that graafian vesicles may be maturated, and ova impregnated, independently of the menstrual flux.

We are here reluctantly obliged to suspend, for the present, our comments on Mr. Whitehead's treatise. We shall return to it in our next Number; and shall then subject the more original portion of the work—that which relates to Abortion and its causes—to the same careful analysis and critical scrutiny as that which we have bestowed on its earlier portion. We trust that we shall not be thought to have treated the author with harshness or discourtesy in the free expression we have given to our strictures on what we consider the least valuable portions of his work; more especially as our estimate of its merits, taken as a whole, is so high, that we have no hesitation in cordially recommending it to the attentive notice of our readers.

Art. XII.

Lectures on the Physical Phenomena of Living Beings. By Carlo Matteucci, Professor in the University of Pisa. Translated under the superintendence of Jonathan Pereira, m.d., f.r.s., Vice President of the Royal Medical and Chirurgical Society.—*London*, 1847. Post 8vo., pp. 435.

The general character of Professor Matteucci's work has been made so extensively known by the elaborate reviews which it received from our predecessors, and by the translations of it which have appeared in the pages of the weekly journals, that it is quite unnecessary for us to speak at any length either of its peculiarities or of its merits. With reference to these last, we deem it but just to Dr. Pereira to state the fact, that he had advertised *his* translation as in preparation several months before *they* began to appear; so that whatever merit may be claimed for the introduction of the work to the English reader is fairly due to that gentleman. We think it fortunate for Professor Matteucci, that his treatise should be edited by one so highly qualified, both as regards literary attainments, and practical knowledge of the subjects of which it treats, to do it full justice; and we cannot hesitate for a moment in assigning to the present translation a marked superiority over those with which it has been contemporaneous. It is distinguished by its scrupulous rendering of the exact meaning of the original; and, although elegance is not aimed at, yet it reads by no means disagreeably. We could still point out, however, a few blemishes, which we hope to see removed in another edition. Thus we were not aware that the circulation of the blood can be seen in the *claw* of the frog (p. 358), having always ourselves looked for it in the membrane between the toes; in the preceding page we observe the *separate* sections of the branches of a blood-vessel spoken of as the *partial* sections; in p. 31, by the displacement of a comma, the meaning of the statement is completely altered, for the sentence, "a saline solution filtering through a long tube filled with sand runs out more or less, completely deprived of salt," should read thus, "a saline solution filtering through a long tube filled with sand, runs out more or less completely deprived of salt;" in p. 399 we are startled by meeting the word *conscience* where mere *consciousness* is implied; and in p. 280 we find the name of the physiologist Panizza strangely metamorphosed into Pandyza. We trust that we shall not be considered hypercritical in noticing these trivial blunders. They are all that we have detected after a careful examination of the entire book; and we should not have thought it worth while to enumerate them, save for the sake of showing that we are not blinded by the well-earned reputation of Dr. Pereira to the peccadilloes he may commit, but expose them even more mercilessly than we should those of a less practised writer. Even Homer sometimes nods: and we fancy that our excellent Editor must have been a little oblivious, when he revised the passages we have noticed, as well as another on which we shall comment presently.

But it is pleasant to turn to the more agreeable duty of showing in what the peculiar excellencies of this edition consist. In the first place, it has been made from an *improved* copy of the original, furnished to the

Editor by Professor Matteucci himself, who has made numerous corrections and some important additions. The French edition, although more complete than those which were first published in Italian, nevertheless contains many errors, most of which were ascribable to the translator, whilst some few appear to have been due to a want of care on the part of the author. A copy of this edition was carefully revised by Professor Matteucci; who has not only rectified the mistakes of both kinds, but has also embodied in it the results of his most recent investigations; so that Dr. Pereira's translation must not be regarded as a mere reproduction of any of the editions previously published in France or Italy. Further, it has been enriched with several valuable notes by the Editor; and some additional wood-engravings have also been introduced. We must not omit to mention too, that the French weights, measures, and temperatures have been brought to English standards, wherever it was desirable to give the power of comparison with the latter; a change which, on account of the labour it involves, is very commonly slurred over by editors and translators, but which is of the greatest importance to the reader. It ought to be recollected, that every one who desires to make himself fully acquainted with the fact stated by the author, is obliged to go through the whole process for himself; and thus the neglect of the editor imposes the labour from which he shrinks upon his hundreds or thousands of readers. Dr. Pereira, having been duly impressed with this consideration, has very properly taken this trifling task upon himself; and has thus removed a stumbling-block, which less conscientious translators are content to leave in their readers' way.

As a specimen of the notes which have been added by Dr. Pereira, we shall quote the following; which puts upon its right footing the history of the discovery of endosmose, confused as this has been by a singular resemblance in the names of the two original investigators of the phenomena thus designated.

> "Dutrochet's first memoir on endosmose and exosmose was read to the *Académie Royale des Sciences* on the 23d of July, 1827. Ten years previously, my friend Mr. Porrett, the present treasurer of the Chemical Society of London, had communicated to the editor of the Annals of Philosophy (July, 1816) a paper on two 'curious galvanic experiments,' one of which was the production of endosmose between two liquids separated by a membrane, and subjected to the action of voltaic electricity. He called the phenomena *electro-filtration*; and asks whether, jointly with electro-chemical action, it is not in constant operation in the minute vessels and pores of the animal system? M. Parrot, of St. Petersburgh, has recently presented to the *Académie Royale des Sciences* an inaugural dissertation published in 1803, giving an account of the phenomena presented by two liquids of unequal density, separated by a permeable organic diaphragm, and pointing out their relations to physiology and pathology." (p. 34, note.)

Under the head of respiration, Dr. Pereira adds a note (p. 134), which expressed in a tabular form the mutual diffusion-volumes of the principal gases, according to Professor Graham's law; and also gives a summary of the results of the experiments of Valentin and Brunner, which indicate that the exchange of oxygen and carbonic acid, through the walls of the capillaries of the lungs, takes place in conformity with this law. And further on we have a notice of the interesting researches lately communicated to the Royal Society, by Dr. G. O. Rees, on the conversion of the phosphoric fats found in venous blood, into a tribasic phosphate of soda found in arterial blood, during the respiratory process; of these researches a fuller account will be found in our Periscope.

One of the most important additions made by Professor Matteucci himself, relates to the cause of that very curious phenomenon, the *induced contraction* of a muscle whose nerve is laid upon another muscle in a state of contraction. His former experiments went to prove that the induced contraction cannot be explained by the transmission of any electrical current from the contracting muscle to the nerve laid upon it; and he felt obliged to consider it a phenomenon *sui generis*. With the candour of a true philosopher, however, he now abandons a position which his later inquiries have rendered untenable, and gives us the following statement of his present views:

> "After all, I am compelled to say that, recently, I have resumed the examination of the induced current, by considering it as due to a very feeble electric discharge analogous to that of the Leyden jar. After having seen that excessively feeble discharges produce contractions in frogs, knowing that the presence of these discharges cannot be detected either by the galvanometer or any other instrument, but only by the frog, it appeared worth while to ascertain whether a very slight discharge of the Leyden phial, traversing a muscle, acted on the nerve of a galvanoscopic frog, and under the same laws that we have found induced contraction to do. I must admit that, notwithstanding a great number of endeavours, I have not been able to discover any differences. I wish, then, to tell you frankly, that until new facts are obtained with regard to induced contraction, we cannot decide whether it be due to a nervous induction, or be the effect of an electric discharge occurring during contraction. If we could succeed by experiment in proving the truth of the latter hypothesis, we should have made a grand step in the analogies between muscular contraction and the electric function of fishes." (p. 321.)

We find, in the 'Lecture on the Circulation,' an extraordinary blunder, into which we are much surprised that Professor Matteucci should have fallen, and are at least as much surprised that Dr. Pereira should not have corrected it. "The most simple observation," he says (p. 355,) "proves that the sum of the sections of the small vessels is more considerable than that of the trunks;" and he gives as an instance the division of the aorta, whose diameter at the orifice is 28 millimetres, into two trunks, the diameter of one of which is about 20 and that of the other 16. Now this brings to our minds the old error of physiologists, whom Professor Matteucci seems to have here followed without hesitation, although the point is one into which his acquaintance with physical science ought to have given him a clearer insight than they possessed. The arterial system, according to them, forms a cone, whose apex is at the heart, and whose extended base is in the capillary system; since at every subdivision the combined areæ of the branches is greater than that of the trunk. We remember to have heard, not many years since, of a London water-company, whose managers, having let off at a certain rate as much water as could flow through a pipe of four inches in diameter, sapiently agreed to allow a pipe of double the bore to be used on condition of receiving double the remuneration,—being ignorant of the simple principle in geometry, that circles are to each other as the *squares* of their diameters, and that consequently the sectional area of a pipe only *twice* the diameter of another is really *four* times as great. Physiologists were first set right on this subject by Mr. Fernely, who showed that they had been misled by comparing the *diameters* of the vascular trunks with those of the branches, instead of their *sectional areæ;* and that the latter really correspond very closely. Mr. Paget has since ascertained, by an elaborate series of measurements, that there is sometimes a slight increase, and sometimes a slight decrease in the sectional areæ of the branches as compared with the trunk; the difference, however, being in either case very trifling. Now in the very instance cited by Professor Matteucci, the difference is really contrary to that which he states; for the sectional area of the trunk, being represented by 28 × 28, is 784, whilst the area of the larger branch is 20 × 20, or 400, and that of the smaller branch 16 × 16, or 256; the two added together make only 656, or *less* by one sixth than the area of the trunk,—the difference, however, being probably due in great part to a trifling over-estimate of the diameter of the latter.

We shall not dwell longer upon the work before us; but must content ourselves with earnestly recommending the study of it to all who are interested in physiological pursuits, or who are desirous of knowing how far physical agencies participate in the production of the phenomena of life. We have shown that neither the original nor the translation is absolutely faultless; but we are not acquainted with any physical philosopher on whose dicta we would more implicitly rely, than we do on those of Professor Matteucci, whenever he is stating the results of his own experimental inquiries; and we doubt whether a better editor could have been anywhere found than Dr. Pereira. The most interesting and most valuable parts of the treatise are those which Professor Matteucci has made the object of his own personal researches; and we may particularly refer to the Lectures on Imbibition, Endosmose, and Absorption, to those on the production of Phosphorescence and Electricity in living beings, and to those which relate to the Nervous force and to the influence of Electricity upon the Nervous System, as containing much important matter that is not to be found elsewhere. On the present aspect of these last subjects, we shall offer a few remarks of our own.

The striking analogies which exist between the Nervous force and the Electric current, have led, as is well known, to a very general belief in their identity, especially amongst physical philosophers; notwithstanding several differences, which have been pointed out from time to time by physiologists, in regard to the conditions under which these two agents respectively exert their power, and without sufficient consideration of the very important fact, that other agents than electricity,—e. g. heat, chemical action, and mechanical stimulation,—will produce effects through the nervous system of the same nature with those developed by electricity, and almost as powerful. We are glad, therefore, that Professor Matteucci has directed his particular attention to this question; and although he has treated it much more after the manner of a physical philosopher than as a physiologist, and has ventured upon speculations which involve (as it seems to us) some grave physiological errors, yet his experimental researches are of the highest value, and his immediate conclusions from them cannot be disputed, unless the fallacy of his experiments can be demonstrated. After searching in every mode which has been suggested as possible, for indications of an electric current in the trunks of nerves excited to functional activity, he has come to the conclusion that—"*In the present state of science, and with the means of experimenting which we at present possess, no sign of the electric current is found in the nerves* *of living animals.*" (p. 283.) We could wish, however, to see his experimental results rendered still more precise and complete than they are at present, by the application of the same tests to nerves which are *exclusively* sensory or motor, as those which he has employed for nerves of mixed endowments. For, as Dr. T. Williams has recently pointed out (Lancet, Nov. 13), it is quite possible, and not altogether improbable, that if electric currents really do exist in nerve-trunks, their power of exciting the same state in the nerve of the galvanoscopic frog laid upon them would be neutralized by the opposition of their directions in a nerve of mixed endowments. And this gentleman has devised an ingenious experiment, by which the existence of currents of some kind, moving in two opposite directions in the sensory and motor nerves respectively, appears to be demonstrated. For he found that when the nerve of the galvanoscopic frog was laid across the anterior and posterior roots of the spinal nerves of a dog or rabbit, within the theca vertebralis (that is, before they unite with each other), distinct contractions took place in the muscles of the frog's leg; which seem due to the induction of a polar condition in its nerve, through the opposite states of the two roots which it was made to connect. It would be most interesting to follow out this inquiry with reference to those nerves of the head in which the sensory and motor endowments are distinct; as, for instance, the 3d and the ophthalmic division of the 5th, the second division of the 5th and the 7th, or the glosso-pharyngeal and the 9th or motor linguæ. Should the statement of Dr. Williams be thus confirmed, even to its fullest extent, there will be still no proof whatever that the *inducing* current in the two nerves is of an *electrical* character; but the same careful testing should be applied as in other instances, to enable us to say with confidence that it *is not.* At present the evidence is decidedly *against* the identity of the nervous and electrical agencies; as will appear from the following summary of Professor Matteucci's researches on this subject:

> "Let us conclude, then, that the electric current does not naturally exist in the nerves of a living animal. The laws of its propagation require conditions which are not found fulfilled in the nervous system; the propagation of its force is interrupted by causes which could not produce a similar effect upon the electric current." (p. 286.)

But however strongly we may be convinced of the absence of *identity* between the nervous and electric forces, it is impossible to be otherwise than impressed with the extraordinary *analogy* which exists between them. To use Professor Grove's term, they are mutually *correlated;* and this in the closest degree. This correlation is incidentally noticed by Professor Matteucci, in more than one of his writings; but he does not anywhere (to our knowledge, at least) develope it as fully as it seems to us to deserve. For his attention has been fixed so exclusively upon the relation of the nervous force, as manifested in muscular motion, to electricity alone, that he has altogether overlooked the corresponding relation which it bears to those other forces to which electricity is itself correlated; and he has, in like manner, overlooked the fact, that its sensory as well as its motory manifestations may be excited by these agencies. This deficiency we shall endeavour in some measure to fill up, by briefly noticing some of the phenomena which seem to us to indicate the more extended correlations we allude to.

In the first place, then, a correlation between the nervous force and *electricity* is obviously manifested by the fact, that a current of electricity made to traverse the trunk of a *motor* nerve for a short distance so disturbs the nervous polarity in its branches, as to excite contraction in the muscles which they supply. On the other hand, if this current be passed through a *sensory* nerve, it will excite in the sensorium the peculiar sensation which the impressions normally conveyed by that nerve are adapted to produce; that is to say, not merely the ordinary sensory impressions, but those of a special kind also, according as the current is transmitted along the nerves of common sensation, or the optic, auditory, olfactive, or gustative. By proper management, we may be made conscious at one and the same time of flashes of light, of distinct sounds, of a phosphoric odour, of a peculiar taste, and of pricking sensations, all excited by the same cause—electricity; its effects being thus modified by the character of the instruments through which it operates. Thus it is shown that electricity will induce a state of nervous polarity, or, in other words, will excite the nervous force, in nerves of both kinds, sensory as well as motor. But further, the nervous force will excite electricity; the demonstration of this being given by the phenomena displayed by the electric fishes, the operation of whose electric organs has been fully proved to depend upon their connexion with the nervous centres, and to vary in intensity according to the amount of that connexion. We might also, perhaps, revert to the manifestation of electricity in the act of muscular contraction, as shown by the phenomena of induced contraction; but it would be premature yet to do so, until it has been demonstrated that the influence which operates upon the nerve of the galvanoscopic frog really *is* electrical.

It is, then, fully established that electricity has the power of exciting the nervous force; and that, conversely, the nervous force has the power of exciting electrical disturbance. Thus the correlation between these two agencies seems to be demonstrated.

But electricity is by no means the only agency by which the nervous force may be excited; for *heat* when applied to motor nerves will produce muscular contractions; and when applied to sensory nerves will occasion sensations, both common and special. Conversely, there are many phenomena which have not yet been explained on the purely chemical doctrine of calorification; and which seem to prove that heat may sometimes be directly generated by the excitement of the nervous force. Thus it has been frequently noticed that the immediate effect of the section of the spinal cord in a warm-blooded animal is to produce an elevation of temperature in the parts below; as if from the irritation of the nervous matter of the cord by the violence practised. Here, too, then, we seem to have heat producing nervous force, and nervous force producing heat; showing a relation between these two agencies of the same *kind* with that which exists between the nervous force and electricity, though less intimate in *degree*.

Precisely the same may be said of *chemical affinity;* which may easily be made to excite both motor and sensory changes in the nerve-trunks; whilst there is strong reason to believe, on the other hand, that the nervous force may produce or modify chemical actions in the parts to which it is conveyed.

We know too little at present of the influence of *light* and of *magnetism* in producing nervous force, or of the converse power of nervous force to produce these agencies, for us to assert with confidence that the same correlation exists between them; but it would seem to be indicated in regard to light, by the circumstance that the influence of this agent on the optic nerve produces sensation, which can scarcely be in any other way than by exciting the nervous force; and, on the other side, by some curious phenomena of luminosity among certain marine worms, which seem to bear a much closer analogy to the discharge of the electric organs of fish, than to these manifestations of phosphorescence in insects, medusæ, &c., that depend upon a slow combustive process. If we have rightly interpreted these phenomena, we have, as in the preceding cases, the production of the nervous force by light; and, conversely, the production of light by the nervous force.—Any similar relations that may exist between magnetism and the nervous force, have yet to be developed.

Lastly, we have to inquire into the relation between the nervous force and *motion;* which constitutes in our apprehension one of the most interesting points in the whole inquiry. That motion seems to possess the same kind of correlation to light, heat, electricity, magnetism, and chemical affinity, which these agencies bear to each other, was first pointed out, we believe, by Professor Grove, in his lectures on the 'Correlation of the Physical Forces;' and it appears to us to constitute a most interesting confirmation of his views on this subject, as well as to demonstrate the propriety of introducing the nervous force into the same category, that we find motion to possess the power of exciting the nervous force, just as it will excite heat or electricity; and that, conversely, the nervous force excites motion. Every physiologist knows that *mechanical irritation* of a nerve—as by pricking or pinching it—calls the endowments of that nerve into activity; that is, if it be a motor nerve which is thus treated, muscular contractions are excited; whilst if it be a sensory nerve, sensations are produced. Now this mechanical irritation is nothing else than a *motion* of the particles of the nerve-trunk; and thus we see that the nervous force may be excited by it in both directions. There might seem to be an exception in the case of the nerves of *special* sensation; as no signs of pain are given when their trunks are compressed or irritated. But mechanical force, or motion, applied to the sentient extremities of these nerves, in an appropriate manner, will occasion production of the several kinds of sensations to which they respectively minister. Thus pressure on the ball of the eye produces sensations of light and colours; and forcible pressure against the meatus of the ear occasions *tinnitus aurium.* It is not so easy to excite sensations of taste and smell by mechanical irritation; but yet, as Dr. Baly has pointed out,* the former may be produced by striking the surface of the tongue at its tip, or its edge near the tip, quickly but lightly with the finger; this taste, sometimes acid, sometimes saline, like the taste produced by electricity, will sometimes continue for several seconds after the application of the mechanical stimulus. Conversely, we find the nervous force producing mechanical motion, through the intermediation of the muscular structure; just as it excites electricity through the instrumentality of the electric organs of fishes.

It may be said, however, that the nervous force does not produce motion, since it merely acts as a *stimulus* upon the muscular substance, in which the motor power really exists in a dormant state. And this objection, plausible enough at first sight, might lead us still further to consider whether the *vis musculosa* is not as nearly related as the *vis nervosa* to the agencies in question. For we find that muscular contraction may be excited, not merely through the nervous system, but also by electricity, heat, chemical agents, or mechanical irritation, applied to the muscle itself. All these influences are said to *stimulate* the muscle to contraction; but are they anything else than the forces which, in a changed condition, become the contractile force of the muscle?—just as, on Professor Grove's view, impeded motion becomes heat or electricity, according as the friction takes place between similar or dissimilar substances. It may be objected that a slight mechanical irritation of a muscle shall call into operation a contractile force so much more powerful, that the former cannot be supposed to be transmuted into the other; but it must be remembered that chemical affinity is involved at the same time as a concurrent agent in producing the contraction, since every such action on the part of a muscle appears to be essentially connected with a change in its composition. Thus the chemical changes which take place in the muscle itself, concur with the stimulus which excites the contraction,—whether nervous, electrical, calorical, or mechanical,—to develope or produce mechanical motion. Conversely, we find that the *vis musculosa,* which is most obviously and commonly manifested in mechanical motion, may also produce electricity (according to Matteucci's most recent inquiries); there is much reason to think that it may also generate heat; and if the observations of Quatrefages* are to be trusted to, muscular contraction may produce light.

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* Translation of Müller's Physiology, p. 1062, note.

We might extend our speculations further, and suggest the inquiry whether the action of *stimulants* of every kind, in modifying the operations of living structures, is not due to a correlation between the stimulating force and the force (of whatever kind) which is the result of the operation. Thus *heat* is commonly said to be a stimulant to the process of nutrition; and we see its influence most remarkably manifested in accelerating the acts of vegetation. But it may be reasonably asked, whether the heat does not operate by directly producing the chemical affinities on whose action these processes immediately depend; and whether these again are not similarly correlative to the *vital* force, which the tissue, when once generated, is found to possess. We are inclined to believe that such will ultimately prove to be the case; and we offer the speculation, crude though it may at present seem to be, because we think it may serve to give a useful direction to future inquiry. There can be no doubt that the present tendency of scientific investigation is to show a much more intimate relation than has been commonly supposed to exist between *vital* and *physical* agencies; and to prove that, whilst the former are of a nature altogether peculiar, they are yet dependent upon conditions supplied by the latter. And the more closely these phenomena are investigated, the more intimate and uniform does that dependence appear; so that we seem to have the general conclusion almost forced upon us, that the *vital* forces of various kinds bear the same relation to the several physical forces of the inorganic world that they bear to each other; the great and essential modification or transformation being effected by their passage (so to speak) through the germ of the organic structure, somewhat after the same fashion that heat becomes electricity when passed through certain mixtures of metals. In offering this speculation, we desire to be understood as limiting ourselves strictly to the vital forces which are concerned in the building up and maintenance of the *material* organism; and as not in any way desiring to include in our hypothesis the entirely distinct phenomena of *mind.*

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* Annales des Sci. Nat. N. S. Zool., tom. i, p. 23.

Art. XIII.

1. *Researches into the Pathology and Treatment of the Asiatic or Algide Cholera.* By E. A. Parkes, m. d. Lond., Assistant-Physician to University College Hospital.—*London*, 1847. 8vo. pp. 250.
2. *The Cholera not to be arrested by Quarantine: a Brief Historical Sketch of the Great Epidemic of* 1817, *and its invasions of Europe in* 1831-2, *and* 1847; *with Practical Remarks on the Treatment, Preventive and Curative, of the Disease.* By Gavin Milroy, m.d., &c.—*London*, 1847. 8vo. pp. 51.

We may fairly presume that there is not one of our readers, whose attention is not fully awake to the probability—we might almost say the certainty—of the return to our shores of that dreaded visitant, whose progress has never yet been checked or its course in any considerable degree averted by precautionary measures, the impenetrability of whose nature has hitherto defied the acumen of the most distinguished scientific investigators, and whose fearful results have left upon the mind, alike of the profession and the public, the lamentable impression of the utter incapacity of the resources of the therapeutic art to cope with its destructive energies. We need not, therefore, say anything by way of preface respecting the seasonableness of the first of the publications before us; but shall at once proceed to give such an account of it as our limited space will allow,—an account which, we hope, will *not* satisfy our readers, but will induce them to have recourse to the work itself, as one richly deserving their attentive perusal.

The value of any such treatise mainly depends upon the competence of the author to observe accurately and to record clearly and faithfully, and upon the opportunities of observation which he has enjoyed. As to the first of these points there can be no doubt; Dr. Parke's academical career having afforded ample evidence of a combination of diverse faculties and talents, which is alike rare and valuable, and which leads us to feel the greatest confidence in the acuteness and industry with which he has prosecuted his researches, and in the fidelity and discrimination with which he presents the world with their results. And with respect to the second point, it will be enough to state, that the observations were made during two severe epidemics of cholera, which prevailed in India in 1843 and 1845, at which times Dr. Parkes was serving as assistant-surgeon in one of H.M. regiments in India, having received this appointment in consequence of the pre-eminent qualifications which he had manifested at his University examinations. He desires that his treatise should be considered, not as a history of cholera, but simply as an account of his individual knowledge of it; and his chief motive in its publication is to point out the path which, in his opinion, should be followed by those who seek to prosecute more minute and elaborate inquiries into the fundamental nature of the disease, than his own circumstances would permit. He looks to our present improved acquaintance with organic chemistry, and especially with the chemistry of the blood, as likely to throw much light upon its pathology.

"In order, however, that chemistry may beneficially act, a prior discussion seems to be necessary, for the purpose of indicating the proper method of inquiry. The value of the different symptoms, and their probable relations to each other, are subjects which require to be known beforehand, and the possible conditions of the blood, as connected with different sets of symptoms, should be previously determined." (p. 3.)

The leading idea which Dr. Parkes has formed of the nature of cholera is, not only that it is a primary disease of the blood, as has been very generally surmised, but that the changes induced in the function of respiration directly consequent upon the alteration of the blood, are the proper and distinctive symptoms of the disease; and he employs the term *Algide*, first introduced by the French pathologists, as pointing very happily to one of its most remarkable and constant symptoms, viz. the diminution of animal heat. This idea, however, has evidently been formed as a *result* of his pathological inquiries; instead of having been (as too frequently the case) the guiding hypothesis under which they were prosecuted, or the distorting medium through which the phenomena have been viewed. The first part of the work is occupied by an account of the post-mortem appearances seen in forty-six fatal cases of cholera occurring in males; of whom thirty-nine died in the blue or collapsed stage, while the remaining seven died of the subsequent febrile affections. This account bears the impress of great care, the different appearances being described and specified with the utmost exactness that their nature permitted, and the number of cases in which each presented itself being constantly mentioned. We can only find room for the following general summaries:

"*Head.* I conclude from this statement that the most usual appearances in the head consist in the accumulation of blood in the veins of the dura and pia mater, and in the effusion of serum or of blood consequent upon this. This congestion is sometimes as great in recent cases, as in cases of consecutive fever with head symptoms. It is considerable in the most malignant cases, but I am unable to say whether the accumulation of blood, and the rapidity or malignancy of the case, bear to each other any determinate ratio." (p. 13.)

"*Lungs.* The most common appearances in the lungs are, the presence of blood in the large vessels chiefly or solely; the collapse, and the deficient crepitation, arising from the more or less complete absence of air and blood, and from the approximation from some unknown cause of the molecules of the pulmonary substance. In other cases there is more blood in the minute structure, a corresponding dark colour of the lung, and a variable amount of frothy serum; the quantity of frothy serum bears an inverse ratio to the degree of collapse." (p. 21.)

"*Heart.* The right side of the heart and the pulmonary arteries were generally filled, and, in some cases, distended with blood; the left side and aorta were generally empty, or contained only a very small quantity of dark blood. The inference which was drawn from the state of the cavities, in the greater number of cases, was, that the right side had continued to receive blood till, in some cases, it had become full and even distended, while the left side had received little or no blood, but had continued to contract, in some cases even violently, on the last drop of blood which had entered it." (p. 24.)

"*Blood.* It appeared probable, from all the observations, that there was a deficiency in the quantity of fibrine, or a great tendency to the separation and deposition of this ingredient; that albumen and salts were present in undetermined quantity; and that the red particles were sometimes partly dissolved in the serum, but not in other cases appreciably altered in figure or appearance." (p. 34.)

These appearances were so constant, although differing in the degree of their development, as to point out unequivocally that in cholera the blood is obstructed in its passage through the lungs; which inference derives strong confirmation from the order of the symptoms, since this indicates that the loss of animal heat, embarrassment of the respiration, and gradual arrest of circulation, are produced by some aberration of, or impediment to, the proper respiratory changes. A careful analysis of the several possible causes which might be supposed to interfere with these changes, leads Dr. Parkes to the following conclusion, which we consider to be at the least highly probable:

> "As, therefore, the mechanical part of respiration is perfect, and as there is no impairment in the voluntary command of the respiratory muscles, and as the heart evidently beats in many cases till stopped by the want of blood on the left side, and by its accumulation on the right side, we are compelled to look for the cause of such arrest of the circulation in the only remaining element of respiration; namely, in the blood itself." (p. 107.)

From the importance of this part of the inquiry, we shall enter a little more minutely into the account of the changes in the circulating fluid observed by Dr. Parkes. Its defective or imperfect coagulation was a very constant feature in its condition. In little less than a quarter of the whole number of cases, the presence of fibrine in the blood was not indicated by any coagulation, either in or out of the body. In some of the remaining cases, the blood in the heart had coagulated imperfectly, whilst that of the vessels had coagulated more firmly; whilst in others, the reverse was the fact, the blood drawn from the vessels showing the coagulating power, whilst that in the heart did not. In some cases, blood from one part of the vascular system (e. g. the jugular veins) coagulated, when that drawn from another part of the same system (e. g. the pulmonary artery) did not coagulate. The clots when formed were loose and fragile in texture, sometimes almost semi-fluid, and seldom showing the power of contracting so as to expel the serum. But even where the coagulation was most imperfect, and the clot loosest in texture, the coagulation was as rapid as in the cases where the fibrine was most abundant and firmest.

> "It appeared to me, therefore, from these considerations, that the fibrine in the cases in which there was no coagulation, was not prevented from taking this form by the presence of any foreign substance, but that the complete incoagulability of the blood was only the highest degree of a diminution in the fibrine more or less obvious in the whole range of cases; in other words, judging from the nature, both of the clot after the removal of the blood, and of the fibrinous coagula in the cavities of the heart, the fibrine appeared to be lessened in quantity in most cases, and in some instances to be altogether wanting. If the defective or imperfect coagulation is not allowed to be a test or indication of an absolute diminution in quantity of the fibrine, it must at any rate be allowed to be the proof of the more or less abundant presence of some agent preventing coagulation." (p. 33.)

Whether coagulated or not, the blood had usually a dark colour; but it generally acquired an arterial tint when brought into contact with the air in thin layers. Nitrate of potash, chloride of sodium, &c., added to the blood, always gave a bright arterial hue; and it is a very interesting observation of Dr. Parkes, that a few drops of the thick substance taken from the intestines had sometimes the same effect in restoring the vivid arterial colour of the blood, as when a solution of chloride of sodium was added to it. The notion of the thickened consistence of the blood, which would be derived from the epithet "tarry," is altogether fallacious. The serum appeared frequently to contain hæmatosine; but it did not seem to have lost much of its albumen, as it always coagulated firmly on the application of heat. Dr. Parkes's opportunities, however, of making accurate quantitative analyses of the blood were not such as to enable him to speak decidedly, either on this point, or with regard to the proportion of saline matter remaining in the blood. He could detect with the microscope no alteration in the form of the red corpuscles; but they seemed to have lost their power of running together and applying themselves side by side; or if they did this, it was much more slowly than usual.

In strong contrast with these invariable phenomena, were the variable changes presented by the abdominal viscera. The *liver* was natural in the majority of cases, and the bile secreted before the attack or during its early stages was unchanged; there was usually, however, some accumulation of blood in the larger branches of the vena portæ and hepatic vein. The *spleen* was generally contracted, and contained little blood (a condition quite opposite to that which it usually presents in ordinary asphyxia; and its consistence was almost invariably firm. In this respect Dr. Parkes's experience is at variance with that of Mr. Twining and others, who described the spleen as enlarged and congested. The structure of the *kidney* was apparently unaltered. The *stomach* and *intestines* presented no well-marked changes; some turgidity of the larger vessels, ramiform congestion of the mucous membrane, slight flushing of the peritoneal surface, and enlargement of the solitary and agminated glands, constituting the sole phenomena that could be regarded as at all dependent upon the choleraic condition. Their development, however, bore no relation to the intensity of the disease; as "they were decidedly most conspicuous in the least severe, and in the prolonged cases, which were generally those attended with the greatest amount of purging;" from which it may reasonably be inferred that "they were merely a consequence of the purging, and of secondary importance." (p. 41.)

The fluid effused into the alimentary canal is one of the constant appearances in cases of death during the cold stage of cholera; and this fluid is found in its most unmixed condition in the small intestines. It consists of a thinner and a thicker portion; and it appears to be the former which chiefly constitutes the "rice-water" stools, which may be passed off very copiously, with very little admixture of the thicker substance:

> "The thicker portion was flaky, stringy, curdy, or clotted; it was not spread uniformly over the surface, but lay in masses here and there, as if it had been deposited from an agitated fluid; some of these masses or bundles often adhered with considerable firmness to the mucous membrane, and were detached from between the valvulæ conniventes. It had a peculiar albuminous or caseous smell, more developed by heat." (p. 44.)

After giving an account of the experiments made by himself, and referring to those of other observers, particularly those of Dr. O'Shaughnessy, Dr. Parkes thus continues:

> "There can be little doubt, judging from these experiments, and from the observations of other writers which I need not detail here, that the fluid in the intestinal canal and the peculiar stools consist in part of the water and salts of the blood mixed with a proteine constituent. That the greater part of the proteine constituent consists of fibrine, also appears probable; but albumen is undoubtedly sometimes present, as proved by the coagulability of the thin fluid; in other cases, if this ingredient be present, it seems to have assumed the insoluble form immediately after being poured into the canal.
>
> "Referring now to the state of the blood as already described, it is impossible

to avoid connecting these two observations together;—that whereas the blood was generally deficient in its power of coagulation, or was altogether destitute of this property, or in other cases separated the fibrine more readily than usual,—so in the intestinal canal a substance was found, which presented many of the physical and chemical qualities belonging to the ingredient which appeared to be wanting in the blood." (p. 47.)

Dr. Parkes refers to the opinion recently stated by Andral—that the effused white substance is not fibrine nor any other constituent of the blood, but merely modified mucus,—and points out the insufficient grounds on which this assertion rests, together with its want of inherent probability. He is far from denying, however, that it may undergo a change in passing from the blood-vessels into the intestinal canal; but this change seems rather a simple transformation under the influence of a chemical reagent, than the result of a true process of secretion.

The symptoms of cholera are then treated of, first separately, and then collectively, from materials supplied by accurate notes of forty-seven cases witnessed by Dr. Parkes, in which the disease was fatal in the stage of collapse. These are considered under the following heads: 1. Duration of the disease in fatal cases.—2. Passage of fluid from the mouth and anus.—3. Cramps.—4. Symptoms referable to the functions of respiration and circulation.—5. Passage of fluid from the skin and kidneys.—6. Condition of the blood drawn during life.—His observations fully bear out the position adopted by most English writers of reputation, that the purging and vomiting bear no relation whatever to the algide symptoms, or that, if any relation exist, it is *inverse* rather than direct.

> "Thus, at a period of the case when the algide symptoms were most fully developed, viz. in the last five hours, the purging ceased; in the cases where the algide symptoms were prominent throughout, and which cases were consequently the most malignant and most rapidly fatal, the passage of fluid from the intestines was oftentimes trivial in degree, and shortened in the period of its occurrence. In cases in which the vomiting and purging were excessive, the algide symptoms often came on slowly, and were less marked and deadly." (p. 80.)

With regard to the connexion between the cramps and the other symptoms we find the following observations:

> "The question is easily answered in respect of the algide symptoms, as, when these were at their point of greatest intensity, the cramps ceased, and it was also a matter of familiar observation that, in the worst and most rapid cases, there were hardly any spasms. The relation between the vomiting, purging, and cramps appeared to be more intimate. The average period of cessation was in each case nearly the same, and there was throughout the whole of the disease a general accordance in the prevalence of the two symptoms. In many cases the cramps were observed completely to ally themselves with the purging; to manifest themselves at intervals corresponding to the stools; to cease, and again to recur, as the stools ceased and recurred; and finally to disappear altogether when the purging was arrested. But it was also equally certain that the cramps never showed themselves *pari passu* with the purging from its first commencement; they appeared hours after the first stool, and often only when the algide symptoms were well marked; it was also certain that they were very severe in some cases with little purging. How are these contradictions to be reconciled? The explanation is perhaps to be found by referring to the nature of the stool: in the earlier periods of the case the stools were more watery, copious, and freer from white flocculi; in the after periods they were less copious, and white flocculi were abundantly present: it was in this after period that cramps became common. This coincidence of prevalence does not necessarily imply connexion; but an

additional argument is furnished by the fact that the greater or less prevalence of cramps did seem to bear a constant ratio to the white flocculi in the stools, and to the curdy substance found in the canal after death." (p. 92.)

Our author then adduces strong grounds for the belief that the ordinary cramps in cholera are reflex spasms, produced by the action in the intestinal canal of a fluid to which it is unaccustomed; and he thus concludes his examination:

> "If, therefore, the cramps are to be referred to the passage of fluid into the intestinal canal, we have in cholera only two sets of symptoms to consider: first, the group derived from the lesions of the circulation and respiration, and which, for the sake of distinction, has been called the algide group; and secondly, the abdominal symptoms of vomiting, purging, and the dependent spasms. These two groups are, however, in an inverse ratio to one another in point of prevalence. The one does not induce the other; they are independent effects of the common cause. But the algide symptoms are, in fact, the disease; in proportion to them is the malignity and rapidity of the case; they afford the only measure of its severity, and from them only can a correct prognosis be formed. Whence it follows, that the vomiting, purging, and cramps must be considered merely as usual but non-essential symptoms of cholera, whose absence would not in the least affect the diagnosis of the disease, and that consequently it is within the bounds of possibility or even probability, that cases of cholera may occur entirely divested of these symptoms.
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> "I need scarcely remark that several cases of this kind are upon record; and, although I have never myself witnessed these extreme instances, it is satisfactory that, from reasoning on my own fatal cases, in every one of which purging was present at some period of the case, I have come to a conclusion identical with that derived from actual observation by several of the most eminent writers on this subject." (p. 95.)

The same results present themselves when the general course and progress of the symptoms of the disease, in any severe case, are attentively analysed, as in Dr. Parkes's fifth chapter; and the doctrine which he propounds derives further confirmation from a review of the post-mortem appearances in connexion with the symptoms. Nearly all these facts are capable of being readily interpreted in accordance with the belief that the blood is the primary seat of the disease; that it becomes contaminated by the absorption of a poison introduced (probably) by the lungs; and that this contamination involves a change, which produces a great obstacle to the continued flow of the circulating current through the capillary system, both pulmonary and systemic. The most obvious change in the blood is shown in the condition of its fibrine. The red corpuscles, as already stated, do not exhibit any sensible alteration; and their hæmatosine is affected in the usual way by exposure to oxygen, or by treatment with salines. It is not difficult to understand *how* any considerable change in the condition of the fibrine should produce the obstruction in question. For it is well known that a certain viscidity of the fluid is physically required for its ready passage through the capillary tubes; and the changed state of the fibrine will impair or destroy this condition. Again, as Dr. Parkes suggests, the fibrine may be gradually deposited, as it becomes insoluble, in the capillaries or in the small arteries, and may there form a mechanical impediment to the passage of the blood. But further, on the doctrine—which seems to us one of the established truths of physiology (although we are well aware that there are many distinguished physiologists who do not regard it in that light)—that the reaction between the blood and the tissues, during its passage through the capillary system, creates one of the forces essentially concerned in the maintenance of the circulation, we at once perceive why, in such an altered condition of the blood, the entire current of the circulation should be stagnated or entirely arrested, even while the heart may have lost but little of its propulsive power. The almost complete cessation of the respiratory changes, which should have occurred in the pulmonary capillaries, and of the operations of nutrition and secretion to which the systemic circulation is subservient, follows as a matter of course.—We must conclude our extracts with the following classification of the three principal varieties of cholera, which will be seen to coincide extremely well with the notion of different degrees of the presumed change in the blood.

> "1. Thus, if the final change at once occur, and there is a complete and rapid arrest of the circulation, either from the intensity of the cause or from constitutional predisposition, the worst variety is produced, in which 'a mortal coldness comes on from the beginning.' As the circulation is soon almost entirely arrested by physical alterations in the blood—presumably, changes in the fibrine—there can be little purging and comparatively little sweating; there is always some effusion of the thick white substance into the intestines, but often little of the watery part of the blood. The symptoms might be inferred from a statement of this condition: we might have presupposed a very rapid loss of animal heat, loss of voice, deafness, and vertigo, total arrest of all secretions, defective aeration of the blood, consequent dark colour of the surface, and early and deep coma. . . . . . .
>
> "2. If the cause act with less intensity, we have the second variety, in which there is less physical alteration in the fibrine, and the circulation is carried on for a longer time. Consequently, the characteristic change is not evidenced solely or chiefly in the interior of the vessels; but is partly transferred to the exterior of the vascular system. The proteine constituents, fibrine and perhaps albumen, are effused in large quantities, and in all parts of the body, though chiefly on the free surfaces of the skin, alimentary mucous membrane, and more rarely the bronchial mucous membrane. This effusion, and its general nature, form two characteristic distinctions between cholera and diarrhœa; for diarrhœa is a disease confined, in the first instance, to the eliminating part,—viz. the large or small intestines, as the case may be, and is unattended, as a general rule, by the effusion of albumen and fibrine. The worst forms of this variety are seen in those cases in which, after two or three choleraic stools, severe and long continued cramps come on, accompanied and followed by intense algide symptoms; after death the small intestines are generally found distended with the thick, white, flaky substance. Other cases of this variety present infinite modifications in severity, according as watery elimination is added to effusion of the fibrine; in other words, according as they tend towards the slighter forms.
>
> "3. The slighter forms commence with much watery purging and vomiting, and pass into the first and second varieties in varying times. There may be from ten to fifty copious watery stools, and frequent copious vomiting, before there is any great loss of heat and failure of circulation. But there is always some degree of this even in the slightest cases, else the case would be mere watery diarrhœa, attended only by exhaustion, and not by the symptoms peculiar to cholera. Cramps are seldom present till the stools put on the true choleraic character, viz. of copious white flocculi suspended in a watery fluid. The algide symptoms come on gradually, and are less intense than in the former cases; recovery is also more common." (pp. 114–16.)

Sometimes the transition from the last to the first of these conditions takes place in a wonderfully short period; a patient attacked with loose, watery purging, without a single other indication of cholera, and therefore not apparently in danger, becoming suddenly affected with the algide symptoms; and the case thus passing in a few minutes from a trifling to a most dangerous disease. Hence it is of great consequence, during the prevalence of an epidemic of cholera, to apply remedies early in such disordered states; and there seems little doubt that, however powerless any treatment may be when the algide symptoms have once fully developed themselves, medicines that are calculated to check the diarrhœa, when this occurs in the first instance, may have the effect of retarding, even if they do not altogether check, the progress of that change in the blood on which all the severer symptoms appear to depend, and thus to give more time for those natural curative processes to take place, whose agency we must admit whenever recovery takes place from the algide condition, since none of our remedies can be shown to exert any influence when that condition has been fully developed.

With regard to the fever and other affections consecutive upon cholera, Dr. Parkes makes the following preliminary observations:

> "I have already intimated that I do not adopt the opinions of those who regard the blue stage as only an initiatory period, and compare it to the cold stage of ague; I consider the cold stage as the real disease, which, at its termination, is succeeded by various forms of reaction according to the severity of the case, the kind of epidemic, the constitution of the individual, or even, it may be, according to the treatment employed. Therefore it might be inferred that consecutive fever would be sometimes absent and sometimes present. And this is really borne out by the experience of Indian practitioners, although it is not strictly in accordance with observations made in Europe, where consecutive fever was more generally noticed. My conclusions, however, so far differ from those of Annesley and other Indian writers, that I believe some form of reaction to be much more common after cholera, than they have allowed. Consecutive fever does not follow pseudo-cholera, or even very slight cases of true cholera, or even well-marked cases of moderate severity, such as those formerly detailed; but most certainly very severe cases seldom or never attain convalescence, without passing through some transition stage. This agrees also with observations of Jameson and Scott." (p. 131.)

We must pass over the chapter devoted to this subject; and with regard to that which treats of the *Diffusion of the Choleraic poison*, we can only stop to notice that Dr. Parkes has never observed any indication of contagion in cholera, all the phenomena of propagation and development of the virus which have fallen under his own observation being sufficiently accounted for, without calling in the aid of the hypothesis that the virus can multiply itself by its action on the living human system. He wisely abstains, however, from generalizing upon his individual experience, and does not hence dogmatically conclude that the poison of cholera is *never* reproduced by the human body.

Dr. Parkes's chapter on the *Treatment of Cholera* is marked by the same philosophical discrimination and entire freedom from prejudice in favour of any particular system, as prevails throughout the remainder of the treatise; and we regret that our limits will not permit us to make any extracts from it. The size and price of the book are such, however, as to bring it within easy reach of all who desire to consult it; and we cannot recommend a better guide in that practical investigation of the disease to which we shall probably be ere long called upon to return, than he has thus seasonably placed before us. Earnestly do we hope that his sanguine anticipations of the success of well-directed endeavours may be realized; "that some great discovery will speedily reward the efforts of the pathologist; and that a more certain knowledge of the morbid changes in the blood will indicate to us the antidote to this poison at present so terrible and resistless."

The purpose of Dr. Milroy's pamphlet is fully set forth in its title; and we regard this also as a very seasonable publication, since it will aid, we hope, in settling the minds of the profession and the public as to the mode in which the disease is ordinarily propagated, and the means which should be taken for its prevention. The author first adverts to the evidence which exists for the belief that the cholera did not originate in India, as supposed by some, in the year 1817; but that it was known to medical practitioners of that country during the last century, not merely in its *sporadic* form, but also *endemically*, and, occasionally, *epidemically*. In the year 1817, however, the disease "acquired a greater force of diffusive energy, and a more abiding perpetuity of existence, than it had previously exhibited." The outbreak of this fearful epidemic was preceded by a season of excessive rains and great vicissitudes of weather, which seems to have produced an uncommon amount of sickness, and an unusual mortality in many parts of the Indian peninsula. Although attempts have been made to show that the pestilence originated in one spot, and thence radiated to the different towns and stations in India at which it presented itself, yet there is convincing evidence of its independence and contemporaneous appearance in situations remote from each other, between which there was no communication. This epidemic gradually extended itself through southern Asia, and then spread in a more northerly and westerly direction; experiencing, however, a remarkable lull in its diffusive power, which led to the belief, that as it was of Asiatic origin, it would be almost confined to the country that gave it birth. After its passage from the Georgian and Circassian frontiers into Russia, however, its progress continued steady, as most of our readers will remember, in spite of all natural and artificial obstacles, until it had traversed almost every habitable region on the surface of the globe.

The rise and early progress of the present epidemic have not yet been accurately determined. The following descriptions of two of its most violent outbreaks are full of interest, not merely on account of the immediate inferences to be drawn from them with respect to the sources of the pestilence, but also as confirming, in the strongest manner, the views put forth by Dr. Parkes as to the essential nature of the disease. It is evident that, in the fearful scenes here narrated, the pestilential poison was operating in its severest form.

"After having been quiescent during the winter of 1845–6, it broke out with extreme severity in the following May at Teheran, carrying off as many as 300 a day for several weeks, and reducing the population of that town by at least 20,000 souls. The description given of the cases shows the extreme malignity of the epidemic:—'Those who were attacked, dropped suddenly down in a state of lethargy, and, at the end of two or three hours, expired without any convulsions or vomiting, but from a complete stagnation of the blood, to which no remedies could restore its circulation.'

"Now it is a fact full of interest to the medical inquirer that, at the very time when this work of devastation was going on in the north of Persia there took place at Kurrachee, near the mouth of the Indus, that terrific outburst of the pestilence which, in the course of a few days, swept off upwards of 8000 victims.

The description that has been given by an eye-witness of the scene, is so full of fearful and instructive interest, as regards some of the most striking characters of pestilential visitations, that we cannot withhold a brief account of its leading particulars. The heat had been intense during the first fortnight in June, but the station remained tolerably healthy. On the 14th, a Sunday, the atmosphere was more than usually stagnant and oppressive; one correspondent, who was present, says; 'the very heavens seemed drawn down upon our shoulders; the feeling was suffocating.' A dark portentous-looking cloud crept up the sky as the troops were proceeding to church, and a sudden burst of wind threatened the buildings. It passed away almost as speedily as it came, and when the worshippers retired, the air was as still as when they assembled. At the same hour did the pestilence appear. Before midnight, nine soldiers of the 86th regiment were dead; and men began to be brought into hospital in such numbers that it was difficult to make arrangements for their reception. It was a fearful night. With morning, came the tidings that the pestilence was overspreading the town, and that fifty persons had already fallen victims to its deadly poison. How awful must have been the rapidity of the attack, when we learn that sometimes, within little more than five minutes, hale and hearty men were seized, cramped, collapsed, and dead! The only thing we can compare it to is the deadly effect of a serpent's venom. Men, attending the burials of their comrades, were attacked, carried to the hospital, and themselves buried the next morning. Pits were dug in the churchyard, morning and evening; sewn up in their beddings and coffinless, the dead were laid side by side, one service read over all! For the next five days, it raged with appalling fury; it then abated in its intensity, but continued to hover around the place for about another week. Within less than a fortnight, 900 Europeans, including 815 fighting men, were swept away. Besides these, 600 native soldiers, and 7000 of the camp followers and inhabitants of the town had been hurried into eternity! What must have been the scene of desolation and the sickening pollution of the air after such a visitation, when nearly 9000 bodies were festering under the ground beneath a tropical sun!

"Altogether, this comparatively insulated eruption at Kurrachee, while the head-quarters, so to speak, of the pestilence were in the north of Persia, presents an instance very analogous to that of the equally dreadful invasion of the disease in the camp of the Marquis of Hastings, in Nov. 1817, not long after the first appearance of the great epidemic in the delta of the Ganges. The same idea is naturally suggested by both, viz. that the cause of the malady was something altogether independent of infectious communication, and must have existed in the atmosphere." (pp. 16-17.)

The epidemic, however, has not always presented itself in this highly malignant form; and it has sometimes exhibited a considerable diversity of character. In many places, the vomitings and cramps have been the principal symptoms; and occasionally the disease has appeared under the somewhat unusual type of a bloody flux, its choleraic character being marked, however, by the presence of other characteristic symptoms, and by the prevalence of the disease at the same time and in the same place under its ordinary form.

Dr. Milroy then proceeds to point out the strong analogies between the mode of diffusion of cholera and that of influenza—a disorder to which no sensible observer attributes the possession of an infectious character, at least in a degree sufficient to render the transmission from one individual to another the usual or chief means of its propagation; and he adverts to the curious circumstance, remarked upon by Dr. Hancock and Dr. Hecker, of the striking concomitance of these two diseases in later times, and of the concomitance of the older epidemics of influenza with epidemic fevers, dysenteries, &c., showing that they all depend upon modifications of a common cause. He then, in a few brief but judicious and well expressed remarks, places the question of its propagation or non-propagation by infection upon its right practical footing. It is quite possible that a malady not originally and necessarily infectious *may* become so under certain unsalutary conditions; so that a limited propagation may take place by personal intercourse, under the influence and during the continuance of the epidemic constitution. And we believe, with Dr. Milroy, that all the facts which have been urged by the stoutest upholders of the infection-theory, are explicable upon this admission. But, on the other hand, that the propagation of the cholera, like that of influenza, depends *mainly* upon atmospheric malaria,—that infection cannot transmit it in the absence of the malaria,—and that, even when the epidemic condition is fully established, the propagation by infection bears such an inconceivably small proportion to the propagation of the disease by other agencies, as to render it practically almost a nullity, —are truths, which, we feel convinced, are no less historically and scientifically true, than they are practically important. Dr. Milroy strengthens his position, by adverting to the precursory *choleroid* or *cholerine* attacks, which frequently prevailed for some time before the real disease manifested itself, and which indicated the operation of the same causes in a less intense degree. These could not be in any way ascribed to contagion. Again, the sudden seizure of hundreds of persons at the same time and in the same place is utterly incompatible with the notion of radiation from a focus of contagion; as is also the well-known fact, that in many instances the distance of a few hundred yards had made all the difference between a region of inevitable death and one of complete exemption and even of health, notwithstanding that uninterrupted communication existed all the while between the two.

The negative evidence, extracted from the fact that in certain localities around which a strict *cordon sanitaire* was drawn, the cholera never made its appearance, is of little value; since these localities were not more extensive than many others which escaped without any such artificial isolation, and were in general such as presented no attractive soil (so to speak) for the growth and propagation of the malaria. Our attention has been called by a highly respectable practitioner in Newport, Isle of Wight, to the following facts, which constitute the entire history of the former epidemic of cholera in that island:

"1. In the month of July, 1832, A. B., an old man living in the outskirts of this town, who had formerly been in the navy, and spent several years of his life in tropical climates, and who, according to the statement of his neighbours, was half starved by his wife, was attacked early one morning, after a few days' slight indisposition, with severe vomiting and purging, and subsequently cramp in the legs; he died about five hours after the commencement of the vomiting and purging. This was a perfectly isolated case; the man had never left his own neighbourhood for a long time previous to the attack; and there was no report even in the town of its having been preceded by any similar case, although we were all on the *qui vive* at the time on the subject of cholera, which was then raging in many parts of the kingdom. I believe there had not been any case of the kind in any part of the island; nor did any occur subsequently, I believe, until the following summer, when the case next detailed arose. A very offensive open drain ran just in front of this man's dwelling; which had become partially stopped up by an accumulation of filth. The drain was cleared out, and other measures of cleanliness adopted, in spite of the discontent of the immediate neighbours.

"2. August 29, 1833. B. G., an Irish tramper, came into the island from Portsmouth, where she had been living for some days, and where cases of cholera were continually occurring. She proceeded directly to this town on foot, above seven miles from the spot where she landed. She arrived here apparently in good health, and procured a lodging in a small court situated in a poor and dirty quarter. At one o'clock the next morning she was suddenly attacked with vomiting and purging of the characteristic rice-gruel fluid. In the afternoon she was removed to a detached and secluded building, a mile distant, on an elevated spot, which was habitually devoted to the reception of patients afflicted with infectious disease. This woman died twenty-three hours from the commencement of the attack.

"3. August 31. J. B. was seized early in the morning with vomiting and purging. This was a healthy-looking Scotch woman; she lodged in the same house with the last patient, attended upon her in her illness, and accompanied her on her removal to the hospital in a close covered vehicle. She had been perfectly well previous to the illness of her companion, but had vomited once or twice soon after parting with her at the hospital. She was removed without delay to the hospital, and so also were the other lodgers in the house, viz. an old man and woman, and four children. Everything was taken out of the house; all articles of clothing were burned; and the house itself was thoroughly cleansed and whitewashed. This patient had the characteristic discharge from the stomach and bowels; she ultimately recovered.

"4. September 4. T. G., the old man who was removed from the house where these cases commenced, complained to me of vomiting, purging, &c. It was found, upon close inquiry, that he had had pain in the stomach on the 31st August, and that the purging commenced on the 2d September, although I had questioned him daily respecting his health, and he had denied having anything the matter. The characteristic symptoms supervened, and he died on the 8th.

"5. Many of the persons who lived in the court where these occurrences commenced were attacked on the morning of the 31st of August with purging, vomiting, and pain in the bowels, particularly one man, who assisted in putting the woman B. G. into the carriage; and one of the nurses at the hospital, who attended upon her, suffered in the same way on September 1st. All these took a dose of aperient medicine without loss of time, and experienced no further illness. And here, fortunately, terminates the history of the cholera in the Isle of Wight. I am not aware that there has been any other case similar to these up to the present time."

This history presents many points of great interest. The *first* case was probably not genuine cholera; but one of that class which usually precedes the invasion of the severer disease. Its connexion with the open drain can scarcely be doubted; and the prudence of the measures taken in consequence is worthy of all imitation. The *second* case was obviously one of genuine cholera, imported from Portsmouth; and the *limited spread* of the disease from this centre, as shown in the *third*, *fourth*, and *fifth* cases, is certainly one of the strongest evidences in favour of the infection-theory which has fallen under our notice. But let it be observed how much the *general condition* of the district had to do with the peculiar circumstances just detailed. The epidemic constitution was fully established at Portsmouth; and would probably have developed itself in the Isle of Wight, but for the generally favorable conditions presented by its population and their residences. At that period—we speak from personal knowledge—there was a very moderate population, scarcely any overcrowding in the towns, steady employment of a healthful nature for the labouring population, and, as a consequence, little or no abject poverty. Moreover, from the statement quoted respecting the open drain, we presume that efficient sanitary measures had been adopted. Hence we might describe the seeds of the disease as suspended over the district, but as not finding in it a soil sufficiently congenial for their development. The importation of an actual case of cholera, however, supplied a more productive stock of the poison; which was concentrated in that one spot, be it observed, most favorable for its development,—namely, a dirty court. The persons most severely attacked were those who were in closest contact with the original sufferer; but most of the inhabitants of the court were in some degree affected. The removal of those principally endangered to a more open atmosphere, and their seclusion from other intercourse, seemed to have killed the growing malaria; and the epidemic influence never again became strong enough to regenerate the disease by itself. We trust that this island may enjoy a similar immunity, if the scourge should again devastate our country.

A general review of the whole case, then, leads us to this conclusion; that where the epidemic influence is strongly developed, infection is not likely to have any perceptible influence in propagating it; that the general march of the disease cannot be dependent upon human communication, and that quarantine regulations and similar restrictions upon intercourse are therefore utterly incapable of checking its progress; and that, *if* human communication be in any case the immediate agency in its transmission, it can *only* be so when a strong predisposition has been occasioned by epidemic influence, the developement of that epidemic influence being chiefly dependent upon those health-destroying conditions, which it is the object of the sanitary reformer to remove. And thus, to use the emphatic language of Mr. Farr, in the last quarterly Report of the Registrar-General—quoted by Dr. Milroy as the motto to his pamphlet,—"Internal sanitary arrangements, and not quarantine or sanitary lines, are the safeguards of nations" against epidemic diseases. "As surely," says Dr. Milroy, "has cholera always sought out and settled down upon the abodes of misery and filth in every city of Europe that has been visited by it, as the vulture-crows in the East ever congregate where the most offal and garbage are to be found."

We are very glad to perceive, from the 'First Report of the Metropolitan Sanitary Commission,' which has just been published, that the views thus advocated by Dr. Milroy, and in which we have expressed our hearty concurrence, have been adopted by them, and made the basis of their recommendations. We may not, perhaps, *theoretically* adopt the non-infection doctrine in the extreme form in which it is propounded in this Report; but we are fully satisfied that *practically* infection has little or nothing to do in the propagation of the disease (except in such curious cases as that just quoted, where it seems to decide the balance between the "to be or not to be"), and that efficient sanitary measures, therefore, constitute all that can be done with any utility in the way of prevention.

Dr. Milroy concludes his pamphlet with some judicious observations on the treatment; but as on this point we think that Dr. Parkes's remarks are more worthy of consideration, we shall only say that he attaches much value in the early stage to saline emetics; and that he lays much stress on the admirable precept of Hippocrates, not to do our patients any harm, if we cannot do them any good. It is evident that, in common with Dr. Parkes, he attributes many of the unfavorable *sequelæ* of cholera to the treatment employed, rather than to the disease itself.

PART SECOND.

Bibliographical Notices.

Art. I.—1. *A Guide to the Examination of the Urine in Health and Disease, for the Use of Students.* By **Alfred Markwick**, Surgeon to the Western German Dispensary, &c.—*London*, 1847. 8vo., pp. 155.

2. *A Table of Urinary Deposits, with their Tests, for Clinical Examination.* By **Ray Charles Golding**, M.D.

Mr. Markwick has attempted to supply a want which has been long recognised amongst students and junior practitioners. We regret that, after a careful perusal of his work, we must express our decided conviction, that he has failed in his proposed object. His book bears internal evidence that he is practically unacquainted with his subject, and that in many instances he has not even repeated the process which he describes. As the subject is one that is now attracting much attention, and as this circumstance may very possibly obtain for Mr. Markwick's volume a very considerable number of readers, we deem it advisable to notice a few of what we regard as the defects in its execution.

In a work intended for students, one or two of the simplest and most approved processes should be given, and all either involving difficulty of manipulation, or deemed questionable as regards their accuracy, should be omitted. It is better to have a thorough knowledge of one process, than a faint glimmering perception of a dozen. Let us see what are Mr. Markwick's claims on this point. We will take the test for bile and sugar. Six processes are given for the detection of bile. The fourth (why was the essential principle put there?) is for bilin or choleic acid; the first three and last two are for the bile-pigment. The third and fourth of these might have been well omitted.

For sugar we have eleven tests; and then a notice of the formulæ for calculating the amount of sugar by means of Biot's polarising apparatus, although we have been previously informed (see page 20) that "the obstacles and difficulties attending this method, are such as will in all probability prevent its coming into general use; and moreover, Dr. Leeson has proved that it is by no means to be depended on."

These are, in our opinion, mere errors of judgment. In page 38, we have a worse error—an error in chemistry. In speaking of the origin of urea, we are told that "Dumas states it [urea] to be 'manifestly derived from the oxidation of the azotised materials of the blood,' which, he says, 'have a tendency to pass into organic acid and *oxide of ammonia*.'" !! This is the first time *we* ever heard of such a compound.

We have been struck with several errors of carelessness, two of which we feel it our duty to notice, as, if allowed to pass uncorrected, they might give rise to serious confusion in the minds of Mr. Markwick's readers.

In reference to the tables that have been compiled with the view of calculating the amount of solid matter in a given quantity of urine from its specific gravity, we are told that they "are all based on the now *well-known fact* that the proportion of the solid contents of one thousand parts of urine is equal to the excess of its density over that of water multiplied by a given number." We doubted the accuracy of this statement as we read it, and were gratified to find that Mr. Markwick soon began to join us in these doubts; and in page 75, actually proceeded to show that "tables thus constructed can never do more than give an approximation to truth." Alas, for the *well-known fact!*

In page 53 there is a similar instance of carelessness. In his remarks on the conversion of benzoic into hippuric acid within the animal organism, we are told that Liebig accounts for the transformation by supposing two atoms of benzoic acid to act on the *elements* of one atom of lactate of urea, and produce two atoms of the acid in question. The student, happy in the conviction that he has now obtained a clue to the clear understanding of a most interesting metamorphosis, and rejoicing in the idea that animal chemistry is indeed illuminating the most hidden regions of physiology, stands aghast at learning *in the very next line* that "the presence of lactic acid and its compounds in the urine has been disproved by Liebig." Disproved by that very chemist who, in the preceding sentence, assumed its presence as a matter of course! That Liebig *did* give the above explanation, is very true; and it is equally true that he *does* not believe in the presence of lactic acid in the urine; but Mr. Markwick is surely not justified in giving to students such a strange and contradictory medley of Liebig's past and present views, as he appears to have done in this instance. As we are on the subject of lactic acid, we may notice that in page 28, Pelouze and certain other chemists are made to *doubt* the existence of lactate of urea in urine. Pelouze has done more; he has disproved the existence of such a salt. Again we are told (p. 55) that Liebig has discovered a particular crystallizable substance which he believes to have been mistaken for lactic acid and its compounds, but we are not told that it is the "peculiar azotised principle" which is described in page 55, and we strongly suspect that Mr. Markwick is himself not aware of their identity; neither are we informed that it has been proved by Liebig (Researches on the Chemistry of Food, p. 62) that it consists of creatine and creatinine.

We deeply regret that the duty imposed upon us, as conscientious reviewers, has compelled us to notice these defects in Mr. Markwick's book; and we regret it the more, because we believe it was published with a worthy object; to be, as it professes, a guide to students. We trust that the author will accept our assurance that we could not have acted otherwise than we have done; and that if his volume reach a second edition he will correct the faults we have noticed.

We cannot leave him without another warning; and that is, to get some literary friend to revise the sheets of any future edition. *Emunctaries*, p. 4; *analagous*, pp. 10, 56, 102, &c.; *crystalization*, p. 26; *izomeric*, p. 38; *elimentating*, p. 38; *hydrocianic*, p. 42; *fomation*, p. 43; *anæmia*, p. 46; *resinuous*, p. 58; *oscilate*, p. 61; *scarletina*, table opposite p. 64; *alcholic*, twice in p. 79; *mal- assimmilation*, p. 111; *accidulated*, p. 108; *hæmorrhoidal*, 110; *spermatozoæ*, p. 115; *kiestein*, p. 124; *ebulition*, p. 132; *tentaculæ*, p. 138, &c., strike the eye of the critical reader somewhat unpleasantly, and are errors which might have been avoided by a very little care. We may add, that *phospha-tes*, p. 23, must not be made a word of three syllables to suit the convenience of any compositor, and that *urox-anthin* and *micros-cope* might be more orthographically divided.

A plate, illustrating the forms of the various crystals, globules, &c., would add very considerably to the value of the work; in fact, it is indispensable for the clear understanding of even the best description of microscopic objects.

Dr. Ray Golding's Chart is extremely accurate in its chemical details, and will be found very useful to those who do not use the microscope. But, in these days, who does not?

ART. II.—*Principles of Physics and Meteorology.* By J. MUELLER, Professor of Physics at the University of Freiburg. Illustrated with 530 Engravings on Wood, and Two coloured Plates.—*London*, 1847. 8vo, pp. 574.

THIS very handsome volume forms one of a series of similarly-illustrated standard scientific works, which it is the intention of the spirited publisher (M. Baillière) to issue at regular intervals; and of which Dr. Prichard's 'Natural History of Man,' and Mr. Waterhouse's 'Natural History of Mammalia' (the latter at present in course of publication) formed a worthy commencement. The present volume commences that portion of the series devoted to the physical sciences; it has been already followed by a volume on the 'Mechanical Principles of Machinery and Engineering,' translated from the German of Professor Wiesbach; and we are promised at an early period a new edition of Professor Graham's 'Chemistry,' and a 'Practical Treatise on the Use of the Microscope,' by Mr. John Quekett, than whom, we may say with certainty, no more competent writer on the subject could have been found.

We believe that we may safely assert that no treatise on Physics has ever made its appearance in this country more splendidly "got up," so far especially as regards its illustrations, which are engraved and printed in the very first style of art, and bestowed with a liberality that almost amounts to profusion. And the general plan of the work is extremely well calculated to afford a comprehensive knowledge of the principles and most important facts of the various departments of physical science, to those who do not desire to enter deeply into any one branch, or who wish to obtain such knowledge as a preliminary step to a more exclusive study of some particular division. But we must take exception to some of the details, the execution of which is by no means such as to give to the work the value which its general aspect promises. In the first place, the weights and measures are nowhere brought to English standards; and the comparative table supplied, to enable the reader to make his own calculations, is so incomplete as to be almost unserviceable. Secondly, the translator, in attempting to give a literal rendering of the original, has in a great number of instances really perverted the author's meaning, by employing words and phrases which are not in our language the real equivalents of the German terms; thus completely proving his ignorance of the technicalities of many of the subjects on which his pen has been employed, and justifying the principle that a translator should be as well acquainted with these as the original writer. Thirdly, the work is in many parts decidedly behind the present state of knowledge; and we particularly notice the omission of philosophical advances and practical improvements which have been made in this country. Thus not a word is said of Faraday's recent most important discovery of the Universality of the Magnetic Influence, and of its Relations to Light; nor do we find any notice of Smee's Voltaic Battery, which, for the greater number of purposes, has superseded every other in this country. There is not a word on the mutual relations of the Imponderables; Professor Matteucci's researches on Animal Electricity are not anywhere referred to; and the whole subject of the Physics of Living Beings is most summarily and imperfectly discussed. Of the somewhat gaudy frontispiece, no explanation whatever is given in the text, save that it represents different ring-systems of polarised light; what these severally are, we are not told; and the plate, with its various references, is therefore altogether useless except as an attraction to the intending purchaser.

We cannot but think that the spirited publisher would have done much better, if he had caused an original treatise on the subject to have been prepared by some competent English writer (and of such there is no lack); or if, at any rate, he had secured the services of a translator qualified to present a good English version of the original, to supply its deficiencies, and thus render it, what it might easily be made, a standard text-book for the student of the physical sciences.

ART. III.—*An Essay on the Use and Abuse of Restraint in the Management of the Insane, including some remarks on the Origin and Nature of their Diseases. With copious Notes.* By HAMILTON LABATT, A.B., T.C.D., Fellow of the Royal College of Surgeons, Ireland; Demonstrator of Anatomy at that College, &c.—*Dublin*, 1847. 8vo, pp. 76.

WE learn from the preface to this essay, that it was written in competition for a prize offered by Sir Edward Sugden, whilst Lord Chancellor of Ireland, and that the author considers that he has reason to complain of certain proceedings relative to the adjudication of the prize by the Council of the Irish College of Surgeons. Into the merits of this question we are not disposed to enter; and, with regard to those of the essay itself, it will be enough to state that it is a very respectable performance, not distinguished by any remarkable qualities, but showing a very fair acquaintance with the subject, and a proper discrimination with regard to the true value of the non-restraint system. The accounts which he gives of the success which has attended the adoption of that system, in a large proportion of the public lunatic asylums of Ireland, are very encouraging, and bear out the statement of Dr. Conolly that, on the whole, the Irish asylums are better managed than those of England. There are, however, some terrible exceptions, which repeat the worst features of the management of our County Asylums some thirty or forty years since; and we trust that these will not long be permitted to disgrace the management under which they occur.

ART. IV.—1. *Elements of Chemistry, including the Actual State and Prevalent Doctrines of the Science.* By the late EDWARD TURNER, M.D., F.R.S., L.S.E. Eighth edition. Edited by Baron LIEBIG, Professor of Chemistry in the University of Giessen, and WILLIAM GREGORY, M.D., F.R.S.E., Professor of Chemistry in the University of Edinburgh. Part I.—*Inorganic Chemistry.* Part II.—*Organic Chemistry.*—*London*, 1847. pp. 1394.

2. *A Manual of Elementary Chemistry, Theoretical and Practical.* By GEORGE FOWNES, F.R.S., Professor of Practical Chemistry in University College, London. Second edition.—*London.* pp. 596.

WE at present notice these works, chiefly to draw the attention of our readers to the recent completion of a new and improved edition of one of the best of our standard works on Chemistry, and to the appearance of a new edition of a smaller treatise,—one of Mr. Churchill's beautiful series of Manuals,—which is the production of a gentleman who seems likely ere long to attain a position of great eminence in the scientific world, unless (as we fear may be the case) his career should be interrupted, like that of the lamented Turner, by the premature decay of his health. His merits have been recently most fully recognised, by the award of one of the medals at the disposal of the Royal Society, for original contributions to the science; and we trust that he may return from his sojourn in a warmer clime, with powers equal to the demand which his position and his reputation will make upon his exertions.

We had prepared a general survey of the present state of Chemical Philosophy as set forth in the works before us; but we are compelled to postpone this until our next Number, and must content ourselves with briefly expressing our opinions as to the degree in which they respectively meet the wants of the medical student.

We have always looked upon Turner's as the most methodical of our works on chemistry, and as possessing this peculiar merit, that it brings together the facts and conclusions of its various subjects into the form of general expressions. We may refer, for example, to those relating to the transmission of heat (p. 15); to specific heat (p. 33); to evaporation (p. 45); to induction (p. 77); to the laws of electrical accumulation (p. 83); to those of the action of voltaic circles (p. 97); to the facts connected with electro-chemical decomposition (p. 112); the laws of combination (p. 163); &c. &c. This we conceive to be a particularly advantageous mode of treating such subjects, and of the greatest assistance to the medical student. We know that during the lifetime of its lamented author, no pains were spared to make the work the clear and brief expositor of modern chemistry; and though, like us, it has lost much by his death, yet may we say also, that it has gained much, by passing into the hands of its present editors, than whom none could be found more fitted for such a work, or more capable of keeping it up with the rapid advances which chemistry is now making.

In regard to the more modest-looking Manual of Mr. Fownes, we may regard the following as its most peculiar merits. It gives a good and brief description of the properties of bodies, and it is careful also to present the reader with a clear exposition of the various working processes; and on this account it will be found a valuable guide to the student in the laboratory. But it is, in our estimation, not so well adapted as a text-book for the medical student. The section devoted to the principles of the science, the Philosophy of Chemistry,—a section, in fact, which might be made of the utmost service to him,—is neither clearly nor correctly written, everything being introduced abruptly, and without sufficient regard to method or arrangement.

We should like to see an improvement, a great improvement, effected in these 'Manuals;' more attention paid to the wants of their readers; and not such an attempt to grasp and embody the whole of their several subjects, theoretical and practical, within the limits of one small volume.

We can commiserate the condition of the medical student who is seeking to acquire an adequate but not superfluous knowledge of Chemical science; for there is really no treatise to which we can point as *his* text-book or guide. All the works upon chemistry are so overcrowded with details, and so brief and superficial in the exposition of general principles, that he cannot make them of much service to him. Look at the several Manuals and Elements of Chemistry which have been published during the last six years; we can point to six of them whose pages amount to the frightful sum of 6486; and yet there are not 100 of those pages which can be read with interest or profit by him. Let us consider the labour and discretion he must use in seeking out this 100; and can we wonder that chemistry is his horror? Again, we may remark, as we have often done before, that the position of chemistry is much too high in the present curriculum of medical studies; the boards of examiners requiring, in our estimation, a much too special acquaintance with the whole range of the subject. We think this might be bettered, and we trust that it will be considered in the plan of reform which is now being sketched out. It would be enough for the medical man to be well acquainted with the principles and the philosophy of the science. This, in fact, should constitute a portion of his preliminary education; it should be a subject of matriculation examination, and might be taught in the medical schools during the summer session, so as to leave the winter free for more important duties. After this, the special bearings of chemistry upon toxicology, physiology, and materia medica might be made evident, and be more fully discussed in the treatises and lectures upon these several subjects.

While we are saying all this, however, we are not forgetful that the science of chemistry might suffer much, if the system and place of teaching it were altered too far. We know that the school of chemistry is in this country the medical school, that the members of the medical profession are almost the only disciples of the science, and that they are its pillars and support; but still we think that some alteration might be made, which would be advantageous upon both sides; for we cannot be blind to the fact that at present a too great demand is made upon the time and attention of the medical student, and that any divergence from the main object of his education is fraught not only with pain to him, but with danger to the public. The applications of chemistry to the science and practice of medicine, notwithstanding their great extension of late, still lie within a compass which is wonderfully small when compared with the entire range of chemistry as a science; and they may be made by any one who is conversant with its general principles, and who takes sufficient pains to trace their bearing upon the phenomena which are the particular object of his attention. We are utterly at a loss, therefore, to see the necessity of cramming the medical student with details, which have scarcely the remotest chance of ever becoming practically useful to him in any way whatever; and shall esteem it a great improvement in our system of education, when it is better adapted to his *real* wants in this respect.

ART. V.—*An Experimental Inquiry into the Cause of the Ascent and Descent of the Sap, with some observations upon the Nutrition of Plants, and the Cause of Exosmose and Endosmose.* By G. RAINEY, M.R.C.S.E., Demonstrator of Anatomy, and of Microscopic Anatomy, at Saint Thomas's Hospital. With Two Plates.—*London*, 1847, 12mo, pp. 52.

THIS little treatise contains the results of numerous experimental inquiries and microscopic observations, which have been carried on for some time by its able author; and the perusal of it will suggest many new ideas to the vegetable physiologist. We are sorry that want of space prevents us from entering upon a more detailed critical examination of its contents; particularly as Mr. Rainey's conclusions in regard to the course of the sap run counter to the views which are commonly entertained on the subject. He considers that he has shown, "First, that the crude sap ascends along a tissue which chiefly exists between the cells, but which enters also into the structure of the more solid and permanent parts of a plant; and, secondly, that the elaborated fluid, both in its descent and ascent, passes along the ducts and spiral vessels." The first of these positions is based on the results of experiments on the absorption of a solution of bichloride of mercury. These results are not a little curious, and we have no doubt that they are most accurately stated; but we do not see that they require, or even justify, the inferences founded upon them, opposed as these are by so many other reasonings, based on structural arrangements, as well as by actual observation of the process. We cannot understand what Mr. Rainey means by the *ascent* of elaborated sap, as distinguished from that of the crude sap, or by elaboration taking place in the roots. Every vegetable physiologist knows that in spring the ascending sap is charged with a considerable amount of gummy and saccharine matters, which it has derived from the starchy deposits laid up in the roots at the end of the previous season; but the presence of these does not seem to us to deprive the liquid of its character as "crude sap;" and the fact is unquestionable, that this liquid rises through the large open ducts which most woody stems possess. We believe that Mr. Rainey is perfectly justified in his assertion that the "spiral vessels" not unfrequently communicate with each other, and serve for the conveyance of fluid; but we believe also, from our own observations, that the fact of non-communication is more common, and that the closed spiral vessels, at certain periods of their life, at least, contain air only. We recommend Mr. Rainey's treatise to all who are interested in the inquiries of which it treats.

ART. VI.—*Report to the House of Representatives of Massachusetts, of the Commissioners appointed to inquire into the Condition of Idiots in the Commonwealth.—Boston,* 1847. 8vo, pp. 20.

WE are very glad to see from this Report, that the progress recently made in France, Prussia, and Switzerland, in improving the condition of idiots, has attracted the attention of the authorities of some of the states of the North American Union; and that they are taking active steps towards the attainment of the same philanthropic object in their own country. The Commission, of which Dr. Howe—so well and favorably known in connexion with the remarkable case of Laura Bridgman—is chairman, has applied itself—1st, to the determination of the number of idiots in the commonwealth; 2d, to an examination of their actual condition; and 3d, to an investigation of the amount of improvement which may be expected from well-directed treatment.

Their information on the first point is as yet only partial, extending to 171 towns, containing an aggregate population of 345,285 inhabitants; the number of idiots in which is 543, or about one in 800. From the results of their inquiries under the second head, which show that the condition of these unfortunate persons is very materially influenced by the character of those who have the charge of them, we shall make the following extract:

> "In some towns, we found the idiots, who were under the charge of kind-hearted, but ignorant persons, to be entirely idle, given over to disgusting and degrading habits, and presenting the sad and demoralizing spectacle of men, made in God's image, whom neither their own reason, nor the reason of others, lifted up above the level of the brutes.
>
> "In other towns, idiots, who, to all appearance, had no more capacity than those just mentioned, were under the charge of more intelligent persons, and they presented a different spectacle—they were healthy, cleanly, and industrious.
>
> "We found some, of a very low grade of intellect, at work in the fields, under the direction of attendants; and they seemed not only to be free from depraving habits, but to be happy and useful.
>
> "The inference to be drawn from this is very important. If persons, having only common sense and common humanity, but without the advantage of experience or study, can so improve the condition of idiots, how much could be done by those who should bring the light of science, and the experience of wise and good men in other countries, and the facilities of an institution adapted to the training of idiots,—how much, we say, could be done by such persons, towards redeeming the minds of this unfortunate class from the waste and desolation in which they now lie!" (p. 3.)

The information collected under the third head was not in a state sufficiently complete to be presented; but an interesting letter is appended from Mr. George Sumner, a gentleman of admirable qualifications as a discriminating observer, as well as a genuine philanthropist, relative to the amount of progress already made in the Parisian schools for idiots. We should gladly have transferred nearly the whole of this letter to our pages, as the testimony of an intelligent and unprejudiced eye-witness; but we can only find room for the following passages, which will give a general summary of the results of his inquiries, these results, be it observed, not hastily caught up, but gathered as the fruit of prolonged and watchful attention.

"During the past six months, I have watched with eager interest the progress which many young idiots have made, in Paris, under the direction of M. Seguin, and, at Bicêtre, under that of Messrs. Voisin and Vallée; and have seen, with no less gratification than astonishment, nearly one hundred fellow-beings who, but a short time since, were shut out from all communion with mankind,—who were objects of loathing and disgust,—many of whom rejected every article of clothing,—others of whom, unable to stand erect, crouched themselves in corners and gave signs of life only by piteous howls,—others, in whom the faculty of speech had never been developed,—and many, whose voracious and indiscriminate gluttony satisfied itself with whatever they could lay hands upon—with the garbage thrown to swine, or with their own excrements;—these unfortunate beings—the rejected of humanity—I have seen properly clad, standing erect, walking, speaking, eating in an orderly manner at a common table, working quietly as carpenters and farmers; gaining, by their own labour, the means of existence; storing their awakened intelligence by reading one to another; exercising, towards their teachers and among themselves, the generous feelings of man's nature, and singing, in unison, songs of thanksgiving.

"It is a miracle, you will exclaim; and so, indeed, it is,—a miracle of intelligence, of patience, and of love.......

"The fact, I have said, is now clearly established, that idiots may be educated,—*that the reflective power exists within them, and may be awakened by a proper system of instruction*; that they may be raised, from the filth in which they grovel, to the attitude of men; that they may be taught different arts which will enable them to gain an honest livelihood; and that, although their intelligence may never, perhaps, be developed to such a point as to render them the authors of those generous ideas and great deeds which leave a stamp upon an age, yet, still, they may attain a respectable mediocrity, and surpass, in mental power, the common peasant of many European states." (pp. 7, 8.)

We trust that it will not be long ere our own government turns its attention to this most important subject. The lunacy commission is, we believe, in possession of information that shows the proportion of neglected idiots in this country to be at least equal to that which we have cited from the Massachusetts Report; which would give a number of upwards of 2000 human [illegible] in the metropolis and its environs alone, having mental and moral [illegible], capable of a considerable degree of development, but remaining in a state of degradation scarcely elevated above that of brutes, or even in many instances absolutely as well as relatively below it.

Art. VII.—*A Few Remarks on the Expectant Treatment of Diseases.* By ΑΚΕΣΤΗΣ.—*Bristol*, 1847. 8vo, pp. 16.

These "Remarks" appear to have been suggested by the controversy between Dr. Symonds and the late lamented Dr. A. Combe, in the pages of one of our predecessors; and their purpose is to show, on the one hand, that "the normal route of Nature is one of pain, decay, and death,"—that, "from the instant that growth was assumed disease was produced, the law which developed beauty unfolded deformity, the power which caused increase wielded decay, the principle of life was pregnant with death;" whilst, on the other, the intelligence of man enables him to "oppose, in a limited sphere, a new train of actions to those which are at that period existing in the physical universe,—he can alter the relations of dead matter to dead matter, and of this to organized being, although he cannot change the laws, or modes of action, which regulate their mutual effects." The disciples of the "expectant," or so-called "natural" method of treating diseases must possess, it is argued, a firm and rooted *optimism* in respect to organic phenomena. They ought, to be consistent, to be perfectly content with the *present* as right and good, and to make no attempt to improve it; that is to say, they ought to allow disease to run its course without interference of any kind, satisfied that the powers of the 'vis medicatrix' are competent to do all that the nature of the circumstances permits.

We cannot admit that either the premises or the conclusions of this argument are sound. In treating of the natural tendency of the body to disease, decay, and death, the author appears to us to have kept the fact too much out of view, that the daily, hourly, constant tendency is to the maintenance of life, health, and vigour; and this, not by a simple, regular progressive movement, like that of a clock, but by a complex series of actions, balanced against each other, of which a large proportion are destined to keep in check the processes that would otherwise destroy life, and do restrain them during the allotted period of man's existence, unless their regular course should be interrupted. And even when disturbed by external agencies, these functions do possess the power, in a vast majority of cases, of self-rectification; noxious matters introduced into the blood being eliminated by the excreting processes, losses of solid substance being repaired by renewed growth. But that, in a large proportion of such cases, Art can assist and accelerate the process of cure, and that in many other cases, in which the "vis vitiatrix" would be too strong for unaided Nature, the power of art can restrain and finally vanquish the morbid process, no reasonable man can hesitate in admitting. The great question for discussion, in our apprehension, is, the true foundation of the therapeutic art; whether we are to build up its rules upon the supposed results of the action of certain remedies in particular cases of disease, the natural course of that disease unaffected by interference being comparatively unknown; or whether we should in the first place study the means by which Nature works out the cure, in the large proportion of cases that tend spontaneously to a favorable termination, and should so direct our remedies that they may operate in accordance with her indicatio[illegible]ead of (as has been too frequently the case) setting up a new action o[illegible]r own, which may be completely opposed to her plans. And even in the cases in which the disease seems to have a tendency from the first towards a fatal termination, the question is, whether the cure would not be accomplished [illegible] natural processes, if *time* could be given for these to come into play; [illegible]d whether the success of a great deal of our treatment (e. g. the temporary support given by stimulants in cases of extreme prostration) does not rather depend upon this mode of action, than upon any more direct agency as regards the disease itself. We cannot, at present, enter more fully into the discussion of this question; but we may advert to cases of narcotic poisoning as affording a good illustration of our meaning. If the amount of the poison be moderate, it is gradually decomposed or eliminated from the blood, probably in great part through the respiratory process; and the symptoms, at first alarming, gradually pass off spontaneously, though judicious treatment may accelerate the recovery. But if a more powerful dose has been carried into the circulation, and threatens to suspend the respiratory movements, art can do much to avert the fatal result which would otherwise ensue. But all its efforts are directed towards the sustenance of the ordinary vital operations, particularly of the respiratory movements, until the decomposition and elimination of the poison are effected by the natural curative processes. In other words we give the "vis medicatrix" *time* and *opportunity* to act. So, in idiopathic fevers and other diseases, resulting from a poison introduced into the blood, we can do little else than aid in the eliminating processes, keep in check some of the accidental results of the change in the circulating fluid, and sustain the powers of the system until such time as its natural tendency to restoration brings round recovery.

We are far from asserting that these are examples which are applicable to all the disorders which come under the care of the physician, or to many of those which fall to the province of the surgeon. But we are convinced that the more the natural progress of disease is studied,—the more we discriminate between the results of the injurious agents by which it has been produced, and the operations by which Nature endeavours to remedy or oppose these,—the more shall we find the benefit, nay, the absolute necessity, of working in accordance with Nature's dictates, and of giving to our remedies such a direction as may aid and not antagonize her salutary processes.

To this extent, then, we own that we are disciples of the "natural" system of treating disease; but we cannot assent to the justice of the term "expectant," as applied to that system: we simply prefer to follow Nature rather than Empiricism as our guide in the therapeutic Art; and we have shown that we are far from recommending a let-alone practice, where there is any indication for rational treatment.

Art. VIII.—*Account of a New Anæsthetic Agent, as a Substitute for Sulphuric Ether, in Surgery and Midwifery.* By J. Y. Simpson, m.d., f.r.s.e., Professor of Midwifery in the University of Edinburgh, &c. &c. —*Edinburgh*, 1847. 8vo, pp. 24.

We notice this pamphlet, not because we suppose that any one of our readers is ignorant of the essential part of its contents, but because we deem it right that our Journal should contain some record of so important an improvement as that which Dr. Simpson has introduced in the induction of artificial anæsthesia, and also because the information which we have obtained from various sources enables us to confirm, in the fullest degree, the statements put forward in the first instance by Dr. Simpson, with regard to the advantages of Chloroform over Ether. The more rapid and complete production of the insensibility, the facility and freedom from injurious effects with which this may be almost indefinitely prolonged, the quickness with which its effects subsequently pass of, and the almost invariable absence of any unfavorable influence that can be fairly attributed to its use, all concur with the ease and readiness with which it may be administered, and with the absence of necessity for any special apparatus for the inhaling process, to give to Chloroform a decided superiority, and, in fact, to make it as nearly *perfect* as any such agent can be expected to be, the varieties and idiosyncrasies of the human constitution being duly kept in view.

We have reason to believe, not merely that chloroform will universally replace ether, but that the majority of those practitioners, who, for various reasons, objected to the inhalation of ether, are already yielding to the virtues of chloroform; and we trust that the earnest appeal which has been made by Dr. Simpson, in reference to the prevention of human suffering which its use may procure during the process of parturition, will not be without its influence on those engaged in obstetric practice. Whether it should or should not be exhibited in an easy natural labour, is a question which may be safely left, we think, to the decision of the patient; but that in difficult and protracted labours, of almost every description, and in nearly every case requiring manual interference, great and varied benefits are derivable from its use, seems to be the concurrent testimony of all who have employed it under such circumstances. In a case recently published by Dr. Simpson (Lancet, Dec. 11, 1847), a patient was kept under its influence for *thirteen hours* consecutively, the labour being rendered tedious by the narrowness of the pelvic canal, and the child being at last delivered by the forceps. If any permanently injurious influence were to be dreaded from the exhibition of chloroform, it would surely be manifested in such a case as this; and yet we are assured by Dr. Simpson that the child when born showed no other want of vital power than was fully accounted for by the long-continued pressure to which it had been subjected; and that the mother's recovery was remarkably rapid, the child also speedily becoming perfectly well.

We much regret to find that Dr. Simpson's philanthropic efforts for the relief of human suffering have been opposed on religious grounds by some well-meaning individuals, who consider that we are not justified in doing anything to alleviate or remove the effect of the curse pronounced upon our first progenitrix. Of course, to be consistent, such persons should set their faces against every kind of assistance which the skilful obstetrician can afford to the suffering mother, as well as to all the improvements in machinery, &c., by which the labour of *man* can be in any degree diminished, and Adam's share of the load thus lightened.

It is right that we should add that Dr. Simpson, in his latest communications on the subject, lays great stress upon the purity of the chloroform, as of importance in securing its early and complete action, and in preventing unpleasant consequences; and it appears that, in its perfectly pure condition, it exerts no irritating influence upon the cutaneous surface.

We purpose returning to the subject of the inhalation of anæsthetic agents in an early Number, for the sake of presenting our readers with a digest of the results of their extended use, involving, as these do, many considerations of great physiological interest, as well as of great practical importance.

ART. IX.—1. *Disinfection; or Remarks on the Health of Towns, and the Manufacture of Inodorous Azotized Manure from Animal and Vegetable Matter.* By CHARLES F. ELLERMAN, late Hanoverian Consul at Antwerp.—*London*, 1847. 8vo, pp. 24.

2. *Report of the First Meeting of the Metropolitan Sewage Manure Company, incorporated* 1846. *Together with an Analysis of Evidence, &c.—London*, 1846. 8vo, pp. 50.

ALTHOUGH the attention of one section of our readers, at least, has been recently drawn (Medico-Chirurgical Review, Oct. 1847, p. 483) to the claims advanced in favour of certain "disinfecting fluids" by the parties severally interested in their sale, yet the appearance of another candidate for public favour in this department gives us a reason for briefly expressing our own convictions upon the subject.

We dissent, *in toto*, from the proposition, that an offensive odour and a poisonous miasm have any necessary relation. Thus, on the one hand, some of the most pestilential spots in the world, such as the mouths of certain African rivers, the Sunderbunds of the Ganges, and the Pontine marshes, give no warning of the presence of their destructive exhalations by an accompanying fœtor, so that the poison exists there without a stench. On the other hand, there is ample evidence that the most offensive odours, such as those disengaged from putrescent animal matter, do not themselves act as poisonous miasms in generating fever and other complaints. This position, which was fully established by Parent-Duchatelet, is in harmony with another well-known fact, namely, that the decomposition of vegetable matter, though attended with far less of odorous effluvia than that of animal, is far more pernicious in its influence on the human system.

This being the case, it is perfectly obvious that a chemical agent which should be quite capable of removing the offensive odours produced by animal and vegetable putrefaction, through their power of decomposing or fixing the sulphuretted hydrogen, hydrosulphate of ammonia, and other gases set free in the process, may, for anything that can yet be proved to the contrary, be perfectly inert as regards the poisonous miasmata on which the origin and spread of infectious diseases are presumed to depend. It is, therefore, a complete *petitio principii* to assume that a *deodorizing* agent, however complete may be its operation, is also a *disinfectant;* and we cannot but regret that any members of our profession should have so far lost sight of these simple truths, as to sanction the claims put forward by interested parties in favour of the "disinfecting" powers of their several nostrums.

The latest of these claimants, Mr. Ellerman, who comes forward merely as the agent of Mons. Dam, the real inventor of the compound, has wisely yielded to the representations made by competent judges on this point; having substituted in his subsequent appeals the term "deodorizing" for "disinfecting;" and having urged, as the peculiar merit of the invention, not any assumed power of disinfection, but the facility which its employment will afford in the solution of one of the most important and difficult problems of the day, the application of sewage-waste to the production of human food, by its employment as manure. The great obstacle to this application, as our readers need scarcely be told, lies in the difficulty of transporting and distributing the sewage to the spots in which it is to be thus employed, arising out of the offensive and noxious characters of its exhalations. And *if* it should prove that Mr. Ellerman's deodorizing process should be as effectual in preventing the generation of febrile miasmata, as it is in preventing the liberation of odorous gases, it will *in this manner* be a great boon to the public. We have witnessed comparative trials between this fluid, Sir W. Burnett's, Ledoyen's, and Beaufoy's chloride of lime; and we are bound to say that, as far as regards its deodorizing powers, Ellerman's fluid is decidedly superior to any one of the rest. It has, however, a slight acidulous smell of its own; which will be an obstacle to its employment in ships and dwelling-houses. One of its greatest promised advantages will be its comparative cheapness. But we feel ourselves bound to append to this commendation the important qualification, that although the odour of sewage manure may be completely neutralized by such processes, it is quite possible that the poisonous miasmata may be generated as powerfully as before—a point which experience alone can test; and further, that, however promising may be the appearance and sensible properties of the manure which is made by its use, the only test of its value is its actual productiveness, which, so far as we know, yet remains to be proved. It seems to us quite possible that the chemical agents contained in the deodorizing process may have a serious effect in impairing this.

Should the facility with which privies, cesspools, &c., can be *deodorized* by Ellerman's process lead to the retention of such nuisances, we shall consider that it is positively prejudicial rather than useful. Our sense of smell was certainly given to us, among other purposes, in order to warn us against noxious emanations; and it is, as it were, neutralizing its utility, to deodorize without also destroying poisonous miasmata. We believe that the immunity from fever enjoyed by catgut-makers, glue-makers, tanners, and others whose employments lie amidst putrescent animal matter, greatly depends upon the warning which they receive through their noses, *compelling* them to obtain as much fresh air as circumstances will permit, and preventing that accumulation and stagnation of noxious matter in the air, which seems to be the condition most favorable to the generation of pestilential miasmata. We trust, therefore, that deodorizing fluids will never be employed, or even proposed, as substitutes for efficient drainage, ventilation, &c.; and that the profession in general will strenuously endeavour to set the public right on this head. To their legitimate employment, in facilitating the use of sewage as manure, we heartily wish all success. There can no longer be the slightest question of the marvellous efficacy of the refuse of our towns in promoting the growth of the materials of their food; and the agricultural public is fully alive to the fact. The value of certain meadows in the neighbourhood of Edinburgh has been increased, through the use of a part of the town drainage for the purpose of irrigation, by more than £5000 a year; and there seems no reason to doubt that, if the whole drainage of London could be so employed, at a sufficient distance from the town, the annual increase in the value of the land to which it would be applied would exceed £500,000. By a different method of computation, founded upon the comparison of the substance of the excreta with other manures, and taking the market price of the latter as the standard, it has been calculated that the refuse of the town of Sheffield, with a population of 110,000 inhabitants, is worth £30,000 a year. Surely these sums are worth saving, if efficient means can be devised for the conservation and application of the sewage, without evil results to public health.

Art. X.—*Illustrations of Instinct, deduced from the Habits of British Animals.* By Jonathan Couch, f.l.s. &c. *London,* 1847. Post 8vo. pp. 343.

The purpose of this little work is to aid in the diffusion of improved views as to the relative modes of action of instinct and intelligence, and to point out the path by which these may be more clearly distinguished, so that their respective provinces may be better defined. It does not aim at the character of a complete or even of a systematic treatise on the subject; but brings together in a pleasing form a variety of disconnected observations, that may serve as *illustrations* of the nature of the inquiry which the author desires to promote.

"Preference has been given to examples derived from the creatures of our own country; because these are best known to an ordinary inquirer; and because it is the author's desire to call into this field of examination a greater number of men capable of useful observations, if their attention were once directed to the pursuit. Many curious habits remain undescribed, to reward the industry and patience of an observer who will study them abroad, in the fields and woods; and the arts by which the wild animals of Britain still maintain their standing among us, in [illegible] of the exterminating endeavours of their great enemy, and amidst so many [illegible] opposing influences, form perhaps the most interesting portion of their history." (Preface.)

Mr. Couch starts with what we regard as essentially correct notions of the relative characters of instinctive and intelligent actions, of the almost exclusive predominance of the former principle of action in the lower animals, and of the modifications gradually introduced by the increasing importance of the latter, as we ascend the scale. And we can therefore recommend his work as possessing a real scientific value, as well as from its presenting an agreeable source of healthful recreation. We could wish, however, to be able to trace some principle of sequence in the arrangement of its contents; and we should like to meet, in a future edition, with a larger proportion of original facts, such as an observer possessed of Mr. Couch's qualifications and opportunities can scarcely fail to have collected.

Art. XI.—1. *Portraits of Diseases of the Skin.* By Erasmus Wilson. F.R.S. Fasciculus I. *London,* 1847. With four coloured Plates. Folio, pp. 8.

2. *On Ringworm, its Causes, Pathology, and Treatment.* By Erasmus Wilson, F.R.S., Consulting Surgeon to the St. Pancras Infirmary. *London,* 1847. With 12 Figures on Steel. Post 8vo., pp. 102.

These two works are not more dissimilar in size, than they are in their objects and characters. The first-named may be truly designated a splendid performance, surpassing in the artistic beauty of its delineations, and fully equalling in their fidelity to nature, anything of the kind which has yet been brought out either in this country or on the continent; and one, consequently, which is most creditable to all parties concerned in its production. The following extracts from the preface will explain Mr. E. Wilson's intentions in regard to the objects of the work:

"I have determined that no labour or expense shall be spared to render it complete as a series of 'Portraits of Diseases of the Skin;' and my confidence in the talent of the artist, Mr. William Bagg, emboldens me to hope that the strict fidelity of the delineations to Nature will only be surpassed by their beauty and excellence as works of Art.

"In another sense I might have termed the work, 'Illustrated cases of Cutaneous Disease,' inasmuch as it is my intention to report the case of the patient with each plate, introducing among the details of the case such general deductions and practical observations as may tend to explain more fully the history and management of the disease. By following this plan the student will be placed [illegible] position of the consultant before whom the case is brought; and the peculi[illegible]racters of the disease, being thus strongly impressed on his mind, will be [illegible] available when a similar case is presented to his notice in practice. Indeed the cases will be given and the portraits drawn, in the order in which they come before myself, and without reference to classification; the circumstance determining the selection being the character of the case as a good specimen of the particular disease, and the instruction which it may be calculated to convey. By pursuing this course, the practitioner will have ensured to him the most characteristic and best examples of cutaneous disorder, and the author will be relieved from the yoke of following a particular arrangement in the sequence of his illustrations. Another advantage will arise out of this plan, which is, that the artist, by transferring his drawing immediately to the stone, will be enabled to complete his colouring of the lithograph from the patient; and while all the peculiar tints of the disease are fresh in his mind." (Preface, p. 1.)

The progress of such a work must be necessarily slow, and somewhat uncertain; and we think, therefore, that Mr. Wilson acts judiciously in not venturing to promise more than two fasciculi in each year. Each separate plate may be regarded as complete and independent; but the whole will form, it is hoped, a complete series of illustrations of cutaneous diseases, which may be arranged according to any classification preferred by the purchaser, but which will have, of course, a special reference to that proposed by Mr. Wilson himself in his general treatise on the subject. The plates in the fasciculus before us represent cases of *chloasma*, or liver-spot, *favus*, or honeycomb ringworm, in two states, and *psoriasis palmaris*, or dry tetter. They are all most admirably drawn and coloured: but we give the preference to the last for its life-like aspect. We trust that Mr. Wilson will receive sufficient encouragement from the profession to enable him to carry on his spirited undertaking without loss to himself—profit he evidently does not expect. We can scarcely speak too strongly of the merits of the work.

Of the treatise on Ringworm, on the other hand, the less that is said the better for Mr. Wilson's reputation. It is one of those hybrid books, which is not addressed to the profession nor to the public exclusively; but which, under the guise of instructing the former, seems to hang out as a bait to the latter. There is very little original matter in it, except some new nomenclature, for which, of course, a purely scientific treatise is the proper place; and what little there is might easily have found a place in a medical journal. Mr. Wilson restricts the title of ringworm to "diseases producing a brittleness of the hair, and giving rise as a consequence to its breaking off near the skin." The diseases coming under this designation are the crusted or honeycomb ringworm (*Favus*) and the common scurfy ringworm, to which Mr. Wilson gives the new designation of *Trichinosis furfuracea*. He gives a description of the microscopic characters of the former, derived from his own observations; and these are illustrated with good figures. But there is little of novelty in these descriptions; and in his assertion of the non-vegetable nature of the cells, the development of which constitutes the peculiar feature of the disease, he is by no means so original as he seems to imagine. In opposition to most pathologists, he maintains the non-contagious character of the disease; but as he seems to have had but small experience of it, we can scarcely set his negative evidence against the positive testimony of those who have had more extended opportunities of watching its propagation. Mr. Wilson is, of course, aware [illegible] any contagious disease may present itself sporadically, when circums[illegible]ces do not favour the development and transmission of the poison. With respect to the common ringworm, we are surprised to find the same doctrine of entire non-contagion maintained; since we have ourselves seen numerous examples of its propagation, under circumstances in which no other reasonable explanation could be given of its diffusion. The relation between the common ringworm and favus is thus stated by Mr. Wilson:—The cells characteristic of favus are a peculiar development of the germs that would otherwise only constitute the granular matters of the interior of the hair, and are aggregated by multiplication into plant-like stems. In common ringworm, on the other hand, the process of cell-development ceases with the production of nucleated granules, which constitute a sort of hypertrophied state of the natural tissue of the interior of the hair. We find nothing particularly novel in regard to the treatment of the complaint; and we are surprised to find no mention made of a remedy which has long been in frequent use, namely, the application of the nitrate of silver in substance to the diseased patch. The remarkable efficacy of this remedy, when applied in recent cases, not merely in checking the spread of the disorder in the individual, but also in preventing its appearance in other children, amongst whom it was previously diffusing itself rapidly, is to our minds another indication of the contagious nature of the malady. The book concludes with a chapter on diseases analogous to, or liable to be mistaken for, ringworm. Of these he has no others to enumerate than the *Plica polonica*, which he supposes to correspond with ringworm in its essential nature; and *Alopecia areata*, the *Porrigo decalvans* of Willan, which is often mistaken for ringworm by ignoramuses both in and out of the profession, but which is decidedly not contagious. He mentions the case of a poor governess who lost her situation from this cause; and says, "If I wanted a motive or an excuse for writing this book, I need not have sought a stronger one than that afforded by this young lady's case. A poor victim discarded and homeless, the victim of a baseless prejudice."

We are afraid that Mr. Wilson finds excuses for book-making rather too easily. At any rate, we hope it will be long ere he gives us another treatise of this kind.

Art. XII.—*A Brief Topographical and Historical Notice of Calcutta: with a Sketch of the Rise and Progress of Sanitary Improvement in the East Indies.* By J. R. Martin, Esq. f.r.s.—*London*, 1847. 12mo, pp. 82.

This little pamphlet, the substance of which originally appeared in the Lancet, gives a very interesting account of the history of the sanitary movement in Calcutta, which anticipated by several years the corresponding movement at home; chiefly in consequence of the energy and perseverance with which Mr. Martin urged upon the Indian government the importance of preventive measures, and the clear-sighted and decisive conduct of the late lamented Lord Metcalfe, who took up and acted upon the suggestions made by Mr. Martin, *in opposition* to the official authority of the Bengal Medical Board. From the account which is here given of the nature of the site, the deficiency of drainage, the crowding of the population, the bad construction of the dwellings of the lower classes, the want of a good supply of water, the filthy state of the public markets, the various bad habits of the native population, and other evils of a similar character, the high rate of mortality which has prevailed at Calcutta is most fully accounted for. "A greater and more appalling array of fatal causes, both moral and physical," says a writer in the Calcutta Review, when commenting on Mr. Martin's Report, "could not be found probably in any other town [illegible] the world,—no, not in Africa or in Turkey."

It is one benefit of an arbitrary government, that, when conducted by enlightened men, it can act for the benefit of the community in advance of public opinion, instead of feeling itself perpetually restrained and retarded by the claims of "vested interests." A series of legislative acts has been already passed by the Indian government, having for their object to render Calcutta both healthy and commodious; and we cannot doubt that their fruits will be apparent, as soon as ever they shall have come into efficient operation.

Mr. Martin's recommendations further extended to a collection of reports from the various provinces, cities, and cantonments, on the medical topography and statistics of each place; and these, too, were adopted by Lord Metcalfe, in spite of the "cold water" thrown on them by the Bengal Medical Board. His instructions have been admirably followed out in the Madras Presidency; though, for some reason, which does not appear, they have been almost nullified in Bengal by the obstructive party. We shall look forward with great interest to the publication of these Reports, as calculated to throw great light upon the etiology of disease in general, and of the diseases of tropical climates in particular.

Art. XIII.—*The Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias; comprising standard and approved Formulæ for the Preparations and Compounds employed in Medical Practice.* By Henry Beasley. Fourth Edition, corrected, improved, and enlarged.—*London*, 1848. 16mo, pp. 456.

The steady and continued demand for this little work sufficiently attests its utility; and the following extract from the advertisement to the new edition now called for, will show the anxious care of its compiler to maintain the reputation which it has acquired:

> "Besides the insertion of formulæ and processes for the new remedies which have come into use since the former editions were issued, numerous additions and alterations have been made throughout the work. Many typographical and other errors, so difficult to avoid in a work of this kind, have been discovered and corrected; several formulæ, derived from respectable compilations, have been amended by a reference to the original authorities; the doses are more frequently given, especially of the remedies not in general use; the comparison between the different pharmacopœias has been extended; deficient details in the processes have been supplied; and many short practical remarks introduced, all of which, it is hoped, will increase the utility of the volume. By particular request, a condensed list of the principal works consulted, is now appended."

Art. XIV.—*An Essay on the Diseases of the Jaws, and their Treatment.* By Leonard Koecker, Surgeon-dentist, &c. &c. New edition, with copious Notes, and an Appendix, containing tables of upwards of three hundred cases, by J. B. Mitchell, m.d.—*London*, 1847. pp. 94.

This is one of those works of pretence written ostensibly for the instruction of the profession, but in reality to advertise the author, and to lead the public to believe that he is thoroughly versed in the knowledge of the diseases of which he treats. The appendix, containing a table of 335 cases, taken from various sources, is calculated to impress the readers of the work with the extent of the author's researches, but [illegible]lueless in respect to the object for which they were collected, viz., to e[illegible]date the causes of diseases of the jaws. At best, the table furnishes an example of misapplied industry. We should consider it a waste of time and space to bestow further notice on so indifferent a production.

PART THIRD.

Periscope.

ANATOMY AND PHYSIOLOGY.

On the Intimate Structure of Bone. By John Quekett, Assistant Conservator in the Museum of the Royal College of Surgeons.

The chief purpose of this paper is to draw attention to the differences that exist between the bones of different animals, in regard to the size and form of the osseous lacunæ, and the number and mode of radiation of their canaliculi. In Mr. Quekett's opinion, these characters are sufficiently definite and constant to serve for the determination of the class, and sometimes even of the order, to which the animal belonged, from the microscopic examination of even a minute fragment of one of its bones. If this should be capable of satisfactory proof,—and we have great confidence that Mr. Quekett would not advance any general statement of this kind but as the result of sufficiently extended researches,—the test is one of great importance in palæontology, whilst the fact is one of high interest to the physiologist. We may notice that Mr. Bowerbank has arrived at the same general conclusions; and has specially applied this test to the determination of some doubtful wing-bones found at the Isle of Sheppey. The question lay between their having belonged to a long-winged sea-bird, such as the albatross, or to a gigantic pterodactyle; and it was decided, unequivocally as we believe, in favour of the latter, thus enlarging our ideas of the size of these flying lizards of the ancient world, since the creatures of which the fragments in question formed part must have had a spread of wing not less than twelve or fourteen feet.

The following table gives Mr. Quekett's measurements of the lacunæ in different animals, which may be taken as representatives of their respective classes:

	Long Diameter.		*Short Diameter.*
Man . . .	1-1440th to 1-2400th	of an inch	1-4000th to 1-8000th.
Ostrich . .	1-1333d to 1-2250th	„ .	1-5425th to 1-9650th.
Turtle . .	1-375th to 1-1150th	„ .	1-4500th to 1-5840th.
Conger-eel .	1-550th to 1-1135th	„ .	1-4500th to 1-8000th.

From this it will be seen that the lacunæ of the bones of birds are somewhat longer but narrower than those of mammals; and that there is an extraordinary increase in length in the lacunæ of reptiles and of certain fishes. There is, however, such a variety in the forms and dimensions presented by the latter class, and some of them approach so closely to reptiles in this character, that great caution must be employed in using it with any diagnostic purpose. It must be remembered, however, that the dimensions of the lacunæ, though capable of being most readily expressed, constitute only a part of the distinctive characters to which Mr. Quekett draws attention. The following table is of peculiar interest. It will be seen from it that the lacunæ of the perennibranchiate amphibia, to which group the first three animals belong, are distinguished by their remarkable breadth, as well as by their length; their large capacity thus corresponding with the enormous size of their blood-corpuscles. The lacunæ of the bones of the Lepidosiren correspond very closely with the foregoing in this character, and cannot be likened to any which present themselves in the class of fishes. The lacunæ of the pterodactyle nearly resemble those of ordinary reptiles.

	Long Diameter.	*Short Diameter.*
Proteus . .	1-570th to 1-980th of an inch	1-885th to 1-1200th.
Siren . . .	1-290th to 1-480th " .	1-540th to 1-975th.
Menopome .	1-450th to 1-700th " .	1-1300th to 1-2100th.
Lepidosiren .	1-375th to 1-494th " .	1-980th to 1-2200th.
Pterodactyle	1-445th to 1-1185th " .	1-4000th to 1-5225th.

Transactions of the Microscopical Society of London, vol. ii, pt. 2.

Experiments on the Glosso-pharyngeal Nerve. By Dr. Biffi and Dr. Morganti.

As the glosso-pharyngeal nerve has different anastomoses with other nerves, as soon as it has emerged from the foramen lacerum basis cranii, the authors, in order to determine whether it possesses primitive motor fibres, particularly directed their attention to the nerve whilst still situated within the skull. For this purpose they laid open the skull in animals, and then, having detached the nerve from the medulla oblongata, they irritated the peripheral, divided end, in order to see whether in this way contractions were excited in those parts to which the glosso-pharyngeus is distributed. Young dogs, two or three months old, were selected, on account of the ease with which the cavity of the skull could be opened, and the nerve exposed before the powers of life were exhausted. The brain was taken away, and the nerve was irritated, both by the forceps and by applying the poles of a very weak voltaic pile. At the instant when this was done, it was seen that convulsive twitchings were excited, owing to the contractions of their muscles in the uvula, velum pendulum palati, and in the two anterior arches of the fauces, on the side corresponding to that on which the nerve was irritated. These weak and limited contractions were very different from the powerful and extensive contractions produced in the whole of the velum palati and pharynx, by irritating the spinal nerve; so much so, that by merely observing the kind of motion, the experimenter could predict which nerve was being excited. The authors are satisfied by the results of the above experiment, performed on nearly thirty dogs, that muscular contraction is excited by the irritation of the peripheral part of the divided glosso-pharyngeal nerve. They obtained also the same result in experiments performed on the horse and lamb.

Drs. Biffi and Morganti mention some circumstances which we regard of importance, as they tend to explain the cause of the conflicting accounts that have been given respecting the existence or non-existence of proper motor-fibres in the nerve. They found that the excitability of the glosso-pharyngeal nerve vanishes very quickly; so that if, owing either to the inexpertness of the operator, or to other causes, the experiment be prolonged, no movements follow the stimulation of the nerve. In any future experiments, if such should be deemed necessary, two points would thus, it is clear, require particular attention: first, that the nerve should be so quickly exposed as to retain sufficient power, together with the muscles implicated, to respond readily on the application of stimuli; second, that the nerve should be excited whilst still contained within the cranium.

The general conclusion of the authors is, that "the glosso-pharyngeal nerve is provided with intrinsic motor fibres."

Those of our readers who have attended to the progress of physiology, will perceive that these experiments tend to contravene the opinions lately advocated in this country, resting more especially on the inquiries of Dr. Reid, according to which the glosso-pharyngeal in itself contains no motorial fibres, and can only excite muscular movements when stimulated as an incident spinal nerve. We have ourselves always felt a difficulty in admitting the correctness of this opinion, notwithstanding the numerous and careful experiments of Dr. Reid; and for this reason more especially, that the denial of a direct motor power to this nerve is incompatible with its interesting and well-marked anatomical characters. It is known that the glosso-pharyngeal has two roots; that whilst it is lodged in the upper part of the foramen lacerum, one of these fasciculi presents a distinct ganglion (*g. jugulare* of Ehrenreiter); and that on the surface of this ganglion the other fasciculus runs, without in any degree participating in its formation. In each and all of these respects the nerve entirely corresponds with the compound nerves of the spinal cord; in fact, it is precisely on a small scale what these nerves and the fifth pair are on a large one; and if it be carefully exposed in a recent subject, and be then examined with a lens, this identity of character becomes so striking, that those who have thus made a direct observation will be slow in admitting any inferences drawn from vivisections, which would, if received, invalidate so significant an anatomical structure. Our own opinion is, that whilst the glosso-pharyngeal determines some of the muscular actions about the palate and fauces by its own motor fibres, it provokes other muscular nerves, especially those of the vagus, as an excitor nerve; and that it is in addition, though we are scarcely justified in saying exclusively, a nerve of taste.—*Müller's Archiv für Anatomie*, 1847.

ORGANIC CHEMISTRY.

On a Function of the Red Corpuscles of the Blood, and on the Process of Arterialization. By George Owen Rees, M.D., F.R.S., &c.

The author [illegible] that he was first led to the new theory he has formed for the explanation of the chemical phenomena of respiration, and more especially of the change in the colour of the blood which occurs in that process, by having observed that a garlick odour, similar to that evolved from phosphorus, was produced by agitating in distilled water the clot obtained from some specimens of venous blood. His attention was consequently directed to the investigation of the state in which the phosphorus exists in the blood; and the result of that investigation was the theory, of which the following is a succinct outline.

The venous corpuscles are known to contain fat in combination with phosphorus. This compound ingredient of the corpuscles, on coming into contact with atmospheric oxygen during the respiratory act, is consumed, and combining with that oxygen, forms the carbonic acid and water which are expired, and also phosphoric acid, which, uniting with the alkali of the liquor sanguinis, forms a tribasic phosphate of soda. This salt, like many others, acts upon hæmatosine in such a manner as to produce the well-known bright arterial tint.

The analyses which the author has performed in order to test the correctness of this theory were made upon the blood, both of the veins and of the arteries of the same animal; and also upon separated portions of the same venous blood, one of which portions had been artificially arterialized by having been brought into contact with air, while the other portions had not been so exposed. These comparative experiments showed that arterial blood, both when obtained from the vessels, and when artificially produced, contains in its serum a larger proportion of tribasic phosphate of soda than that obtained from the veins. The venous corpuscles, as they are contained in the clot, yield a fatty matter combined with phosphorus; while those from arterial blood yield a fat, the ashes of which manifest an alkaline reaction. Thus the venous corpuscles are shown to be acted upon, both by respiration and by the artificial arterialization of the blood, in such a manner as to lead to the formation of tribasic phosphate of soda at the expense of the phosphorus they contain.

No exact quantitative analyses were attempted by the author, the comparative experiments having been performed on small portions only of serum (from 25 to 40 grains); sufficiently large, however, to furnish satisfactory evidence of the actual presence of the phosphate in arterial blood, and also in those portions of venous blood which had been arterialized out of the body; while no such indications were obtained from similar portions of the blood contained in the veins.

At the conclusion of the paper, the author notices the experiments of Enderlin, in which no alkaline carbonate could be detected in the ashes of blood; and shows that this is the natural consequence of the phosphates of the clot being oxidized during combustion, and thus supplying a quantity of phosphoric acid sufficient to decompose completely the alkaline carbonate produced by the incineration of the lactate and albuminate of the serum. Most specimens of serum, even when obtained from arterial blood, yield an alkaline carbonate when incinerated; and this is always the case with the serum of venous blood. The author therefore thinks himself warranted in regarding the conclusion founded on Enderlin's experiments, that the blood contains no lactate, as being erroneous.—*Proceedings of the Royal Society, June* 3, 1847.

On the Urine in Typhoid Fever. By M. MARTIN-SOLON.

M. MARTIN-SOLON recently (Nov. 1847) read at the Académie an interesting paper upon the condition of the urine in typhoid fever, the principal points of which are thus summed up: 1. The urine in typhoid fever is less abundant, higher coloured, and generally more dense, than in health. 2. It is as acid as in the normal state, and sometimes more so. 3. It is rarely alkaline, but in consequence of its large proportion of urea, it rarely passes into the condition of alkalescence. So abundant is the urea, that sometimes a nitrate may be at once formed by the addition of nitric acid without any preliminary evaporation,—a density of from 1·030 to 1·036 indicating this condition. 4. Generally transparent, the urine is sometimes clouded with mucus (the *cna crema* of the ancients), or by an excess of too sparingly soluble salts, which give it a "*jumenteux*" appearance, and which give rise to sediments, formed especially of uric acid and the urates with colouring matter. 5. That critical signs deduced from these appearances are not to be depended upon. 6. That in transparent urine, nitric acid sometimes gives rise to no reaction, but in other cases produces a *cloudiness* immediately. 7. The same thing is observed in "jumentous" urine, when rendered clear by filtering. 8. That this cloud, of a peculiar tomentous aspect, formed by a bi-urate of ammonia, but the nature of which is perhaps not yet entirely known, is seen especially at the period of the resolution of typhoid fever and acute diseases, which it precedes and announces; and according to our clinical observations (in 54 cases) possesses a *critical* value which deserves attention. 9. That the bile undergoes a notable alteration during typhoid fever, which is doubtless the cause of the appearance of *biliverdine* in the urine. 10. That the urine sometimes becomes temporarily albuminous during the course of acute diseases; but that the congestion of various organs, especially partaken of by the kidneys, and the especial tenuity of the blood in typhoid fever, render such temporary albuminuria far more common in this than in other affections. 11. Temporary albuminuria is especially seen in severe cases of typhoid, and generally gives rise to the most unfavorable prognosis. 12. Temporary albuminuria may sometimes become continuous, and the kidneys then exhibit the usual pathological characteristics of confirmed albuminuria. 13. The inspection of the urine throws light upon the progress of a case of typhoid fever, and may serve as a means for the direction of its treatment.—*Bulletin de l'Académie*, tom. xiii, p. 398.

PATHOLOGY.

An Anatomical Examination of a Case of Hypospadias. By Professor THEILE, Berne.

THE subject of these observations was a man aged 41 years, who died from pneumonia, and was brought to the dissecting-room. It was not known whether he had been married or not. The penis, when the body was lying on its back, presented at first sight nothing abnormal. The pendant portion measured 2¼ Paris inches, but, when dissected, the whole length from the junction of the corpora cavernosa to the end of the glans, equalled 4 inches 10 lines. Its circumference was 14 lines. The glans, which was uncovered by the prepuce, measured from the corona to the point 11 lines, and its greatest breadth equalled 14 lines. No orifice of the urethra could be seen in the glans. At the under surface of the glans was a longitudinal fissure, 5 lines long, and two lines deep in the middle, covered by a whitish mucous membrane. The mass of the glans formed two projections on the sides of this fissure, which the author states was nothing else than the fossa navicularis, open below.

Towards the root of the penis, 16 lines behind the point of the glans, was found (as is not unusual in these cases) an opening leading into the urethra, and to which a slight ridge or elevation, 1¼ line broad, passed from the above described fissure. Six small openings, leading into canals running towards the root of the penis, were seen on stretching the skin belonging to the ridge: "these openings are the lacunæ Morgagnii, situated in the wall of the unclosed urethra;" they have been noticed in similar cases of hypospadias.

The scrotum included two testicles of the normal size and consistence; the other appended parts were also present; namely, the vasa deferentia, vesiculæ seminales, and prostate gland; the latter was 14 lines in length, 8¼ thick, and 16 lines broad. The membranous part of the urethra entered into a normal bulbous urethra.

But the most interesting part of this case concerns the state of the utriculus, or vesica prostatica, which has lately, and especially by E. H. Weber, been regarded as the analogue, in the male, of the uterus in the female. In order precisely to ascertain the anatomical condition of this interesting, and, as regards many cases of so-called hermaphroditism, important structure, and [illegible] exact relations to the neighbouring parts, Professor Theile took such precautio[illegible] as to obviate all sources of error. He found that a canal, originating by the usual opening on the utriculus, ran backwards for 1¼ inch, ending in a cul-de-sac 4 lines in diameter, and placed between the two vasa deferentia; this canal (vesica prostatica) with the exception of its anterior part, did not lie within the prostate, but below or behind this gland. Besides this structure, a small, oval, glandular body, 5 lines long, 4 broad, and 2 thick, was found behind, lying between the vesica prostatica and the prostate gland itself; it did not appear that this body was continuous with the substance of the prostate, although this continuity might have existed and escaped detection. Examined by the microscope, this small body presented an aggregation of cells or vesicles, which were much more easily seen in it, than in the proper prostate. Dr. Theile regards "this body, lying closely upon the vesica prostatica, as a middle lobe of the prostate." In order to ascertain the relation of the ductus ejaculatorius with the vesicle, a wax injection was thrown into the lower part of the vas deferens. On a careful examination, it was found that the ejaculatory duct did not open into the utriculus, but was only closely applied to its lateral wall, and then penetrated into the urethra in the usual place, near to the colliculus seminalis, or caput gallinaginis.

After describing the discrepancies among writers, from Morgaghi downwards, respecting the exact mode of termination of the ductus ejaculatorii, Dr. Theile points out the interest attaching to the presence of a large vesicle in and below the prostate gland, in relation to cases of hermaphroditism. He considers that the existence of so large a utriculus can only be attributed to an arrest of development; for, whatever differences of opinion may exist among embryologists as regards the primitive relations of the vasa deferentia, at their entrance into the sinus urogenitalis, it is at all events agreed that at a certain epoch a wedge-shaped projection, or pushing out (düsstulpung), arises between the mouths of the two seminal ducts, which by degrees becomes reduced into the small vesica prostatica, as usually happens in the adult. "In many cases of hypospadias, such an enlarged utriculus would be taken for a vagina or uterus. Huschke has already correctly observed, that the pretended uterus cystoides, described by Ackerman, in a case of hypospadias, is nothing else than the enlarged vesica prostatica. At its orifice the two small openings of the vasa deferentia running in the walls of the uterus, were found."

Professor Theile next notices a case of hypospadias, which, taken in connexion with that dissected by himself, and with the view he entertains of the character of the vesica prostatica, is of marked interest. In this instance, which was examined by the elder Soemmerring, and figured by his son, the individual, who had no enlargement of the mammæ, and had a thick beard, and the voice of a man, was nevertheless married to a man; the marriage was subsequently set aside; he lived to the age of 74, but without laying aside the female dress. On examination of the body after death, it was found that the upper part of the body had entirely the male characteristics; but the lower part, especially the pelvis, more resembled the female. The urethra and scrotum were fissured; the testicles remained in the abdomen. Between the glands and the anus, two openings were found, lying one above the other, which, according to Soemmerring's figure, were separated by a partition about 1 line in breadth. The opening next the penis, was the orificium urethræ; the lower one, situated two inches from the anus, led into a narrow canal, into which a quill could not be passed. The physician who first examined the parts, Dr. Schneider, thought he had detected a species of carunculæ myrtiformes in the entrance of this canal. A more careful examination showed that the body was 1½ inch long; that, when inflated, it was nearly as large as the little finger; and that it was placed between the urinary bladder and the rectum, but nearer to the former. Soemmerring laid open the canal by dividing the wall turned towards the rectum, and it appeared like an "alveus communis," into which the vesiculæ seminales opened. When quicksilver was injected into the vasa deferentia, which had a tolerably normal structure, it ran partly into the vesiculæ seminales, but partly into this pouch. It is of importance to the true interpretation of all cases of this kind of malformation, to det[illegible]ne the relation of this alveus communis, that is of the utriculus, to the openings of the vasa deferentia.

In Soemmerring's first plate, at the place where Schneider thought he saw carunculæ myrtiformes (parts, that is to say, of the female organs), two sounds are represented as being placed in the orifices of the *vasa deferentia* and *vesiculæ seminales*, unequivocal parts of the male organs. Upon this interesting conformation, Theile remarks: "this case allows of a parallel being drawn with the one above described by myself. If for example, we conceive, in the case observed by me, the fissure of the urethra to be prolonged backwards as far as to the colliculus seminalis (caput gallinaginis), then it must happen that the opening of the urethra will be placed above, the opening of the utriculus below, and on the two sides of the latter must be situated the orifices of the ductus ejaculatorii."

Although it is not our object on this occasion to enter into the question of hermaphroditism, it is yet desirable to call the attention of our readers to the new and important light which the facts just stated throw on some of the most involved and obscure instances of genital malformations. The existence in the male of a central sac or canal, occupying precisely the same relation to the orificium urethræ, the bladder and the rectum, as the vagina in the female, is particularly elucidative; and among many other facts, for which we are indebted to embryological research, further corroborates the conclusion of the most scientific anatomists of the present day, that every variety of so-called hermaphroditical malformation is referrible to an abnormal condition, either of the male or of the female organs, existing singly, and but rarely conjointly in the same individual. As a proof of the necessity of a close application of the laws of development in the investigation of these cases, we may mention that examples, like that of Soemmerring, have to our knowledge been mistaken at no distant period for females. It would not indeed be difficult to show by a reference to cases and to the discussions of medical societies, that there is no branch of developmental anatomy which is so little understood by the majority of practitioners, as that which relates to the subject under consideration; and with this conviction, we recommend the very valuable paper of Professor Theile to the general attention of our readers.—*Müller's Archiv für Anatomie*, 1847.

Angioleucitis and Phlebitis. By M. Velpeau.

In both cases red streaks or lines exist, but in *phlebitis* beneath these are voluminous, indurated cords. These are not present in *angioleucitis*, because the vessels are small and do not become indurated. This had led me to lay down as an aphorism; Angioleucitis is seen but not felt; while phlebitis is felt rather than seen: so that the affections might be distinguished even with closed eyes.—*Gaz. des Hôp.*, No. cxxvii.

MEDICINE.

On the Employment of Large Doses of Tartar Emetic in the Treatment of Pneumonia, especially in Children. By DR. HERARD.

DR. HERARD observes, that although it is now universally allowed that doses of tartar emetic, which toxicologists heretofore pronounced poisonous, may be administered with safety, yet the greatest discrepancy of opinion prevails as to the amount of benefit derivable from these, notwithstanding the question has received so much illustration from very able writers. Having had the opportunity of witnessing the treatment of pneumonia by tartar emetic, at the Hotel-Dieu by M. Sandras, and at the Hospice des Enfans by MM. Baudelocque and Blache, and this in a great number of cases, he believes that the results may be advantageously published, especially as the antimony was the only remedy employed. He does not agree with those who consider that because we have so powerful a therapeutical agent as bleeding, inquiries like these are superfluous; for not only is it desirable to possess more than one efficacious means, especially if their conjoined use is found to diminish the chance of death and hasten convalescence; but there are cases in which bleeding cannot be performed without danger, as in ataxic pneumonia, particularly that of drunkards, and in pneumonia which manifests itself in persons already exhausted by prior diseases, or by being surrounded with anti-hygienic circumstances. Children, too, at least those who are found in hospitals, ill bear bloodletting even in its mildest form.

Dose and mode of administration. In the adult, the dose has been rapidly raised from five or six to ten grains, but never carried beyond this; while in children, commencing with one and a half or two grains, it has rarely reached five grains. It has always been administered in a julep, giving a spoonful or two every hour or every second hour, suspending it for two or three hours after meals, and preventing the patient drinking too abundantly of ptisans while taking it. With such precautions the medicine becomes easily tolerated, the great error of practitioners being the administering it in too large a quantity of fluid. Moreover, a small proportion of syrup of poppies or gummy extract of opium is always added to the julep.

Action on the economy. 1. *On the alimentary canal.* The author's investigations in no wise confirm the assertions of those, who state that inflammation of the bucco-pharyngeal mucous membrane is a common result of the employment of large doses of antimony. In four or five cases only out of sixty in which these were given did he meet with a slight aphthous ulceration, apparently resulting from the child's retaining the julep in the mouth before swallowing it. In seven only of thirty-one infants did the tartar emetic produce repeated vomiting and purging; in twelve the tolerance became established by the second day; while in the rest it was observed from the beginning. As a general rule, tolerance is more rapidly and durably established as regards the stomach than the intestines. Diarrhœa is, however, seldom increased by the antimony, and in some cases obstinate constipation follows its use. When tolerance is of somewhat difficult establishment, it seems to be more readily brought about by increasing the dose and the frequency of administration, than by a contrary proceeding. After the fever has subsided and the medicine is only continued because of the persistence of some local symptom, tolerance is still maintained. Some patients even take it at their meals, and in others it seems to excite the appetite.

2. *Circulation.* All observers agree that antimony exerts a marked effect in diminishing the number of the pulse. This, in the adult, rapidly falls from 105 to 80, 70, 60, or even 45. In the child, the diminution is less marked and less permanent, the pulse still retaining somewhat of the oscillatory character proper to the time of life. This effect upon the pulse cannot be said to result simply from the ameliorated state of the patient, since it is observed the very next day after giving the antimony, and during the existence of very grave pulmonic lesions ascertainable

by the stethoscope. More than once a pulse which had descended to 50 has risen to 70 on discontinuing the antimony, again to fall upon its resumption. This effect upon the pulse is usually more marked in proportion to the ease with which tolerance is established. Simultaneously with the diminution of the number of the pulse there is sometimes, but not always, an enfeeblement of the arterial and cardiac pulsations; but the intermissions and irregularities mentioned by Trousseau have not been observed by the author. The effect upon the pulse is sometimes long in disappearing, this continuing slow for several days after the suspension of the medicine.

3. *Respiration.* This, as well as the pulse, has almost always been found slower than natural; but its condition is much more difficult of exact appreciation, especially in the child. It is best examined during sleep, and then is found sometimes to have become affected even by the second dose.

4. *Secretions.* Transpiration has not seemed either in the adult or child to be increased by large doses of antimony; nor has the increased secretion of urine, noted by some authors, been observed.

Mode of Action. Many excellent observers explain the curative agency of antimony in pneumonia by the powerful revulsion it produces upon the alimentary canal; and M. Chomel attributes some of the advantage to the mechanical effect which the repeated vomitings exert in disgorging the lungs. But if these explanations are correct, the medicine should be given in smaller doses, for these exert a far more powerful effect upon the alimentary canal than the larger ones; and every emetico-cathartic medicine might be expected to operate as beneficially. In fact, the most marked benefit is derived from the use of antimony, just in those cases in which its complete toleration prevents any effect being produced upon the stomach or intestines. According to others, it determines towards the skin the morbid material which had become concentrated upon the internal organs; but the slight amount of increased transpiration ill accords with this explanation.

The rapid and marked depression of the circulation and respiration has been differently explained. Some, as Mialhe, have seen in it a chemical action by which the oxidizing power of the lungs upon the blood becomes diminished; while others, as Trousseau, believe that the antimony admitted into the circulation acts directly through the nervous system upon the heart and inspiratory muscles. The lung consequently receives in a given time a less quantity of blood, and has, as an instrument of hæmatosis, less blood to elaborate; and it is to this repose of the diseased organ that we may refer the resolution of the disease. To this it may be objected, that antimony is as efficacious in some other inflammations, as rheumatism. The author of the present paper is not disposed to add to these hypotheses; but observes that, however explained, the diminution of the number of the pulse is, in some measure, the index of the beneficial effort of the remedy. When this is marked and rapid, a cure is almost always certain.

Therapeutical effects. The antimony, in the present series of observations, was administered to nine adults and thirty-one children. Of the former, one patient, admitted in an advanced stage of the disease, died; and the rest were completely cured. Of the children, seven died, two falling victims to tubercular pneumonia. In the other five, lobular pneumonia was very extensive; and five in thirty-one is a very favorable proportion in a disease so fatal as is the pneumonia of young children in hospital. Lobular pneumonia is far less amenable to tartar emetic than the lobar; and those cases of broncho-pneumonia, as M. Guersent terms them, occurring in children under two years of age, are generally far more efficaciously treated by emetics than by antimony given in contra-stimulant doses. For the same reason, probably, the Rasorian method is less successful in aged persons than in the adult; for it is well known that at this period of life pneumonia is complicated with bronchitis, and the characteristic tubular *souffle* is often absent. So, also, in pneumonia succeeding to bronchitis, M. Sandras has found antimony far less efficacious than in cases of pure pneumonia. The most remarkable fact among the present cases was the rapid disappearance of the bronchial *souffle*, and its replacement by crepitant or sub-crepitant *râles*. Bleeding itself does not produce so rapid a passage from the second to the first stage of the disease. "Frequently on the day of commencing the antimony, bronchial respiration, with bronchophony and resonance of the cough over the half of a lung, were ascertained; and the next day *râles*, more or less fine, might be heard over this entire space. Had this phenomenon been met with only once or twice, we should have explained it by the undoubted changeableness of the stethoscopic signs in childhood; but the result was obtained in so many children, that it would have been impossible not to have referred it to the antimony, if even it had not been equally exhibited in the adult." The crepitant *râle*, on the other hand, persists with remarkable tenacity, and, becoming assimilated in character to the large, moist, sub-crepitant *râles* of chronic or subacute bronchitis, may extend far into convalescence, every other sign of returning health being present. The rapidity with which convalescence takes place is, indeed, one of the most remarkable and advantageous results of the use of antimony. It can hardly be said to exist, so rapidly does health return, giving to this method of treatment an infinite advantage over bloodletting in this respect.

Notwithstanding his conviction of its great efficacy, the author would not recommend tartar emetic as an exclusive remedy, believing, as a general rule, that the diseases in adults are most advantageously treated by combining it with venesections. It enables us to do with fewer bloodlettings, while these favour its absorption. But, to render it of service, practitioners must be convinced of its therapeutical importance, and prescribe it from the commencement, not as a mere accessory, but as an heroic remedy. In children *above two years of age*, it should be regarded almost as an exclusive medication: although, in exceptional cases, one or two emissions of blood or a blister may be required. This last, sometimes, however, often proves a dangerous remedy in hospitals.—*L'Union Médicale*, Nos. 127, 128, 129, 130, and 131.

On the Employment of Morphia in Chronic Gastralgia. By M. Valleix.

"It is now generally acknowledged," M. Valleix observes, "that almost all the chronic gastralgiæ are produced or kept up by a too debilitating treatment and regimen. If at the end of acute diseases, actively treated by antiphlogistic measures, we prevent the patient for too long a period from returning to his ordinary food, his stomach will suffer. This is often supposed to arise from the return to too substantial aliment, whereas, in fact, this is too little nutritive; and if the amount be still more diminished, the gastralgia increases, and the ingestion of food is followed by pain, epigastric distension, vomiting, &c. At other times the chronic gastralgia may follow the acute form, or may complicate leucorrhœa, anæmia, or chlorosis. In these latter cases, however, it is the original disease rather than the gastralgia that has to be treated."

In the treatment of chronic gastralgia, M. Valleix has derived the greatest benefit from the employment of *small doses of acetate of morphia.* The medicine is not a new one in this disease; but M. Valleix has advantageously modified the usual mode of its administration. Thus, instead of giving it before, he prescribes it immediately after, a meal; and in this way he has relieved cases which had resisted all other treatment. A very well-marked one of this description is detailed. M. Valleix orders 1 grain of the acetate in 30 drachms of distilled water, and 9 drachms of syrup, and directs a teaspoonful to be taken immediately after each meal. Under the use of these small doses, the bowels, so far from becoming constipated, are better regulated.—*Revue Médico-Chirurgicale*, tom. ii, p. 100; from the *Bulletin de Thérapeutique.*

The same journal (p. 163) gives some account of the results of a series of experiments conducted during the last eleven years by M. Lafarrue, upon the *inoculation of morphia* and other medicinal substances, as a substitute for the ordinary endermic method. The morphia is moistened into a thin paste, and inserted under the epidermis with a lancet. A small pimple with an areola is produced by each puncture, but disappears within twelve hours. The means is especially indicated for the production of local sedative action, as in the various neuralgiæ; a twig of nerve, however small, being thus easily pursued. By the employment of less than a grain of morphia, general effects are produced, but some twenty or thirty punctures are then required, the lancet being dipped again into the paste after each.

Internal Use of Ice in Exhausting Diseases.

SOME interesting cases are quoted in a recent number of the 'Revue Médico-Chirurgicale' from a French journal, in which ice taken internally seemed to be of great service in reviving powers fast sinking. The writer employs it in very various diseased conditions, providing these manifest the signs of intense debility. The reaction it induces may prove curative in some cases, while in others, in which this is impossible, a marked temporary amelioration of the patient's state may occur. In the cases in question, there is great atony and extenuation, and an extreme aversion for every article of food whatever, with or without a development of muguet. A number of morbid states and organic lesions, having no other points in common, may induce this condition. Iced water does not succeed anything like so well as the administration of the ice in little lumps, which, by requiring time for their solution, ensure its gradual introduction. These impart great tone to the system, and revive the inclination for food in a remarkable manner.—*Rev. Méd. Chir.*, tom. ii, p. 168.

On the Employment of Mercury in the Stage of Effusion in Acute Hydrocephalus.

By Professor GOLFIN, of Montpellier.

IN this paper M. Golfin relates some cases of acute hydrocephalus arrived at the second, or even third stage, and apparently become desperate, relieved by the free use of mercurial ointment. We notice the communication, because we think the practice is too prevalent of abandoning these cases as hopeless, after the usual remedies have been found unavailing, and when, perhaps, perseverance under apparently hopeless circumstances, might not be unfrequently crowned with success. Three cases are here related, in which, after all other means have failed, the patients were saved by the free employment of a large quantity of mercurial ointment. In one case this was continued for eleven days, 184 grammes having been rubbed into the thighs and armpits in that period. Little or no effect was produced upon the mouth, but the author, in correctly laying down the general principle that pathological conditions induce a great toleration of remedies, and prevent the exertion of their physiological properties, does not seem sufficiently to bear in mind the power which children, under all circumstances, possess of resisting salivation, or rather the little aptitude they manifest of becoming thus affected by mercury.

Those who believe hydrocephalus to be an exclusively inflammatory disease, regard mercury as an antiphlogistic. But, in attributing to it *antiphlogistic* properties because it cures phlegmasiæ, we are only reproducing the faulty notions of the ancients, when characterizing the various pharmaco-dynamic agents. To designate the power these possessed over the diseases against which they were employed, names were given them, not representing their physiological or therapeutical properties, but the results of such properties; and thus the denominations *anti-epileptic*, *anti-hectic*, &c. were attached to the agents by means of which relief had been obtained in the diseases whose names they bore, however different the vital condition of the living aggregate might be in different cases, even of the same disease. In the same way, to term mercury an antiphlogistic, is to designate the result of its therapeutical property, but not the property itself. Indeed, absolutely speaking, the only antiphlogistics are acidulous drinks, for they alone can combat the inflammatory element in a direct manner. "We regard mercury as an empirical and perturbatory remedy. By its stimulant property it deranges the vital and organic forces, decomposes the elementary constitution of the phlegmasiæ, and separates its elements. Amidst all this perturbation, the *vis medicatrix* dissipates these, and reestablishes the normal state." It likewise operates beneficially in the stage of effusion of hydrocephalus, by stimulating the absorbents.—*Revue Médico-Chirurgicale*, tom. ii, pp. 134–44.

SURGERY.

On Fracture of the Clavicle. By M. Velpeau.

Very unnecessary fears have arisen from the bone uuiting not quite regularly. It is true it cannot always be effected without some slight deformity, but this is of no consequence in men, and even in women is only seen in such as are of spare make. This need not take place when the fracture is situated in the external third of the bone, the fragments being maintained *in situ* by the ligaments and muscles, so that a bandage is not even necessarily required. When the fracture occurs within the inner two thirds, there is always some displacement in the adult, although this does not take place in very young children. The most complex apparatuses are in no wise preferable to the following simple plan of treatment. A bandage is carried from the armpit of the sound side across the back and shoulder to the fractured clavicle. The patient's hand is brought up to the sound acromion, so as to raise the elbow as high as the sternum, the shoulder being thrown backwards and upwards. While an assistant holds the limb, the bandage is repeatedly passed over the anterior part of the arm, and brought round by the sound armpit; and over this is passed one well moistened with dextrine, so as to produce an inflexible mould. The bandage need not be put on for four or five days after the accident, and in from a week to a fortnight the fracture will be sufficiently firm to allow of its removal. It is an error to suppose that a patient cannot raise his arm to his head when his clavicle is fractured. He believes he cannot, and is prevented from trying by the pain this causes. But if you insist upon it, and that not doubtingly, you will find he can accomplish it. I have not seen six exceptions in twenty years. —*Gaz. des Hôp.*, No. 115.

On the Employment of Inhalation of Ether in some Forms of Ophthalmia. By Dr. Mackenzie, of Glasgow.

A recent number of the 'Annales d'Oculistique' contains a short communication from Dr. Mackenzie; and as we believe he has not published it in any English journal, we present an abstract, confirmatory as it is of the statements previously made upon the subject by MM. Cunier, Alex, and others. Having convinced himself of the power of ether in preventing the pain of operations, Dr. Mackenzie determined to examine its capability of assuaging some of the more painful affections of the eye, in which intense photophobia is a prominent symptom. "I therefore employed it in a series of cases, both in my infirmary and my private practice, and with the most satisfactory results; I have prescribed it for scrofulous ophthalmia, corneitis, sympathetic ophthalmia, neuralgia of the branches of the 5th pair, and asthenopia; and have obtained some benefit from its use in all these diseases, but principally in the first three." Three cases are given in illustration. The first of these was an example of *scrofulous ophthalmia* of three months' duration, accompanied with great photophobia. Every variety of treatment had been tried, but the temporary amendment produced was always followed by relapse at no distant period. The immediate relief afforded to the photophobia by the ether was remarkable; the patient, who had shrunk from the least access of light, being enabled to open her eyes and guide herself from a room she had been accustomed to be led out of in darkness. The amelioration continued permanent, the photophobia not returning, although the inhalation was only resorted to twice, at an interval of some days. In a case of intense *corneitis* great pain and intolerance of light prevailed, and much opacity of the cornea existed. It was one of the severest cases Dr. Mackenzie had ever seen, and yet, prompt relief of the photophobia followed the use of the ether. This was resorted to several times, until tolerance of light had become quite established, and the transparency of the cornea somewhat restored. The third case was an example of the removal, by the same agency, of a disease which usually resists our ordinary means of cure, namely *sympathetic* *ophthalmia* of the one eye, succeeding to a wound of the other. In this instance, the pain and intolerance were intense, and the inflammation of the various textures of the eye had not yielded to mercurialization, when great and progressive relief was attained by the inhalation. It was employed at intervals of a few days during a month.—*Annales d'Oculistique*, tom. xviii, pp. 155-9.

On the Use of Ocular Douches for the Treatment of Purulent Ophthalmia of Infants, Opacities of the Cornea, &c. by M. Chassaignac.

M. Chassaignac has for the last six months employed irrigation of the eye for the treatment of the ophthalmia of young infants with the greatest success; so that while formerly blindness at the Foundling Hospital was constantly occurring from this cause, it is now seldom so produced there. The child is laid on a table, and water allowed to flow from a small tap through a tube over the surface of the eye during from 5 to 15 minutes several times a day. M. Chassaignac has discovered that in this disease a pseudo-membrane is frequently produced, the removal of which much expedites the treatment. The mortality of children suffering from disease of the eyes during the last ten years was 1 in 3; while since this plan has been adopted, it has been but 1 in 8. In the course of investigation, this means was found applicable to several other inflammatory conditions of the eye, and also especially for the removal of opacities of the cornea which resist ordinary means. Accounts of its really remarkable success in this last important application, have just been published by one of the assistants at the hospital.—*L'Union Médicale*, No. 140,

Punctures of the Scrotum in Hernia Humoralis. By M. Velpeau.

The little operation I practise almost entirely relieves the pain, and produces no inconvenience. I gently grasp the inflamed part with my hand, so that the thumb and index-finger may thrust the fluid which the hernia vaginalis contains towards the surface. I pass the lancet, held like a pen, perpendicularly into the most fluctuating portions of the tumour, so that its point may enter the tunica vaginalis, and in this way puncture two, three, or four times the portion held in my hand. Generally a little jet of fluid is discharged, and if any inflammation occur, a cataplasm is applied. In almost all the cases the pain and redness diminish at once, and the scrotum recovers its suppleness. These punctures may be made at any stage of the affection.—*Gaz des Hôp.*, No. 136.

MIDWIFERY.

A Case of Rupture of the Womb occurring during Labour and followed by Recovery. By Dr. Prassart.

Cases of recovery after rupture of the womb are of such rare occurrence, that we are desirous of recording the leading features of this one, the subject of which had to contend alike with her formidable accident and the neglect of her attendant. A woman, ætat. 37, of muscular make and very choleric temper, had been in labour for six or eight hours, on 1st of February, when, at about 4 p. m., on getting on to the bed she was seized with a tremendous pain, contemporaneously with which the waters were discharged, and a loud cracking sound was heard. She complained of terrible suffering at the umbilical region, and grasped this with both her hands. Labour-pain ceased, and she became ghastly and cold, so that her friends believed her in the act of dying, and had the religious sacraments administered. After a long period, however, they determined to call in advice, and about six hours after the accident the author saw her. He found her suffering from the extremest prostration and intense tenderness of the belly, through the parietes of which the parts of the child were plainly felt. He easily delivered her of a dead child by means of the forceps, a large discharge of blood following. He endeavoured to ascertain the size and position of the aperture, but could only discover that his hand at once passed into the cavity of the abdomen, whence he removed the placenta, and that large coils of intestine passed into the uterus, the great pain induced forcing him to desist. The woman, after this, seemed almost lifeless, and the author informed her friends that she could not live the night. With true Germanic phlegm, he seems to have taken no pains to ascertain whether his prediction was verified, and in *four days* after was much surprised at being again called to visit the patient. He now found severe inflammatory action of the womb and abdomen set up, accompanied by great prostration of strength. We need not follow the case through its remaining details, presenting, as it does, but another example of the occasional wonderful power which Nature employs in coping with the direst extremities of disease. The author, from his unfrequent visits (alternate days) and the nugatory character of his treatment, may be considered as having delivered it over into her hands, the result being, that in four weeks the woman was enabled to leave her bed. She continued for some time after her recovery to be tormented with occasional severe pains; but in the course of the following year natural menstruation was re-established.—*Casper's Wochenschrift*, No. xli, 1847.

Treatment of Ovarian Cysts. By M. Velpeau.

Women having these cysts live sometimes for 10, 20, or 30 years in good health; and supposing that upon an average there may be 15 years compatible with life, any dangerous operation, which if there were but 5 years might be taken into consideration, is out of the question. Yet is the belly opened and the tumour dissected out in these cases. The mortality is frightful after such an operation, more than half the patients succumbing, and young women in flourishing health being carried off in a few days. Rashness like this must be opposed to the utmost, the offspring as it is of folly. Happily for the honour of our country and our art, nothing of this kind takes place in France; but every year, or almost every month, the English, American, or German journals furnish accounts of these operations. Every one is performing them, and, strange to say, persons of confirmed reputation consent to them.—*Gaz. des Hôp.* No. xlix.

[We have extracted the foregoing passage from one of M. Velpeau's recent lectures, for the purpose of exhibiting the spirit of exaggeration which actuates this clever surgeon when treating of any practice he happens, and in this case from insufficient information, to be opposed to.]

FORENSIC MEDICINE.

On Poisoning by Arsenic. By MM. Orfila and Tardieu.

The October number of the 'Annales d'Hygiène' contains lengthened official reports of the medico-legal investigations which were rendered necessary by the murder of the Duchess de Praslin, and the suicide of the husband. The portion relating to this latter event contributed by MM. Orfila and Tardieu, contains some points worthy of notice.

On the evening of the 18th of August, the murder having been committed in the morning of that day, the Duke was seized with vomiting, accompanied by a very small pulse, and great exhaustion. This state continued the next day; and on the 20th, M. Andral was called in. He found him better, his powers having rallied, and his voice become firm. His aspect and respiration were natural, and he complained of no pain on pressure of the epigastrium. With these conditions, which seemed satisfactory enough, two remarkable symptoms were present, viz. such *extreme smallness of pulse* that it could hardly be felt, while it was also irregular, and an *icy coldness of the extremities.* M. Andral gave it as his opinion that, although these symptoms might result from the intense moral emotions to which the Duke had been subjected, they might also be due to the ingestion of poison; and advised that all matters evacuated in future should be analysed. It seems singular, indeed, that under the circumstances in which this man was placed, his having poisoned himself should not have been the first idea that presented itself to his attendant M. Reymond. In the evening some reaction took place, the pulse became developed and regular but frequent, and the hands regained some warmth. On the 21st, he was removed to prison, and in the evening his pulse again became filiform, and his extremities cold. Great constriction of the throat, intense thirst, and meteorism of the abdomen were present; but little or no urine or stools were passed, and the vomiting had not reappeared since the 19th. On the 22d, every symptom aggravated. The tongue and buccal mucous membrane intensely red, and a sense of burning extending from the mouth to the anus. Pulse very frequent and sometimes weak, sometimes strong; no vomiting; two stools from enemata, and hardly any urine. From this period the patient gradually sank, but lingered on to the 24th, his intellect remaining clear to the last.

At the autopsy, the appearances rendering poisoning probable were as follow: 1. The dissemination on the inner surface of the left ventricle of a large number of small hemorrhagic spots, produced by the effusion of blood beneath the endocardial membrane, some of these penetrating into the fleshy substance of the heart. 2. Within the large curvature of the stomach, seven large black eschars were discovered. They were clearly defined by a yellowish white border, the mucous membrane surrounding these being softened and of a deep red. The eschars did not involve the whole thickness of the walls of the stomach, and nowhere did perforation or ulceration exist. The intervening mucous membrane was sound. 3. At the upper portions of the duodenum and lower portions of the ileum, the mucous membrane was found of a deep red, inflammatory, but nowhere ulcerated. The other viscera were sound.

In reference to the *eschars*, M. Orfila remarks, it would be a great error to suppose they arose from the local action of the arsenic; they in fact resulted from the absorption of the poison, analogous alterations appearing in animals that have been poisoned by introducing arsenic into the subcutaneous cellular tissue at the inner part of the thigh. Such alterations are seen when the quantity swallowed has been considerable, and especially when death has been long delayed; for when this occurs promptly after the poisoning, not only is no eschar present, but scarcely even a slight inflammation.

The liver and intestinal canal, when submitted to analysis, afforded abundant proof of the presence of arsenic. M. Orfila makes some important remarks upon the different modes of operating on the liver by means of chlorine, according to the quantity submited to analysis; and in a note lays down some rules (p. 405) which the practical toxicologist will do well to consult.

To the first two questions put by the Chamber of Peers upon the *cause* of the death of the prisoner, and the *nature of the agent* by which it was effected, the answers given are obvious. "We had not," observes M. Orfila, "to determine what quantity might have been taken; for as a general rule, such a question ought neither to be put nor replied to. As in no case is it possible to know in what proportion the poison, disseminated throughout the body, exists in this or that organ; as the variations in absorption, the different modes of excretion, and the amount of evacuations, necessarily render the amount eliminated variable and indeterminable; and as, on the other hand, a portion of it is invariably lost in the operation of examining it, whatever process be followed, it is evident that the solution of this problem should never be sought, and that the only thing truly important, is the proof of the presence and the nature of the poison." (406.)

At what period was the poison swallowed? By a negligence almost incredible in such sharp-sighted persons as the Parisian police, it seems that although the clothes which the prisoner took off on his apprehension were carefully examined and put under seal, a dressing-gown which he desired might be given him in place of the one removed, was afterwards found to have had in its pocket a phial containing arsenic! Until four o'clock in the afternoon he was surrounded with persons, and no symptoms whatever of deranged health had yet manifested itself. At ten in the evening vomiting first appeared. Can we state the time which should elapse between the swallowing the poison and the manifestation of the symptoms? "There is nothing positive in this respect. Several circumstances may cause the limit to vary, and more or less retard the manifestation of symptoms. The form of the poison, the presence or absence of acid or other fluids in the stomach, the fulness or emptiness of that viscus, the rapidity or slowness of absorption, may accelerate or retard the action of the poison. In this case, the arsenic being taken in the solid form and in a state of coarse powder, and no great quantity of liquid being swallowed with it, might not reveal its symptoms until after a certain period. But, fixing the time as remote as possible, we can scarcely admit, except under some very peculiar circumstances, that the effects could be delayed beyond three or four hours." (p. 409.)

4. Is there anything in the symptoms of the present case to lead to the belief that another dose of poison had been taken at an epoch nearer death? No. The cessation of the vomiting on the second day might have led to the opinion that a real amelioration had manifested itself; but this was only apparent, other symptoms announcing the man to be still under the influence of the poison, such as frequent syncope, involuntary evacuations, feeble pulse, and excessive thirst. Many examples may be cited, of persons dying several days after ceasing to vomit, although no more poison had been taken. "It is very common, in fact," observes M. Andral, "to find in these cases the symptoms diminishing after the early vomitings, and then the poison by its presence in the economy determines new accidents, which result both from the progress of the inflammatory reaction, and from the dispersion of the arsenical molecules among the various organs."

5. *Has the action of the poisonous substance been interfered with or destroyed by any other agent?* "It is by no means impossible that a narcotic may in certain cases, by paralysing the absorbent action of the mucous membrane of the stomach, retard or even destroy the effects of a poison such as arsenic. We have shown, by recent experiments (Annal. d'Hygiène, tom. xxxviii, p.199), that by means of small doses of an opiate compound, we can thus diminish vomiting and retard its appearance, render the pains less severe, and prolong life." (p. 411.) There is, however, no reason to believe that the prisoner took laudanum or opium with the arsenic, the contrary indeed being proved by the symptoms before and the examinations after death.—*Annales d'Hygiène Publique*, tom. xxxviii, pp. 390-412.

This case is highly interesting in a toxicological point of view; and when we peruse the particulars of the remissness of the authorities in duly watching the prisoner, the extraordinary errors of the medical men in not suspecting and treating the case as one of poison, and the apparent temporary amendment which occurred, we cannot feel surprised at the reports which prevailed in the political circles respecting it.

MEDICAL STATISTICS.

A Statistical Notice of the Laws of Mortality and of Survivorship at different Ages of Human Life, deduced from 10,203 *deaths occurring in the Canton of Geneva during the eight years* 1838-45. By Dr. Marc d'Espine.

The author observes that perhaps Geneva is the only city in existence which can furnish, almost uninterruptedly, tables of its mortality, and the changes in probable and mean duration of life, for so long a space of time as 273 years,—viz. from 1560 to 1833. With the results obtained from these, as set forth in Mallet's 'Researches,' he compares in the present article those derived from his own observations, carried over a period of eight years. Although we cannot reproduce the tables in which these materials are exhibited, we may notice some of the conclusions the author arrives at.

Mortality of children. The mortality of the *first* year of life has been continually decreasing; or, in other words, the proportion who survive their first year has been increasing. Thus, the number of survivors per 1000 amounted to 740 in the sixteenth century; to 763, in the seventeenth; to 798, in the eighteenth; to 848, in the first 34 years of the nineteenth; and to 877, in the author's tables,—i. e. a mortality of 123 per 1000. From *one* to *ten years* of age the mortality in the canton of Geneva is very nearly the same as that of the first year, viz. 133. At the earlier period of the present century it was less still (128 and 120), so as exactly to compensate for the comparative greater infantile mortality. Going backwards from 1814, however, the mortality at both periods steadily increases; and thus with 744 survivors per 1000 in 1838-45, we find but 643 in 1761-1800; 601 in 1701-61; 524 in 1601-1700; and 480 in 1561-1600. It is remarkable that in the sixteenth century the number of survivors at one year was less considerable than is that now at ten years.

Mean life. The mean life, *in general*, is the sum of the years lived by a total of individuals supposed to be born together, divided by the sum of the births. According to the author's tables, it was 41·7 years for 1838-45, while it was but 38 years for the first fourteen years of the nineteenth century; 33·1 for the latter half, and 31·8 for the first half, of the eighteenth; 22·8 for the seventeenth; and 18 for the latter half, of the sixteenth. The increase from 1560 to the beginning of the nineteenth century has thus become more than double; but during the last thirty years it seems to have attained its highest point, and to be insusceptible of further augmentation. In fact, the city of Geneva furnished for the years 1816-30 the same mean life as in the years 1838-45. Considered relatively to *different ages*, the mean life is the quotient resulting from the division of the sum of years which individuals of a certain age have lived by the sum of the individuals of that age. In the author's tables it increases from birth to between three and four years, being 47·9 at four years. This maximum attained, it uniformly decreases to advanced age. The maximum was attained at three years, also in the fourteen first years of this century; but in the two preceding centuries and the latter part of the sixteenth, the increase continued to the fifth year.

Probable life. The term is, *in general*, understood to indicate the age at which the half of those born in the same year are dead, while the other half survives. This, according to the author's tables, was 43·62; while in 1801-14 it was 40·68; in 1760-1800, 32·37; in 1700-60, 27·18; in 1600-1700, 11·61; and in 1560-1600, 4·88 years. Thus, the probable life, which in the sixteenth century was four times less than the mean, has from that time become progressively increased, and that more rapidly than the mean life; so that by the end of the seventeenth century these two lives were nearly equal, while from that time to the present the probable life has been maintained some years above the mean life; but, like the latter, it seems to have reached its highest point, and, during the last thirty years, to have somewhat sank.

Influence of sex. The tables published in various parts of Europe all agree in showing a preponderance of *male births;* and of the 10,761 here examined, 5483 were male, 5278 female,—i. e. 1000 to 963; and yet, as in the rest of Europe also, the female population exceeds the male. It did so in the canton of Geneva, in the proportion of 1000 to 926, at the census of 1843. In early infancy the causes of death especially attack males. From that time to a little beyond puberty, but without following this law regularly from year to year, it falls on the female rather more than the male, but much less markedly than in the case of the earlier predominance in the male. From ten towards old age, more men than women die; whilst from old age to the end of life this is the case with women. The above statement applies to all countries that have published their vital statistics, and only differs in any of them as to the precise duration of the several periods. In our author's tables the male mortality predominates until near the eighth year, from which to the nineteenth more females perish. From 20 to 60, men again acquire the ascendancy; of the forty years constituting this period, there are 27 in which the male deaths predominate, and 13 in which those of either sex are equal. From 60 to the end of life, females maintain the ascendancy. At all epochs of human life, in an equal number of births of each sex, more women than men *survive*, except at the first year, when the numbers are equal. M. Quetelet has found likewise, in the Belgian and Parisian tables, that the excess of male births is almost entirely balanced by the excess of male mortality during the *first* year, while the tables for 1841 show that in England the equilibrium is attained even somewhat before that period.

According to the author's tables, the *mean* life of women, setting out from birth, is 3·5 years superior to that of men; and 2 years, according to the five English reports. It is at birth that this differs most in the sexes, after which they tend to approach each other, until an equality is attained at an advanced age (62 in the author's tables, 93 in the English). The difference of the *probability of life* at birth is 6·72.

Longevity. The author regards the proportion of persons reaching advanced age as affording a better test of the *vital force* of a country than its proportionate mean life. He furnishes a table, from which it appears that, while his returns for 1838–45 furnish, in the 1000 deaths of all ages, 238 aged 70 and above, 86 aged 80 and above, and 8·1 aged 90 and above, the English report gives only the numbers 143, 59, and 8·4, and M. Quetelet's table only 170, 58, and 7. He terms the age of 70 the *common age of longevity*, as being that which every person not cut off by disease or accident may calculate on reaching; while he fixes the age of *exceptional* longevity, it is not easy to say why, at 90. Mallet and others have stated that examples of exceptional longevity become rare in a population, in proportion as its mean longevity is augmented,—a proposition to which the author demurs, showing that the number of nonagenarians has regularly increased at Geneva. Thus of 1000 births there survived to the age of 90, 2·05 in 1560-1600, 4·41 in 1700-60, and 5·18 in 1801-13. As respects ultranonagenarians, however, little variety is found in any age or country: 1·54 in the 1000 reached 95 years in 1560-1600, and 1·10 in 1801-13.—*Annales d'Hygiène*, tom. xxxviii, pp. 289-320.

BOOKS RECEIVED FOR REVIEW.

A Practical Treatise on the Causes, Symptoms, and Treatment of Spermatorrhœa. By M. Lallemand. Translated and edited by Henry M'Dougal. London, 1847. 8vo, pp. 333.

Practical Observations on certain Diseases of the Chest, and on the Principles of Auscultation. By Peyton Blakiston, M.D.F.R.S. London, 1848. 8vo, pp. 368.

Will be reviewed in our next.

Observations on some of the Parts of Surgical Practice; to which is prefixed an Inquiry into the Claims that Surgery may be supposed to have for being classed as a Science. By John P. Vincent. London, 1847. 8vo, pp. 364.

Will be reviewed in our next.

On Poisons in relation to Medical Jurisprudence and Medicine. By Alfred S. Taylor, F.R.S. London, 1848. Fcap. 8vo, pp. 835.

Will be reviewed in our next.

A Guide to the Examination of the Urine in Health and Disease, for the Use of Students. By Alfred Markwick. London, 1847. 8vo, pp. 155.

Contributions to the Pathology and Practice of Surgery. By James Syme, F.R.S.E. Edinburgh, 1848. 8vo, pp. 336.

Will be reviewed in our next.

An Essay on the Diseases of the Jaws and their Treatment. By Leonard Koecker. New edition, with copious Notes, and an Appendix, containing Tables of upwards of Three Hundred Cases. By J. B. Mitchell, M.D. London. 1847. 8vo, pp. 95.

Portraits of Diseases of the Skin. By Erasmus Wilson, F.R.S. Lond. 1848. Folio. 4 Plates, pp. 10.

On Ringworm; its Causes, Pathology, and Treatment. By Erasmus Wilson, F.R.S. London, 1847. Post 8vo, pp. 102.

Illustrations of Instinct, deduced from the Habits of British Animals. By Jonathan Couch, F.L.S. London, 1847. Post 8vo, pp 343.

A Treatise on Diet and Regimen. By William H. Robertson, M.D. Fourth Edition, rewritten and much enlarged. Vol. 1, and Part 1 of Vol. II. London, 1847.

Will be reviewed when complete.

An Experimental Inquiry into the Cause of the Ascent and Descent of the Sap, with some observations upon the Nutrition of Plants, and the cause of Endosmose and Exosmose. With two Plates, by G. Rainey, London, 1847. 12mo, pp, 47.

On Indigestion; its Pathology and Treatment by the local application of uniform and continuous Heat and Moisture. With an account of an improved mode of applying heat or cold in irritative or inflammatory diseases. By James Arnott, M.D. London, 1847. 8vo, pp. 107.

Will be noticed in our next.

Researches into the Pathology and Treatment of the Asiatic or Algide Cholera. By E. A. Parkes, M.D. London, 1847. 8vo, pp. 250.

Lectures on the Physical Phenomena of Living Beings. By Carlo Matteucci. Translated under the superintendence of Jonathan Pereira, M.D., F.R.S. London, 1847. Post 8vo, pp. 435.

The Pocket Formulary, and Synopsis of the British and Foreign Pharmacopœia; comprising standard and approved formulæ for the preparations and compounds employed in medical practice. By Henry Beasley. Fourth Edition, corrected, improved and enlarged. London, 1848. 16mo, pp. 455.

An Essay on the Use and Abuse of Restraint in the Management of the Insane, including some remarks on the origin and nature of their disease. By Hamilton Labatt, A.B. T.C.D. Dublin, 1847. 8vo, pp. 76.

Medico-Chirurgical Transactions, Vol. XXX. London, 1847. 8vo, pp. 252. With Six Plates.

Will be reviewed in our next.

Guy's Hospital Reports. Second Series. Vol. V. London, 1847. 8vo, pp. 212. With Plates.

Cosmos: a Survey of the General Physical History of the Universe. By Alexander von Humboldt. Vol. II, Part 1. London, 1847.

Will be reviewed when complete.

Deafness Practically Illustrated; being an exposition of original views as to the causes and treatment of diseases of the ear. By James Yearsley. London, 1847. 12mo, pp. 181.

Will be noticed in our next.

On the Inhalation of the Vapor of Ether in Surgical Operations; containing a description of the various stages of Etherization. By John Snow, M.D. London, 1847. 8vo, pp. 88.

Elements of General and Pathological Anatomy, presenting a View of the Present State of Knowledge in these Branches of Science. By David Craigie, M.D., F.R.S.E. Second Edition, enlarged, revised, and improved. London, 1847. 8vo, pp. 1072.

Will be reviewed in our next.

A few Remarks on the Expectant Treatment of Diseases. By ΑΚΕΣΤΗΣ. London, 1847. 8vo, pp. 16.

The Stars and the Earth; or Thoughts upon Space, Time, and Eternity. Part II. London, 1847. 12mo, pp. 59.

Will be noticed in our next.

The History, Diagnosis, and Treatment of the Fevers of the United States. By Elisha Bartlett, M.D. Second Edition. Philadelphia, 1847. 8vo, pp. 547.

Elements of Physiophilosophy. By Lorenz Oken, M.D. Translated from the German by Alfred Tulk. London, 1847. 8vo, pp. 665.

Will be reviewed in our next.

Account of a New Anæsthetic Agent as a Substitute for Sulphuric Ether in Surgery and Midwifery. By J. Y. Simpson, M.D., F.R.S.E. Edinburgh, 1847. 8vo. pp. 23.

A Popular Essay on Anæsthetic Agents for procuring Painless Operations, particularly on the Action and Effects of Chloroform in Surgery and Midwifery, but more especially in Dental Surgery. By William H. Mortimer. London, 1847. 8vo. pp. 32.

Some Remarks on the Value and Necessity of the Numerical or Statistical Method of Inquiry as applied to Various Questions in Operative Surgery. By James Y. Simpson, M.D., F.R.S.E. (From the Monthly Journal of Medical Science.) Edinburgh, 1847. 8vo. pp. 20.

An extremely valuable paper.

Abstract of the Proceedings of the Obstetric Society of Edinburgh, for Session VI. Edinburgh, 1847. 8vo. pp. 22.

The Cholera not to be Arrested by Quarantine; being a Brief Historical Sketch of the Great Epidemic of 1817, and its Invasions of Europe in 1831-2 and 1847; with Practical Remarks on the Treatment, Preventive and Curative, of the Disease. By Gavin Milroy, M.D. London, 1847. 8vo. pp. 51.

Account of a Case in which Two Fœtuses were United at the Sternum, with only one Liver, and one common Heart. By R. N. West. (From the Edinb. Med. and Surg. Journ., Oct., 1847.) 8vo. pp. 12. With a Plate.

A very interesting monstrosity.

Ventilation Illustrated: a Tract for the Schools of Rich and Poor. London, 1848. 12mo. pp. 36.

Admirably suited to its purpose, and deserving universal attention.

Account of an Alveolar Hemorrhage Compress, constructed by R. Reid, M.D. (From the Edinb. Monthly Journal of Med. Science, Jan., 1847.)

An ingenious and simple apparatus for applying graduated pressure in cases of alveolar hemorrhage without pain or inconvenience, and without impeding the actions of speaking and swallowing.

Speech of the Count de Montalembert, in the House of Peers of France, June 5th, 1847, on the subject of Medical Reform. With Notes of the Translator. London, 1847. 8vo. pp. 32.

Introductory Lecture read to the Medical Classes in King's College, London, October 1, 1847. By George Budd, M.D., F.R.S. London, 1847. 8vo. pp. 24.

An Introductory Lecture delivered at the London Hospital Medical School at the Commencement of Session 1847-8. By George Critchett. London, 1847. 8vo. pp. 36.

Observations on Pleuritis and Empyema in Children. With Cases. By Francis Battersby, M.B., T.C.D. (From the Dublin Quarterly Journ. of Med. Science, No. VIII.) Dublin, 1847. 8vo. pp. 27.

On the System of the Great Sympathetic Nerve, Part V. By Charles Radclyffe Hall, M.D. (From the Edinb. Med. and Surg. Journ., Oct., 1847.)

Will be reviewed, with the preceding parts, in an early number.

Tables for Students. 1. The Elements, with their Chemical Equivalents and Symbols. 2. The Vegetable Kingdom. 3. The Animal Kingdom. 4 Classes and Orders of the Vertebrata. By William E. C. Nourse. London, 1847.

May be useful for reference.

The Principles of Nature, her Divine Revelations, and a Voice to Mankind. By and through Andrew Jackson Davis, the "Poughkeepsie Seer" and "Clairvoyant." (Reprinted from the American Edition.) London, 1847. Two vols. 8vo. pp. 782.

Journal of Public Health, and Monthly Record of Sanitary Improvement. Published under the Sanction of the Metropolitan Health of Towns Association. Edited by John Sutherland, M.D. Nos. I. and II.

We trust that our readers will aid in the diffusion of this journal, as it is admirably calculated to give efficient assistance in the great cause of sanitary reform.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1848.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

1. *Quarterly Returns of the Health and Mortality in 117 Districts of England, for the Quarters ending March 31st, June 30th, September 30th, and December 31st,* 1847. Published by Authority of the Registrar-General.
2. *On Famine and Fever, as Cause and Effect, in Ireland; with Observations on Hospital Location, and the Dispensation in Out-door Relief of Food and Medicine.* By D. J. Corrigan, M.D., M.R.C.S.E.—1846. 8vo, pp. 34.
3. *Observations on the Connexion between Famine and Fever in Ireland and Elsewhere.* By Henry Kennedy, A.B., M.B.T.C.D.—1847. 8vo, pp. 50.

Some months ago it was well observed by a writer in the 'Times' newspaper, that "the great Irish famine and pestilence will have a place in that melancholy series of similar calamities, to which historians and poets have contributed so many harrowing and touching expressions. Did Ireland possess a writer endued with the laborious truth of Thucydides, the graceful felicity of Virgil, or the happy invention of De Foe, the events of this miserable year might be quoted by the scholars for ages together with the sufferings of the pent-up multitudes of Athens, the distempered plains of northern Italy, or the hideous ravages of our own great plague." And, truly, the last year's epidemic, with its causes and its consequences, whilst furnishing copious material for the disquisitions of the poet and the historian, supplies topics of consideration also to the philosopher and the man of science; and most particularly, to the members of our own profession. The connexion of the late pestilential visitation of fever with the antecedent and concomitant famine, the absolute and relative extent of its ravages, and the light that it may have shed upon pathological speculations, are subjects which are all of deep interest in themselves, and of profound and surpassing importance to the well-being of mankind.

Fever has at all times engaged the closest attention and consideration of the medical observer. From the earliest periods down to our own times, it is a subject which has formed the most constant theme of inquiry; and, regarding its causes, its essence, its general characteristics, and its varieties, speculation has ever been busy. Of late years, pathologists have occupied themselves for the most part in exploring the resultant organic lesions as revealed by post-mortem examination; and, in this way, they have brought to light a great deal of valuable information, of a kind, too, that is susceptible of practical application in the treatment of the disease. It must be admitted, however, that, coincidently with these later researches, there has not been a corresponding amount of activity in the investigation of the causes that determine the origin of fevers, or of the probable laws that regulate the development and the progress of epidemics. There has been, up to a very recent period, a too ready disposition upon all hands to rest upon the general notions to which our predecessors had been led, and to settle the complexion of facts according to their degree of correspondence with preconceived opinions. Undoubtedly very much of all this is attributable to the comparative immunity which the nations of western Europe have enjoyed for more than a century from severe epidemics of fever. *Intermittents* have now-a-days become, to the profession at large, materials rather for medical history than for medical observation; it is a rare fact, in many places, to meet with a case. And if we take *continued* fevers,—although we observe them to be always more or less prevalent, we are yet struck with their habitual mildness in ordinary circumstances, and with the length of intervals at which, in this country at least, they exhibit themselves severely in the epidemic form. England, especially, has been favored in this respect; for the large towns of Ireland and Scotland have abounded much more in fever, and in recurrent epidemics of it, than those of our own country. Lately, however, certain parts of England itself have experienced a prevalence of fever so serious, to an extent so frightful, and with consequences so destructive, that nothing which has happened of a like character within the present generation is to be compared to it. The disease, in some places, has raged in a ten-fold degree, as contrasted with its ravages in ordinary epidemics; and the general mortality in localities, where its devastations have been the most remarkable, has, in some instances, for a season been doubled and even trebled. Medical men and clergymen, whose benevolent functions have brought them largely into communication with the sick, have contracted the malady, and fallen victims to this pestilence to an extent probably without parallel. Altogether, the late visitation is calculated, in an unusual degree, to challenge the most serious attention and the deepest consideration of the members of our profession; and in this view of the case, we avail ourselves of the occasion to bring the subject of fever before our readers, and more particularly such points of it as may, in any way, have been elucidated by recent occurrences.

We conceive ourselves to be in a position, that ought to qualify us for the task which we undertake. We have had excellent opportunities of watching the rise and progress of the epidemic, in places where it was most rife; weeks and months have been passed by us in tracing its features under all sorts of circumstances. We have visited patients in their own hideous dens, and we have treated them extensively in hospital; we have studied the disease when, unavoidably, its course was undisturbed by art, and we have watched the effects of interference. We have seen and treated it in private practice; and, in several instances, the patients were our professional brethren whom the contagion had smitten. Lastly, we have suffered severely from the malady ourselves, and so, in the absence of positive delirium, have been in some degree able to trace its manifestations in our own subjective experience. We only mention these things to indicate the circumstances that have induced us, in an especial manner, to observe, to reflect, and finally to write; and because they furnish at the same time, some guarantee that, in any assertions or opinions we may hazard in the progress of this article, we do not speak without some sort of authorization.

There is no circumstance connected with the last year's epidemic more certain than that it was imported into this country by Irish immigrants. Fevers, differing in no appreciable feature from the one imported, had prevailed before, and at times epidemically; but that the visitation of 1847, in the cities and towns of England, was attributable to the pouring in of the half-starved Irish peasantry, there cannot be a doubt. The question of contagion will engage our attention as we advance. It is true that in the autumn of 1846, fever prevailed to a somewhat unusual extent in various parts of England; but there are many reasons for believing that such prevalence had no direct influence upon the epidemic of the following year. The fever that showed itself in the former period was, in a large proportion of instances, associated with dysentery and other abdominal derangements, and seemed to have some dependence upon the tropical heat of the summer that had just expired. It is a matter of somewhat constant experience, that autumns preceded by intense heat, very much abound in bowel complaints; and these at times, as the season advances, become associated with, or followed by, a low form of continued fever. This, however, does not usually display any fixed or well-marked characters; but declines and becomes substantially extinct, with the atmospheric and other causes apparently inducing it. Such seems to have been the case with many kinds of febrile attack in 1846. Whether it be right to rank continued fever so occurring in the same category with the epidemic fever of the last year, designating the disease by a common term *typhus*, we may inquire in the sequel. Meanwhile, we assert with confidence, that the excess of fever in the autumn of 1846, did not constitute the *foyer* from which sprung the fearful irruption of 1847; an assertion which will be rendered obvious by a very brief examination of some of the circumstances.

It will be within the recollection of those who have a moderately tenacious memory, or who have records of their observations, that in July, 1846, an inordinate amount of diarrhœa began to prevail; that, as the autumn approached, cases of dysentery and jaundice became very abundant; and that such affections, towards the end of August, were attended with febrile symptoms to a considerable extent. In the month of September the epidemic type of disease so far changed, that continued fever became a leading feature, and abdominal disorder by no means prominent. The ailments referred to seemed to be little guided in their course by circumstances of class, locality, or communication amongst individuals; all ranks, simultaneously, appeared to be more or less influenced by some general agency; it seemed as if atmospheric causes were operative mainly in production of the prevailing disease. The enteric derangement first became common, induced most probably by the excessive heat; and to these succeeded febrile disorders, chiefly in debilitated and exhausted frames. In some instances, such affections would assume the typhoid condition. A gradual decline, in all these respects, took place through the months of October, November, and December, so that in this latter month all excess of febrile complaints had disappeared. Such at least was the progress of events within the sphere of our own experience. We dwell somewhat upon these circumstances, because Dr. Kenedy, in the pamphlet, the title of which is hereunto prefixed, appears to consider the autumnal fever of 1846, as prevalent in this country, to have been identical in type with the "Irish fever" by which we were invaded in 1847. "Where is this?" he asks, in reference to the autumnal fever in question. "Is it in Ireland? Or is it in England, with its great wealth and well-fed people? It is in the latter country. I find from the periodicals that during the past year (1846) typhus has prevailed to a very considerable extent, and in different places." He then observes, speaking of Ireland, "from any inquiries I have been able to make about this country, the year 1846, taken as a whole, has been unusually healthy and free from fever." We may revert to the views of Dr. Kenedy concerning the origin and progress of epidemics, which doubtless have much in them that is just and well-considered; at the onset, however, we must express a dissent from the notion that ascribes identity of cause to the brief and slight epidemic of 1846, and the protracted and destructive one of 1847; and we will just observe, in passing, that febrile maladies, ensuing upon summers of tropical heat, in apparent connexion with abdominal derangement, must naturally be expected to prevail more largely amongst a "well-fed people" than amongst an impoverished one; and, further, that as some forms of fever, according to Dr. Kenedy's own showing, have an especial preference for the well-fed, and others for the ill-fed, an *à priori* probability becomes established that, in the two cases, they are improperly referred to a like epidemic constitution. But we return to our position, that the extraordinary amount of fever in this country during the last year, and the prodigious extent to which it enlarged the tables of mortality in many parts of England, were circumstances attributable in the first instance to the immigration of unusual masses of the Irish peasantry, bringing with them the germs of disease;—a position that will be demonstrated by certain facts which we shall now examine.

It is well known that Liverpool was the first place in this country that attracted notice by the coincident circumstances of Irish immigration and epidemic prevalence of fever, and that it was more severely afflicted in this point of view than any other town; Liverpool being exactly that particular port on our western coast, to which emigrants from Ireland systematically make,—whether their ulterior purpose be to extend their emigration to America, to advance into the interior of England, or to settle in the place itself. Indeed, the regular debarkation of droves of Irish has been a constant event in the annals of Liverpool. The excessive poverty of large numbers who have thus landed, has always been felt as a grievous burthen upon the poor-rates of that town; nevertheless, a certain average having, up to the last year, generally obtained in this respect, the evil from habit had become patiently endured, and had been but little thought of. We learn from a Report issued in the month of May, 1847, by Mr. Austin, Assistant Poor Law Commissioner, that, prior to January of that year, no very accurate notation had been made, either of the number of Irish poor casually relieved, or of the number of persons habitually arriving from Ireland. But "from the 4th of January," says Mr. Austin, "the accounts of the Irish relieved by the officers of the Select Vestry, showed a progressive increase in their numbers. On the 26th of January there were 25,313 persons relieved, being an increase of more than 20,000 in three weeks." The progressive advance of the numbers from Ireland, landing in Liverpool, will be seen from the following table, inserted by Mr. Austin in the Report just mentioned:

	From 13th January, 1847, to 13th February.	13th February to 13th March	13th March to 13th April.
Men	14,734	19,789	28,630
Women	8,249	10,042	16,505
Children	4,983	6,154	9,968
Total Persons .	27,966	35,985	55,103

The above figures represent a total of 119,054 arrivals in Liverpool from Ireland, in the short space of three months. Some of the immigrants, after a brief sojourn, proceeded to America, and others scattered themselves throughout the country; still, as the tide proceeded in an uninterrupted stream, it will be obvious that the worst-conditioned dwelling-places of Liverpool—already so badly eminent in this particular—must have become literally choked up by this excess of a vagrant population. Mr. Austin, writing on the 1st of May, says:

> "All the parts of Liverpool habitable by the Irish have been densely filled by them for some time. It is possible, however, that an increase has been gradually going on, up to the present time, and that even now the crowding of the dwellings of different descriptions, to which the Irish resort, has not quite reached its extreme limit. In the latter end of January, I personally inspected several streets, and the courts leading out of them, occupied by Irish, and everywhere the houses and cellars appeared to be full of inmates. In one small house I reckoned 41 lodgers. Since that time I have heard of instances of more excessive crowding."

What was naturally to have been anticipated, under such a state of things? Certainly, the irruption of disease in some form or other; more especially, as the immigrants commonly arrived in a half-famished state: for it is a sure fact that, whatever be the morbific effects of human *miasmata*, the evil becomes aggravated to the recipients, under circumstances of undue aggregation of individuals in a state of physical or vital exhaustion. We do not as yet come to the inquiry concerning the share which such conditions may have in the production of fever; our immediate purpose is but to deal with the fact; and certain it is that, towards the end of the month in which the debarkation of Irish peasantry became so formidable, fever of a peculiarly virulent character displayed itself; confined, in the first instance, to the immigrants themselves, but extending itself afterwards amongst all sorts of persons who had habitual communication with the sick, and that too, to an extent altogether without parallel in modern times. Liverpool, for a time, was like rather to a medieval city ravaged by pestilence, than to a town of the nineteenth century, from which art, based upon science, and applied in the architectural and other arrangements of cities, should be expected to have, in a great degree, banished such fell visitations. The justice of this representation will be apparent by attention to some of the subjoined accounts, statistical and otherwise, which we have selected from public documents, and in part obtained from other, more special, sources of information.

On consulting the Registrar-General's returns for the quarter ending March 31st, 1847, we find that an emphatic stress is laid upon "the disastrous effect of the immigration of the Irish poor on the health of English towns;" and reference is made to this circumstance, in explanation of the inordinate mortality characterizing the period to which the return in question relates. It is stated that the deaths of Liverpool, where the mortality has always been high, were 3068, or 1134 more than in the winter quarter of 1846, and nearly 1000 above the average of ordinary seasons. These figures, it is well to observe, do but refer to Liverpool proper—the *parish*—which, in 1841, had a population of 223,054. The following note from one of the District Registrars alludes to the unusual number of deaths in the following terms:

> "The return shows a very great increase in the mortality of this district, which is, without doubt, solely attributable to the many thousands of Irish paupers who have landed here within the last three months, bringing with them a malignant fever, which is here very properly called 'the Irish fever;' and many hundreds of them were suffering from diarrhœa and dysentery, when they arrived, which will account for so many deaths from these causes. Everything which humanity could devise, and money carry out, for their cases has been adopted by the Select Vestry; but so many thousands of Irish are continually pouring in, and their habits are so disgustingly filthy, that little can be done as yet to stay the great mortality among them. Perhaps there is not a parallel case to Liverpool for the last two months in the history of the country." (Quarterly Return, ut supra.)

The corresponding document of the Registrar-General for the quarter ending June 30th, 1847, indicates a great aggravation of the above state of things; thus the deaths registered in Liverpool for this quarter were 4809, against 2098 for the one ending June 30th, 1846,—an increase almost of 150 per cent. The intelligent District Registrar already quoted, again observes:

> "I have now to state that the same disease, 'the Irish fever,' has been raging among the poor with increased mortality. The numbers of destitute Irish who continue to arrive in this port, exceed those of last quarter; and they are still pouring in by every packet, to the great detriment of the health and business of the town. . . . . . The Select Vestry, after encountering almost insurmountable obstacles, have obtained from government the quarantine ships and one sixty-gun frigate, which they have fitted up as floating hospitals, to which the fever patients are removed, as far as they can be accommodated; and they also have fitted up spacious and commodious warehouses for a similar purpose; but still their powers and means are insufficient to cope with so formidable an antagonist. . . . . Eight Roman Catholic priests have fallen victims to their indefatigable attentions to the poor of their church, and one clergyman of the Church of England. Another nearly fell a sacrifice to the same disease. . . . . . . From ten to fifteen persons connected with the relieving department in the parish offices have also died of

fever, taken by them in discharge of their duties. The health of the English inhabitants continues good, indeed above the average, and the fever is at present confined to the Irish locality."

The fact, moreover, is adverted to by the Registrar-General, that already several medical men in attendance upon fever patients had been themselves attacked, and in some instances had fallen victims to the faithful discharge of their duties.

Mr. Austin, in the before-mentioned communication to the Poor Law Commissioners, furnishes a statement of the number of coffins supplied, in Liverpool, for the interment of sick paupers in the early months of the two years 1846-7; from which statement, the difference between the epidemic year and an ordinary one, is shown in another aspect: indicating, as it does, the class of persons amongst whom the special causes of mortality were chiefly at work.

COFFINS FOR DEAD PAUPERS.

	1846.	1847.
January	91	261
February	95	359
March	74	503
April	79	586
Total	339	1709

In the course of the summer, Dr. Duncan, already so honorably distinguished by his sanitary investigations, and who now holds the appointment in Liverpool of Officer of Health, presented to the Town Council of that borough a very interesting chart, which exhibited at a glance the mortality, during each week of the then current year, from fever—represented by the elevation of a red line,—and that from all other causes—typified, in like manner, by a black one; these running from left to right had their altitude interpreted, numerically, by certain figures at the left side margin. Through the kindness of Dr. Duncan, we have before us a duplicate of this chart, from which we make out that, in the early part of January, the deaths registered as from fever little if at all exceeded the customary rate of 8 or 9 per week; that, in February, this rate became doubled, and that a progressive advance took place up to the end of June, when the deaths from fever exceeded 200 per week, about which period the epidemic in Liverpool attained its culminating point.

A very slow but gradual decline of fever went on through the summer months, although the general causes of mortality were more active in the place than ever; attributable, in great measure, to the supervention of bowel complaints, in subjects already debilitated by previous attacks of the epidemic. The Registrar-General's Report for the quarter ending the 30th September, has the following striking passage:

> "Liverpool, created in haste by commerce, by men too intent on immediate gain; reared without any very tender regard for flesh and blood; and flourishing while her working population was rotting in cellars, has been severely taught the lesson, that a part of the population, whether in cellars or on distant shores, cannot suffer without involving the whole community in calamity. In itself one of the unhealthiest towns of the kingdom, Liverpool has for a year been the hospital and cemetery of Ireland. The deaths registered in the four quarters of

1846 were 1934, 2098, 2946, and 2725; in the three quarters of 1847, ending in September last, 3068, 4809, and 5669!"

It will be seen from the above extract, that the general mortality in the summer quarter of 1847 was nearly double that of 1846; and this latter was one of singular fatality. In the same period of 1845 the registered deaths were but 1963, a number constituting about an average of the summer quarter in ordinary years. Thus, at the period in question, Liverpool nearly trebled its customary mortality, and mainly through the agency of fever. And although a considerable share of the excess was due to immigration, this was far from being exclusively the case; for, by this time, the disease had very generally diffused itself among such of the fixed population as were exposed to the causes, exciting and predisposing.

The epidemic declined very rapidly through the months of October, November, and December; the fact being attested by notes to the Registrar-General from the several District-Registrars, published in the last Quarterly Report for the year. One of them observes, "there has been a decrease of nearly one half in the deaths this quarter. This is owing principally to a great decrease in the number of fever cases." Others refer their diminished returns to the giving up of some of the fever accommodation that had become unnecessary. The general mortality for the entire parish in the quarter ending December 31st, was 3725, being less by 1944 than in the preceding quarter. This makes the mortality of Liverpool for the whole year 17,271, the number of deaths in an ordinary year being about 7500.

The comparative magnitude, in Liverpool, of the last year's pestilence will be better appreciated by attention to the following facts and statements, which show the extent and the fatality of fever:—*first*, in ordinary years; *secondly*, in one in which it prevailed as a severe epidemic, as then considered; and, *thirdly*, as prevalent, under corresponding aspects, during the late visitation. For the subjoined particulars we have again to acknowledge the kindness of Dr. Duncan.

The deaths from fever in an ordinary year scarcely exceed 400; and the number of cases under treatment at one time in the permanent fever hospital of the place, do but average from 40 to 50. In 1837, however, when Liverpool, along with some other parts of the country, was more severely afflicted with epidemic fever than within any other recent period, prior to the late visitation, the deaths in a single quarter, when the epidemic was at its height, were registered as 244, being considerably more than double the average; and the hospital was for some time crowded with 130 patients, a number exceeding by 20 its estimated capabilities. Upon this occasion no extra accommodation was made, nor was it urgently required. Now, in 1847, so many as 2227 deaths from fever were registered for the summer quarter; and 1700 cases at once were supplied with some sort of hospital accommodation; whilst, at the same time, about 6000 were estimated to be under medical care at their own dwellings. After these statements we do not expect to be charged with exaggeration, or with the use of language too strong, in reference to the terrible condition of Liverpool during the past most memorable year.

We have been thus circumstantial with regard to Liverpool, because it formed the entrance-hall, so to speak, of the epidemic fever; and a point, at the same time, from which issued to other parts of England this colonization of pestilence.

We shall now adduce some particulars exhibiting the progress and the extent of the epidemic in Manchester; since the contiguity and ready accessibility of that city to Liverpool, rendered it the next most remarkable place of invasion; and because, as the metropolis of the manufacturing districts in the north of England, it will very well exemplify the general state of things in other towns within any proximity to it; all of which were, more or less, invaded in the same manner.

Manchester has always exhibited a more favorable condition in a sanitary point of view than Liverpool, notwithstanding the prevalence there of the factory system in all its giant proportions. Fever, in particular, has rarely been severely epidemic. Its fever hospital, we understand, will but accommodate about 84 patients, and in ordinary seasons it is never full, although it supplies accommodation, not only to Manchester proper, but also to Salford, and other contiguous townships. The sick inmates, we have been informed, will sometimes be as low as 20. It appears from Dr. Howard's 'Sanitary Inquiry into the Condition of Manchester,' published in 1840, by the Poor Law Commissioners, that the admissions per annum do not average more than about 500, and that in some years they scarcely exceed 300. In the epidemic of 1837–8, however, the number was augmented considerably, so many as 1372 being admitted in 1838; and there was opened at this period, moreover, a temporary hospital with about 40 beds, which was in use, Dr. Howard tells us, for four months, during which time 180 patients received treatment therein. Only in a single year anterior to this period, had the admissions amounted to 1000; this was in the year 1802, during the epidemic following the bad harvests of 1799 and 1800. This preliminary statement will facilitate a proper estimate of the magnitude of the more recent epidemic.

In Manchester, as in Liverpool, the febrile disorders prevalent in the autumn of 1846 had pretty well subsided about Christmas; cases of fever, however, continuing somewhat above the average, yet not to an extent calculated to arrest either medical or public attention. During the first two or three months of 1847, fever began to prevail at least with the severity of an *ordinary* epidemic, and the fever hospital was constantly full. It was about the end of March that the authorities deemed it necessary to establish additional accommodation, and that, too, of an extraordinary character; no one seeing a limit to the Irish invasion which, at this time, began to excite the anxious consideration of all classes. A large cotton mill, that for some time had been unoccupied, was engaged, and in a brief period was fitted up so as to accommodate 400 patients. This was about the middle of April, and the place—filled in a very few weeks—became quite inadequate to meet the demand. In consequence, about Midsummer, there were taken for a like purpose two other vacant mills, which together would accommodate the same number of inmates as the single mill already in complete occupation. For about six weeks all the fever hospitals were in full requisition; and during this time not less than 850 persons, including the inmates of the permanent establishment, were simultaneously under hospital treatment; besides large numbers who refused to leave their homes, miserable for the most part as they were; and notwithstanding that adjoining townships, for the first time, we understand, in their history, had fever accommodation of their own. It was about the middle of August that a decline became first observable, which progressed gradually to the end of the year. We have procured a statement that exhibits the number of admissions to fever hospitals for every month in the year 1847; this we subjoin, and therefrom may be seen at a glance, the progress and decline of the epidemic fever in Manchester:

Jan.	66	April.	236	July.	1298	Oct.	514
Feb.	76	May.	525	Aug.	916	Nov.	438
March.	87	June.	830	Sept.	725	Dec.	364
	229		1591		2939		1316

These figures represent a total of 6075 admissions within the twelve months; the highest number in any former year having been but 1552.

The extreme rarity of fever in a place like Manchester is certainly a somewhat remarkable circumstance, especially when contrasted with what obtains in Glasgow and some other localities, comparable with Manchester in most other respects. Yet the rate of general mortality is high, and the population in many parts is densely aggregated. It is, moreover, next to Liverpool, probably the most frequent resort of Irish emigrants. As illustrating the infrequency of fever in Manchester, and as exhibiting the magnitude of last year's epidemic in another point of view, we have procured certain returns from the union district medical officers, which refer to the most Irish division of the township, and that wherein fever was most prevalent during the late visitation; the division in question comprising a population of about 34,000. These returns, which we subjoin, give the quarterly number of cases referred to the medical officers for the three years preceding the last, and for the last itself; an analytical classification of the same being made, as cases of febricula, fever, and miscellaneous disease. In these returns the name of the ailment was determined according to the prominent indications at the period of the first visit; the cases evincing undoubted marks of *fever* were, of course, so designated; and others, seeming rather to be ephemeral—merely feverish cold—were called *febricula*. In some instances, however, these latter became in a few days fairly developed fever, without any corresponding alteration in the record. This explanation will render more intelligible the table supplied below.

1844.

	Febricula.	Fever.	Miscellaneous.
Quarter ending March 31st	11	12	159
June 30th	7	5	86
Sept. 30th	6	0	93
Dec. 31st	20	13	187
Total	44	30	525

1845.

	Febricula.	Fever.	Miscellaneous.
Quarter ending March 31st	10	6	184
June 30th	7	5	153
Sept. 30th	5	6	170
Dec. 31st	18	3	165
Total	40	20	672

1846.	Febricula.	Fever.	Miscellaneous.
Quarter ending March 31st . .	13	1	184
June 30th . .	31	5	205
Sept. 30th . .	62	19	256
Dec. 31st . .	75	27	372
Total	181	52	1017
1847.			
Quarter ending March 31st . .	117	62	603
June 30th . .	153	494	560
Sept. 30th . .	274	882	609
Dec. 31st . .	87	325	462
Total	631	1763	2234

In estimating the remarkable contrast which the above statements show between ordinary years and the late pestilential one, it must be kept in mind that the figures form, in some respects, the statistics of pauperism for the respective periods, as well as of disease. The disparity in numbers must not be taken as coincident exactly with variation in the amount of disease in the several years; because in prosperous seasons the same proportion of its aggregate amount does not come within the jurisdiction or the cognisance of the parochial authorities. Still, the above figures, taken with other circumstances already set forth, may lead very well to a general conception of the magnitude in Manchester of the late epidemic of fever.

We have not the means of accurately furnishing either the absolute or the relative number of deaths registered in Manchester, as from fever, in the year 1847; the Annual Report of the Registrar-General will, however, when published, supply tolerably accurate statistics in these respects; meanwhile, we may gather from the quarterly returns the extent to which the general mortality of the place was influenced by this visitation. Manchester proper, the township, had in 1841 a population of 192,408; in an average of years the deaths slightly exceed 6000. In the last year the deaths from all causes registered in each quarter were as under:

1st Quarter	2185
2d Quarter	2362
3d Quarter	2783
4th Quarter	2210
Total	9540

This excessive mortality, however, was not altogether attributable to fever; in the earlier months the scarcity of provisions and consequent destitution induced and aggravated many other diseases, especially amongst infants; in the summer quarter, diarrhœa and dysentery contributed largely to the deaths, but in these instances fever was very often the remote cause, as the subjects of the attack were in many cases convalescents after fever. Influenza, at the close of the year, added likewise to the average mortality, but, from all we can learn, not in Manchester to a very great extent; much less, certainly, than was the fact here in London.

In all the manufacturing districts of Lancashire, and the adjoining counties, the epidemic raged very extensively; though scarcely to the same extent, proportionately to the population, as in Manchester. In Bolton, Preston, Rochdale, Stockport, and other neighbouring towns, the mortality was excessive throughout the year; a result for the most part attributable to fever. In Leeds, Hull, and other towns of Yorkshire, a like state of things obtained. Localities more southward, such as Birmingham, Dudley, Wolverhampton, and Shrewsbury, suffered largely from the same cause. London, also, had its mortality from fever nearly doubled. In a few words, wherever driven by hunger and destitution from their native shores the miserable Irish fled, thither did they convey the germs of epidemic fever; everywhere were they themselves the first victims; soon, however, involving in the common ruin large sections of the general community. It may, indeed, with truth be asserted, that the virulence with which, in any place, the disease raged, was commensurate with the extent of Irish immigration. We will not, however, illustrate this department of our subject by any additional figures; opportunities for so doing may arise hereafter in some further examination of its bearings upon the great sanitary question, to which, happily, the public attention is now fairly awake. We have been somewhat minute in statistical details affecting Liverpool and Manchester, because these two places were in an especial relation to the imported fever; and because facts there evolved in its rise and progress are fraught with unusual interest in regard to many points in its pathology, and particularly in regard to the very important one of its contagion.

We proceed to sketch the features which the particular type of fever assumed.

All who have interested themselves with the accounts furnished by our Irish brethren respecting fever as prevalent for some years past in the cities and towns of the sister isle, will be familiar with the term "maculated fever;" invented and applied, in the first instance, we believe, by Dr. Graves. The inquiry as to whether maculated fever constitutes a form of disease *sui generis*, or is a mere modification of continued fever arising from various causes, we will enter upon in the sequel. Our immediate purpose is to describe its characters, as manifested in this country, during the late epidemic; the cutaneous eruption, so often alluded to by writers on what is called *typhus* fever, having been a prominent symptom in a large proportion of the cases. In the earlier months of the epidemic, maculæ very generally were to be observed; as the season advanced, the instances betraying this trait became less frequent; and towards the end of the year, as the fever subsided, maculated examples were comparatively rare. For reasons to be set forth as we advance, we were led to regard the development of maculæ as evidence of completeness in the attack, rendering the system secure, in some measure, against a repetition; yet, at the same time, we deemed the great bulk of fever cases throughout the epidemic to be ascribable essentially to an identical poison. The absence in many instances of maculæ, and other deviations from what we consider to form the *normal type* of the disease, we would attribute to various causes; and these being both numerous and complex, it becomes a difficult matter always to appreciate them amongst patients largely and indiscriminately congregated in hospitals. Sometimes the natural course of a malady, like fever, is disturbed by antecedent or superinduced lesion of structure; a previous exhaustion of the vital powers occasionally, we think, renders in some degree abortive the attempt at vascular reaction; and we have reason to believe that, when exposure to cold or some such circumstance sets up a febrile movement before the system is fully charged with the poison of the disease, a fever will be developed that does but imperfectly assume the special type. In reflecting upon the actual circumstances of the destitute poor, we readily conceive how liable to disturbance must be the normal course of all ailments, that tend in any way to observe stages and periodicity. Maculated fever we decidedly rank in this category. In it there may be noticed a definite progress and duration, as in the recognised exanthematous fevers, provided the system upon infection be in an appropriate condition for displaying the normal features of the disease; a deviation herefrom being noticeable, however, according to the character of disturbing influences.

When a person of sound constitution and good general health takes a fever, it is fairly to be presumed that such fever may be seen in its normal form, more especially if the person affected be on the juvenile side of middle age. In cases of this kind the peculiar virus, whatever be its nature, does not, it is reasonable to suppose, overcome the vital resistance until the system is deeply and fully imbued with it; and in the series of changes that supervene upon its morbific agency, there is commonly no defect in the vigour required for conducting the patient through the several stages. During the late epidemic, we had many opportunities of watching the invasion, progress, and decline of the fever under the circumstances here mentioned; the type in such instances was very uniform: and from the data so obtained, we proceed to sketch what seemed to us to constitute the *natural history* of the disease, the only just foundation whereupon to erect a sound theory and practice.

We noticed that the maculated fever evinced all the indications of a periodic affection, susceptible, as it appeared to us, of division into three stages of five days each: these, for the distinction's sake, we will call the stage of *invasion*, that of *dominance*, and the stage of *decline;* altogether occupying a period of fifteen days. Premonitory symptoms were to be observed in most cases for some days anterior to the development of any febrile excitement; consisting of sluggish digestion, speedy exhaustion of muscular energy, headache, and (almost universally) depression of the *morale*. We date the actual commencement of the febrile movement from the first recognition of a rigor or chill, associated with *malaise*, a state of matters which generally showed itself in the afternoon of the day of invasion. Later in the same day, a degree of reaction would ensue, marked, not so much by elevation of the pulse or increased heat of surface, as by restlessness and jactitation. The pulse, in some of the worst cases we saw, did not in the first stage exceed 80 in the minute; and altogether, for the first four or five days, the severer symptoms would be absent. Ill-spent nights and headache generally constituted the main grounds of complaint, prevailing to an extent, however, little exceeding that which frequently obtains in a common feverish cold. Still, when some familiarity had been gained with the disease, it was not difficult to recognise, in a certain prostrate state of the nervous system, the mischief in preparation; and sometimes it was in cases ultimately the most serious, that the pulse would maintain an infrequency of beat for the longest period; a circumstance dependent probably upon congestion of the cerebral vessels, or upon some morbid state of the blood. This comparatively mild state of things would usually last for about five days, when, on the evening of the fifth, some decided exacerbation commonly ensued; and the following day, the sixth, ushered in what we have denominated the stage of dominance, characterized in the following way: a cutaneous eruption became noticeable, situated upon the chest, arms, and neck, extending downwards towards the abdomen; the back when examined was generally found in a like condition; occasionally the eruption was nearly universal, implicating legs and feet; but we do not remember to have seen a case wherein the face was involved. The spots or maculæ very much resembled measles, but they were more elliptical than in this latter disease, and did not usually implicate the cuticle to the same extent. As remarked by several writers, these maculæ differ essentially from petechiæ; the former are red, while the latter are purple; petechiæ are evidently formed by the escape of blood from minute vessels, becoming deposited beneath the cuticle; maculæ result rather from congestion of the arterial capillaries about the cutaneous glands; and again, whilst petechiæ arise in many different diseases, maculæ, we have little doubt, are pathognomonic of a single species of fever. In cases where the efflorescence exhibited a lively scarlet appearance, the spots would disappear upon pressure; but, in very asthenic states of the system, where they had a somewhat dusky hue, an obliteration on pressure was but little perceptible. In well-developed instances, these maculæ would continue visible to the end of the fever, declining with this latter *pari passu.* Coincidently with the cutaneous eruption, the other symptoms underwent aggravation; the pulse rose, the heat of the skin became increased; the pain in the head by this time had generally ceased, to be succeeded, however, by a sense of confusion, or by severe mental disturbance. When the delirium was great, a disposition to leave the bed was evinced in a very uniform manner; the patient, in his own conceit, was rarely at home; and in such cases a determination almost always existed to make an escape at all hazards. In instances where there was no actual delirium, there was sometimes a distressing inability to sleep, combined with some vague sensations of dread. We very well remember some of our own feelings in this respect; through several nights an undefinable terror possessed us, and we *dared* not attempt to sleep, conceiving very forcibly that any approach to unconsciousness would bring about convulsions. Our physiology did not, under the then existing circumstances, cease to influence our imperfect reasonings; setting forth that if the brain by sleep forsook in any degree its control over the muscles, the reflex influence of the spinal cord would become exalted; and for several nights, under such an impression, we resolutely kept awake. On yielding to the persuasions and the reasonings of a medical friend, to disregard even *such* consequences, as being less serious in their nature than the continued sleeplessness, we slept, and experienced the fallacy of our semi-delirious views. This dominant stage would run over the second series of five days, the symptoms upon the whole becoming more severe as the period advanced; and when a fatal termination occurred, it was very usually about the eleventh day, at the close of this stage. The pulse, excepting a few hours prior to dissolution, seldom would exceed 124, even in the most unfavorable cases; and the tongue and teeth were not so generally invested with the dry, black fur and sordes, as in the typhoid condition of some other forms of continued fever. The final stage—the third series of five days—usually manifested itself in progressive decline of the symptoms of excitement; the pulse fell, cerebral irritation gradually subsided, and a greater calm in all respects was daily to be noticed. At the termination of fifteen days, on the morning of the sixteenth, the pulse had commonly attained its lowest point; being very often at this time a few beats in a minute below its customary frequency. In the stage of decline, a very favorable prognosis was afforded by the coming on of a stolid sort of deafness, which speedily disappeared on the complete cessation of fever; we scarcely call to mind an instance of a fatal issue, where this deafness showed itself.

The above sketch of that which we deem to be the normal course of maculated fever, has been taken from what, during the last year's epidemic, we extensively observed and closely scrutinized. We are conscious of being swayed, in the account just given, by no speculative notions or preconceived views; the periodicity, in particular, was, as recorded, a strict deduction from the facts supplied by our own experience. Indeed, after some months' familiarity with the epidemic fever, we could, in appropriate cases, very successfully predict the days of crisis. Several of the writer's professional friends can testify to this circumstance. Thus, in our own case we had some reason to doubt, during the stage of invasion, whether we had really contracted the epidemic fever or not, as for ten days anterior to any symptom, we had been recreating ourselves, far removed from the contagious atmosphere of fever patients,—and hereby a fact became afforded, that bears on the question of *incubation;* we postponed the decision in our own mind till the sixth day, at which time the spots would become due if our ailment were really maculated fever; each day, however, we examined the skin, but not until the anticipated period did any efflorescence appear. And so on, through the case; we predicted to our medical attendants, systematically, the times at which the several changes would take place, and in every instance were substantially correct. And, with rare exceptions, the same thing would obtain in all the examples that were conditioned in the way that has been set forth as proper for the study of the natural history of this disease.

In the class of cases from which we have deduced the above sketch, there was nothing remarkable to be noticed in convalescence; generally speaking, the strength was pretty well recovered in about a month from the cessation of all febrile excitement; and, as frequently obtains under corresponding circumstances, the health for some time afterwards was usually felt to be better than it had been previously to the attack.

It will be inferred from what has preceded, that the great bulk of patients suffering during the late epidemic, whether placed under hospital or domestic treatment, did not exhibit the disease in all respects conformably to the type just adduced; and, in a large proportion of instances, accurate notation in the matter of periodicity was impracticable, as the patients very often, when first seen, had been ill for some days, and no reliance whatever could be placed upon any of the statements referring to their previous condition. Others, again, when invaded by fever, were laboring under other ailments; and these were calculated, it will readily be supposed, to interfere with the natural course of the disease. In some cases, organic affections of the thoracic and abdominal viscera preceded and accompanied the fever; coincident diarrhœa and dysentery, induced by previous exhaustion of strength and want of food, were to be found exceedingly often; the aged were not unfrequently the subjects of invasion, and these in many instances became prematurely adynamic, before any perfect reaction could well be established. But, notwithstanding these qualifying circumstances, we are convinced from several considerations to be advanced in the sequel, that most of these affections were true, though modified, examples of the epidemic disease.

The destitute poor, amongst whom alone throughout the year the fever prevailed to any serious extent, were perpetually sustaining relapses; twice, thrice, and four times, very often did febrile symptoms recur; attributable seemingly to premature exertion upon recovery, to undue exposure, and at times apparently to the fact of the hospital convalescent diet being too strong for their enfeebled powers of digestion. In none of the relapses, however, did we observe either maculæ or periodicity; the features of such cases corresponded very much with what is to be seen in an ordinary feverish cold, occurring in debilitated frames.

We observed throughout the epidemic an unwonted frequency of bed sores, extending very often to ulceration and gangrene; these were commonly to be seen on the hips, loins, and sacrum; about the face and jaws, moreover, glandular enlargements and chronic abscesses were very frequent. These are circumstances that have long attracted notice in Ireland; they were especially mentioned by Dr. Stokes (Pathological Observations) nearly twenty years ago.

The premonitory signs of death, in the well fed and clad, were mostly those of cerebral oppression, and nervous exhaustion; in the destitute, commonly those indicative of the low, typhoid state, so called,—the wild, fixed stare, picking the bedclothes, or attempting to seize within the finger and thumb imaginary objects in the air, paralysis of the sphincters with involuntary evacuations, the cold skin, dry brown tongue, and progressive decline and imperceptibility of the pulse. It has been already stated that, in well-marked cases, death most frequently occurred about the eleventh day; in some instances, however, the patients would sink, almost unaccountably, at any period; the mischief apparently having been, in great measure, accomplished by famine and anguish prior to invasion. Our attention was sometimes directed by nurses in hospitals, to an unusual gathering of flies on the persons of the moribund; a curious fact, though explicable enough.

On cessation of the febrile excitement, we occasionally noticed a symptom to which some of our Irish brethren attach great importance; we allude to a singular diminution in the frequency of the pulse, without corresponding depression of cerebral and nervous energy; a circumstance regarded by Dr. Stokes as caused by softening of the heart. We have several times counted a pulse that was below 40 in the minute. But we ourselves feel a difficulty in admitting this view, for the symptom, under the free use of wine and nutritious fluids, would generally disappear in two or three days and not return; a fact that is hardly compatible with the notion which ascribes the depression in question to so serious an organic lesion.

There is every reason for regarding the fever that so often prevails epidemically in Edinburgh and Glasgow, as identical in type with the one that is now under discussion. Dr. Alison, in a paper quoted by Dr. Copland in his Dictionary, supplies the subjoined statistical table, which elucidates, in some degree, the ages at which persons are more or less prone to an attack; and which, at the same time, may influence the prognosis, as it represents the relative mortality at various periods; it is constructed from particulars of 342 cases.

Age.	Cases.	Deaths.	Proportion of deaths.
Under 15 years . . .	83	2	1 in $41\frac{1}{2}$
15 to 30 . . .	149	11	1 in $13\frac{1}{2}$
30 to 50 . . .	93	17	1 in $5\frac{1}{2}$
Above 50	17	7	1 in $2\frac{1}{2}$
	342	37	1 in $9\frac{1}{2}$

We caused an analysis, upon the above plan, to be made upon a much larger basis; the table below, it will be seen, comprehends particulars of 2662 cases that occurred in an hospital of which we had the principal charge; the results, however, vary but little from those obtained by Dr. Alison.

Age.	Cases.	Deaths.	Proportion of deaths.
Under 15 years . . .	686	59	1 in $11\frac{2}{3}$
15 to 30 . . .	1121	79	1 in $14\frac{1}{4}$
30 to 50 . . .	683	104	1 in $6\frac{1}{2}$
Above 50	172	45	1 in $3\frac{2}{3}$
	2662	287	1 in $9\frac{1}{3}$

In correction of the above table, it must be stated that amongst the cases there was a sprinkling of scarlatina, measles, and smallpox, but only to a limited extent, little calculated to influence the general results. The higher mortality amongst children, than in Dr. Alison's table, may partly be attributable to the foregoing circumstance; but we conceive that it was owing, in a much greater degree, to intestinal and mesenteric disease, which was largely found; having most probably been induced in excessive proportion by the scarcity of food that preceded. Upon subsidence of the fever, the abdominal disease would often interrupt convalescence, and carry the little patients off.

The rate of mortality set forth in the analysis we have just given, does not, however, represent the actual rate in cases taken indiscriminately; large numbers refused, absolutely, all hospital accommodation when proffered: and these furnished, in most instances, the milder forms of the disease. Owing to this circumstance, hospitals generally exhibited a less favorable result than the class of cases treated, or watched rather, elsewhere.

For several reasons, there were few facilities for investigating the morbid anatomy in this epidemic. We have before us notes of the necroscopic results in but fourteen cases; the particulars of these we will furnish in a few words. In nearly all the instances there was alteration either in the substance or the membranes of the brain, varying from mere congestion to marks of extensive inflammation and effusion of serum and lymph. In eight of the cases, there was considerable pulmonary congestion, a result most likely owing, in the majority of instances, to changes wrought by *agony* in articulo mortis; for pulmonary complication, judging from the symptoms during life, was somewhat rare. In not more than four cases, was there material change in the alimentary canal; in this number, however, the lessons so much referred to by our Gallican brethren were noticeable in a somewhat severe degree; there being, in three of the cases, both enlargement of the *glandulæ solitariæ* and contiguous ulceration. The heart was generally pale and flabby, the walls at the same time being in some measure thinned. In one case there was softening of the spleen.

So far as this limited record, taken in connexion with the usual course of the previous symptoms, would justify any induction, there was no reason to regard any of the recognised alterations in the solid structures as yielding the earlier links in the chain of morbid phenomena; there was not any organic lesion that seemed to stand in a definite or uniform relation to the symptoms at large. The brain, it is true, had sustained, almost invariably, some physical alteration; a circumstance that might seem to favor the well-known views of Dr. Clutterbuck; but the lesions traceable in this organ were neither constant nor in proportion to the general symptoms of fever; they were found to correspond very much with the ordinary indications, during life, of cerebral mischief. Sometimes there was but mere congestion observable; at other times, marks of general inflammation were to be noticed; and, in some instances, there were meningitis and serous effusion. When these several alterations were considered in relation to the antecedent particulars of individual cases, there was always reason to regard them as changes superinduced by the general disease, rather than as signs indicating the origin and cause of the previous fever: and the same thing may be affirmed of the other lesions. Certainly the French doctrine, that associates certain injuries of the intestinal mucous membrane and fever as an invariable circumstance, received no corroboration whatever, but quite the contrary.

We now proceed to record the *treatment*, witnessed and practised by us during the last year's epidemic. At its outbreak, there was frequently no treatment at all,—absolutely none; this was in consequence of the suddenness and the extent of its prevalence. In an earlier portion of this article, facts have been mentioned regarding the crowding of cellars and low lodging-houses, by immigrants from Ireland, who brought with them the germs of fever; in several of the large towns of this country, the number of cases suddenly became more than quadruple an average; the unavoidable result of which state of things, in some instances, was that, for a few weeks, no accommodation could be got ready sufficiently ample to receive all the patients. To those who have ever examined the class of tenements that serve, in great cities, as the resort of tramps and vagrants, it will require no argument to show the utter unavailableness of therapeutical prescriptions for such individuals in such places; more particularly so, in regard to a disease like the one that was epidemic. Four or five fever patients might often be seen stretched on heaps of straw, placed on the damp brick floor, in apartments underground, so close and ill ventilated, that neither air nor solar light could approach the wretched inmates in any proportion whatever to the requirements of the human organism; moreover, the ostensible hosts of such dens had, in most cases, no sort of sympathy with their stranger guests; these were admitted, very often, simply because the parochial relief that was sure to come, would afford the proprietors a means of exacting their charges. To complete the difficulty constituted by these several circumstances, the poor fever-stricken patients at times could speak not a word of any language but the Celtic Irish; and, owing to all these causes, the malady was often left, inevitably, to its own unaided course. Still the patients were watched; the semblance of medical care had, probably, some beneficial influence upon the mind; and, as vacancies arose in fever hospitals, these had to be filled up from amongst the outstanding cases; and, for this latter reason, principally, they were kept under constant observation.

It was truly remarkable how small a proportion of patients so distressingly situated actually died; we do not think the mortality under these conditions was very much higher than in hospitals; in about a fortnight from the commencement, the symptoms of fever would generally subside, and after a convalescence, unduly protracted certainly, the patients got well; and this course of events we witnessed in some decidedly bad cases. Relapses, however, were very frequent in these circumstances; and debility with its consequences, such as neuralgic pains and œdema of the legs, was long continued and extreme. It was a common thing to hear for months afterwards of utter inability to work "ever since the fever."

In dealing with this class of patients in hospital, and also in treating individuals differently conditioned in private practice, we could not fail to deduce a practical lesson from the experiment which an unhappy combination of events had prepared to our hand. Having found, upon a moderately large scale, that patients labouring under the epidemic fever would, in a great majority of instances, recover without any treatment, positive or negative, we thought ourselves but little justified in experimental trials of any measures that might be denominated bold or heroic. As a rule, we discountenanced all active or powerful interference; bleeding and blistering, purging or opium, we never resorted to without some well-marked indication; being content, in the bulk of instances, simply to withdraw *lædentia*, where we could detect no satisfactory clue to positive *adjuvantia*. Keeping patients in bed from the beginning, and insisting that there they remain until the establishment of convalescence; exclusion of injurious stimuli, cleanliness and free ventilation, with farinaceous and diluent drinks, sufficed in most instances to bring them successfully through the disease, without resort to active measures at all. Indeed, we had occasionally reason to think that a simple saline, prescribed but as a placebo, constituted a source of irritation to the gastro-intestinal mucous membrane.

With very few exceptions, patients, on admission to hospitals, had attained the second or dominant stage of the fever; and so long as the skin was hot, and the pulse hard and quick, the diluents given were usually prepared with water; on approaching the third stage—that of decline, a considerable proportion of milk was added to the drinks. As the skin began to assume its natural temperature, and the pulse rapidly to fall, beef tea was allowed once or twice a day. On cessation of all febrile excitement, the appetite would quickly return, whereupon we advanced to rice, to bread, and progressively to animal food; going always in arrears, rather than in advance of the appetite. In about a fortnight from the subsidence of fever, hospital patients were usually in a condition for discharge. In at least two thirds of the cases, this simple treatment, and equally simple course of the disease, formed the rule. Nevertheless, we had often to deal with troublesome complications, both of a local and general character.

A very frequent difficulty was experienced in the coming on of diarrhœa or dysentery, especially during the summer months; many times this was apparently the result of antecedent hunger and destitution; it was a complication to be met with in all stages of the malady, least so probably in the dominant one. Our remedy was, uniformly, a combination of two grains of the acetate of lead with a third of a grain of opium, given three times a day until all purging ceased; more or less frequently, however, according to circumstances. If any such affection occurred in the progress of convalescence, we administered the same remedy; at the same time reducing the allowance of food by placing the patient, once more, upon milky farinaceous diet. Constipation was but rarely a cause of embarrassment; at times, however, a little castor oil had to be administered; this was only when some days had been passed without an alvine evacuation, and when complaints of fulness and uneasiness were made. It was seldom that any injurious purging succeeded to this limited use of aperients.

Pulmonary complication was of rare occurrence; a circumstance which was, in part, most likely attributable to the comparatively slight variations of temperature that characterized the past year. The trying winds of early spring had pretty well subsided before the epidemic attained any magnitude, and summer glided through autumn into winter with very gradual transition. Occasionally, however, we had to deal with partial pneumonia of an acute character; this yielded readily to the application of leeches and moderate doses of opium and calomel. Here it may be well to state that, in the administration of small doses of opium, either for thoracic or abdominal complication, no obviously injurious consequences resulted to the brain, notwithstanding the tendency of this organ in some cases to become fatally damaged.

It was the head, indeed, that constituted the principal seat of danger. Amongst persons who had suffered greatly from destitution and want prior to the attack, sinking and fatal collapse would sometimes take place most unexpectedly, and without cerebral symptoms having been displayed to any notable extent. Occasionally, the irretrievable exhaustion preceding death was referable to an intractable diarrhœa. But in most fatal cases there was cerebral complication preceding death, exhibited in the well-known *typho-mania*, generally of a low character, but showing itself at times in symptoms grave and acute,—such as constant delirium, noisy exclamations, hallucinations of sense, resolute determination to rise from the bed, subsultus tendinum, and the like. In one instance we had a patient who contrived to evade the vigilance of the nurse, and to jump from a high window, whereby life was at once lost. The intensity and character of the head symptoms, had usually relation to the temperament and antecedent state of health. In treating the cerebral complication, we were exceedingly reserved in taking away blood, rarely venturing indeed to withdraw it topically. In the earlier periods of the epidemic, and before its asthenic type had become so fully demonstrated, we had had occasion to regret even the very moderate extent to which we did employ local blood-letting in an instance or two. It happens very generally indeed, that inflammatory action, locally developed in the progress of diseases dependent apparently upon some specific poison, will neither yield to nor endure the depletory treatment available in common inflammation; this is distinctly seen in scarlatina, in some forms of erysipelas, and in the pneumonia supervening upon epidemic catarrh or influenza. The rule certainly obtained in the fever of the late epidemic. In cerebral complication, or where this was only threatened, our proceeding was to remove the hair, and to apply cold to the head; withdrawing at the same time, as much as possible, all sources of undue excitement. Occasionally in robust frames we had reason to think that decided relief was obtained by application of a single leech within each nostril,—a course to which we were led from observing, in some instances, a spontaneous coming on of epistaxis, with beneficial results. We did but rarely blister the scalp, having a conviction that the practice was useless; we saw it tried, however, and employed it ourselves in a case or two; and most frequently it seemed to us that the effect was rather to tease and annoy the patient than to do good. The judicious administration of opium, in certain states of the brain analogous in great measure to delirium tremens, produced undoubted advantage to the patient in some instances, by causing sleep and tranquillizing the nervous system; six or ten grains of Dover's powder formed the vehicle in our own practice, given, however, but rarely before the tenth or the eleventh day. We ventured but in a single case upon the heroic treatment of Dr. Graves, in administering large doses of tartar emetic with opium; our impressions upon this point were altogether adverse to the practical conclusions of that eminent physician; to this matter, however, we may return in the sequel.

In the form of fever now under consideration, we believe that wine in some circumstances constitutes our very sheet-anchor: but we do not go the lengths of Dr. Corrigan and some other practitioners, who would administer it very freely in a large proportion of instances throughout the greater part of the disease; because we know that numbers—certainly the great majority—will recover very well without it; and because the tendency to arachnitis, which is notoriously great in this disease, must be aggravated by its superfluous exhibition. But in all cases where signs of collapse began to show themselves, indicated by the feeble pulse, the cold skin, dry tongue, marked obscuration of cerebral energy, paralysis of the sphincters, and involuntary evacuations—or, indeed, when there was any approach to this state of things,—we hesitated not, whatever might be the seeming local complication, freely to use both wine and powerfully nutritious fluids; conceiving that under such circumstances the immediate object was, in the quaint language of Cullen, to "obviate the tendency to death;" and often and often again had we reason to be convinced that by such a course—whereby time was gained—life was saved. The patients were kept alive during critical moments, and the powers of the system gained the opportunity of rallying, ultimate recovery ensuing. We generally kept up stimulation until the circulation was freely restored, and the skin was warm, till the tongue became moist, and the mind restored to some degree of its wonted clearness.

It may almost be unnecessary to add, that we had occasionally to employ the catheter when the head was affected; retention of urine, however, was a much rarer symptom amongst the destitute than with patients well to do in the world; the brain generally suffering more in the latter class of persons.

We have now referred to all the leading circumstances that directed or influenced our course in treatment of the epidemic fever; in many conditions of but secondary importance, our proceeding may be summed in the hackneyed phraseology—"we were guided by general principles." We do not know that in convalescence any remarkable features exhibited themselves; if the great tendency to relapse be excluded, its progress very much resembled that observed in convalescence from other such affections. Before closing this account of the treatment pursued, we will add a word upon the possibility of "throwing off the fever." In many instances, we saw the symptoms of *malaise* and *febricula* removed by the timely use of an emetic and a day or two's confinement to bed; and this, too, when there had been great exposure to the contagious influence; but sooner or later —generally in two or three weeks—the subjects would relapse, and after another "throwing off" or two, would pass through the regular stages of the disease. But in all these respects we noticed no circumstance that is not distinctly mentioned by all writers on fever of any note.

In this our account of the epidemic we have hitherto restricted ourselves to *description*, having cited facts and figures in illustration of its rise and progress, and having to the best of our power, with limited space, portrayed and exemplified the characteristics of the disease and the treatment pursued within the sphere of our own experience. We will now enter into some general considerations relating to the *pathology* of fever, as elucidated in any degree by the foregoing circumstances; and finally discuss, in brief terms, the practical conclusions that are seemingly deducible from all that is known concerning treatment.

We do not rank with those who would regard continued fevers as being all essentially of the same species. We think that radical differences are to be traced, both in the particular causes, and in the phenomena that result. It is quite true, that in so far as the term fever comprehends a group of symptoms co-ordinated after a certain manner, there is a degree of uniformity to be recognised under all circumstances. But in dealing with diseases which, like fevers, do not admit of any certain localization, but appear to be quite general in their nature, we would reject as a basis of classification, symptoms that are but indicative of *degree*, and also the mere locality of structural lesions; for we conceive that more exact notions upon such subjects would prevail, if a higher regard were attached to the demonstrable, or even probable causes, which originate the subsequent series of changes. We think that in this way,—by calculating from the first link—a more accurate interpretation would be gained of the entire chain of morbid phenomena. Gout and rheumatism are general diseases; many of the symptoms in each are alike, and the joints suffer principally in both: hence, guided by mere semeiology and local lesions, some writers have considered the two affections to be essentially identical. The *causes*, however, are obviously different in the two instances; and this distinction furnishes the best evidence of distinct speciality in the diseases themselves. Between intermittent and continued fevers, it has long been the custom to recognise an essential difference in species; and this recognition has resulted rather from a supposed difference in the originating miasm, than in the mere diversity of symptoms. In the exanthematous fevers, this ground of distinction is constantly taken; a certain poison being introduced into the circulation, developes the smallpox and nothing else; another, in like manner, excites the scarlet fever; and a third, the symptoms of measles. In all these cases there is the general phenomenon of *fever*, and in all there is local determination to the skin: but in each instance a separate agent is considered, upon good grounds, to be at work; and for this reason, the exanthematous fevers are regarded as being of distinct species, and not as pathologically unique, although the prominent symptoms and the structures chiefly implicated exhibit considerable uniformity. Now is it not possible still further to establish distinctions amongst continued fevers upon a similar principle? Our own conviction is that it is so.

It is remarkable how we have been gradually returning, of late years, in several departments of pathology, to the views and opinions of our ancestors; but with this qualification, that we do not rest upon them as fixed truths, until they have been verified by observation and experiment. This reaction in the medical mind is nowhere more striking, than in the notions now very commonly entertained upon the subject of fever. Humoral pathology is being revived. Many at the present day would adopt the aphorism of Sydenham with respect to fever: "Nihil esse aliud quam naturæ conamen; materiæ morbificæ exterminatione in ægri salutem omni ope molientis." And in our judgment, certainly, if a broad induction must be made, that will comprehend well the several phenomena of a febrile movement, there is none that adapts itself better to the facts of the case than the aphorism just cited. In all classes of fevers, there is first witnessed a depression of vital power, as if from some sedative influence; then there is vascular reaction, and this is succeeded by abnormal secretion and excretion; the whole being followed, in favorable cases, by progressive restoration to health. Now our own conviction is that, so far as the present state of knowledge will permit, fevers should be regarded as distinct in species, according to the circumstances of the primary sedative impression. The common ephemeral fever, sometimes designated *febricula*, is often induced by taking cold; hence the term "feverish cold." Some sudden or partial suppression of cutaneous transpiration occurs, the patient experiences *malaise*, in a few hours a febrile movement takes place, an unwonted exhalation from the skin is developed, and the fever subsides in a day or two; then the patient is well. If, however, under such circumstances, there have been faulty assimilation for some time preceding, and especially if such be attributable to undue wear and tear of the nervous system, the febrile movement is often prolonged for a week or a fortnight, as though *time* were a condition necessary to complete depuration of the mal-assimilated fluids; such depuration forming probably the cause, why persons after such attacks frequently experience better health than before. Again, individuals who dwell contiguously to accumulations of organic matter in a state of decay, often become sensible of depression in the general health, which a febrile movement most effectually remedies, if the cause be withdrawn; but which, even in the latter case, would have been of slow and difficult cure without the intervention of fever; the sporadic instances of fever, recorded in recent sanitary reports as developed by malaria from open cess-pools and undrained courts and alleys, may exemplify this fact. Some think that, during the decomposition of organic matter, a specific miasm may be generated, which, if not dissipated by effective drainage and ventilation, fructifies in the human system, and not only originates fever in the individual, but also in the persons of such as imbibe the miasmic *excreta* whilst undergoing elimination from the system of one already affected. It is possible that maculated fever may have had some such *origin*, however extensively it may spread, and be kept up by contagion; the primitive poison having, in passing through the human system, become endued with specific qualities, that determine its continuance by some such law as that which maintains in perpetual existence the poisons of scarlet fever, measles, and smallpox. But to return to our position,—we would, so far as positive knowledge will allow, recognise distinctness of species in fever, correspondently with distinctness in the poison or influence that may have set up the febrile movement.

This view, as well as our method of illustrating it, may be deemed very speculative; but what general notions can be advanced upon the subject of fever that are not so? And in the absence of sure demonstrable evidence upon any point, we do not for our own parts see any objection to a little rational speculation, if guided by the probable light of analogy. These remarks introduce us to the particular inference at which we have arrived, from the best consideration we have been enabled to give to all the incidents of the last year's epidemic;—that maculated fever, as first advanced by Dr. Perry of Glasgow, is a true exanthem; and that as such it should, in our nosological arrangements, no longer be grouped with forms of disease which differ in many essential respects from it, which are attributable very often to no specific origin, and which are yet all arranged in one common category under the denomination of *continued fever*. The phenomena of maculated fever, we are quite sure, will more accurately be noted under the assumption of its belonging to the exanthematous class; its study being aided by such analogies as these latter afford.

We know not of a single feature that attaches in common to scarlatina, rubeola, or variola, that does not exhibit itself in maculated fever. It is all but certain that, in most cases at least, it arises from the introduction of some peculiar virus into the blood; and that, after a period of incubation, this determines physical alterations most probably in the whole of the circulating fluid; that vascular reaction is hereupon set up, as if for the purpose of depuration; that, under appropriate circumstances, definite local characteristics display themselves; that a certain periodicity is observable in the progress and duration of the malady; and, finally, that after an attack, a degree of immunity is obtained against a repetition.

For conciseness and perspicuity, Cullen's definition of exanthematous fevers has not yet been surpassed: "Morbi contagiores, semel tantum in decursu vitæ aliquem afficientes; cum febre incipientes; definito tempore apparent phlogoses, sæpe plures, exiguæ, per cutem sparsæ." This definition, we proceed to show, will apply to maculated fever as rigorously as to the diseases comprehended within it by Cullen. The examination we shall now make, may take place under the separate heads of contagion, immunity after attack, general characteristics of the fever, and the cutaneous eruption.

However diverse may have hitherto been the opinions of the profession respecting the contagious property of some forms of fever, perfect unanimity regarding the contagion of maculated fever will henceforth certainly obtain. In this country, at least, we do not believe that any one who has had experience of the disease will doubt it. Very few practitioners in France admit the contagious property of the *fièvre typhoide* of Paris; but soon after the irruption of the epidemic now under discussion, we formed the opinion in our own minds that the disease which has been so admirably elucidated by Louis, Chomel, and others, is not identical in its origin and cause with maculated fever; that, in fact, the diseases are etiologically distinct. We find from Dr. Bartlett's work on the 'Fevers of the United States' (the second edition of which has just come to hand), that the same opinion is entertained by him; and this he has supported by many arguments and illustrations, which, however, our limits will not permit us to cite. Before we adduce any details tending to confirm the doctrine of contagion, as a property of the last year's fever, we will offer a few comments regarding the probable relations subsisting between it and famine.

Dr. Corrigan, in the publication whose title is prefixed to this article, advances the doctrine that deficiency of food *originates* fever, however it may afterwards extend through the influence of co-operating circumstances. "The opinion," says he, "that famine and fever are in Ireland as cause and effect, is an opinion formed not in haste or excitement;" and after a cursory notice of its several visitations during the greater part of a century, he observes: "Upon a general view of all the instances, with the accompanying circumstances, we find one condition invariably present, famine, which we therefore mark down as their common cause." But Dr. Corrigan does not attempt to show by any analysis of individual instances, that famine does directly and efficiently produce, and has produced, fever as its physiological consequence; whilst he largely admits that it may and does spread through the operation of contagion. Dr. Kenedy, whose treatise upon the same subject we have before us, disputes the premises from which Dr. Corrigan deduces his theory, and proves indeed by numerous dates and precise references that, even in Ireland, epidemics of fever have occasionally sprung into existence when food has been abundant; and, moreover, that great scarcity of provisions has existed over some seasons with a very moderate amount of fever. In our own country, we have several times witnessed the effects of great commercial depression in concomitance with high-priced food, in production not only of some degree of famine, but of over-crowding and filth, and yet no epidemic fever arise. If space would allow, we could bring what we deem to be proof of the position we confidently maintain, that neither famine, nor defective ventilation, nor insufficient drainage, will separately produce maculated fever; we doubt if, in the aggregate, they will do this; but we are certain that all and each form powerfully concurring influences that determine its extension. From all that is known of the disease, it is highly probable that its development is owing to the operation of a subtile and specific virus that propagates itself by contagion—and, *we* incline to think, exclusively so; but the *γένεσις* of the poison we conceive to be yet wrapped in the same obscurity as that of other poisons originating zymotic diseases. At any rate, this is sure, that when *any* circumstances depress considerably the vital powers of a population—deficiency of food especially—an extensive soil, so to speak, is prepared for its fructification; and in this way famine may, and in

Ireland certainly often does, constitute the remote or predisposing cause of epidemic fever. Facts, however, will certainly not warrant the wholesale generalization and decisive induction, which Dr. Corrigan has somewhat hastily made upon this subject.

Why the contagion of zymotic diseases should at times acquire extraordinary virulence and induce epidemics, is a subject upon which altogether but little light has been shed; and although some of the predisposing causes—those that are *subjective*—may be of an appreciable character, there are yet others—*objective* in their nature—which in the present state of knowledge are quite beyond human cognisance; we allude to certain external physical conditions, atmospheric most likely, to which writers have assigned the designation of "epidemic constitution." Hence, in all violent epidemics of such diseases as maculated fever, facts authorize the assumption that, besides a specific virus as exciting cause (a supply of which is always kept up by sporadic cases), and besides a depressed state of the vital powers operating subjectively as the predisponent, a remote cause, objective in its character, must exist in some outward physical agency, of the actual nature of which science as yet has revealed but little, or indeed nothing. Why do scarlet fever, influenza, Asiatic cholera, and other such maladies, retire for long periods from particular localities, and without any observable cause resume their ravages? We may admit the influence of depressed energies, and, in some instances, of contagion; but beyond, there is always some inappreciable cause in operation in the epidemic constitution. Famine, as predisponent, must have, in the case of maculated fever especially, its powerful influence fully acknowledged; and it forms the predisposing cause, moreover, the most likely to arise from time to time upon a large scale. Any extraordinary wear and tear of the muscular and nervous energies must rank also in the same category. Still the direct and immediate cause of maculated fever in individuals, we are strongly convinced, is nearly always, if not quite so, the reproduced virus of the malady itself. This is a view of things which most writers admit with regard to the recognised exanthematous fevers.

There was no circumstance more striking during the progress of the late epidemic, than its extension by contagion. Its first victims in this country, as before stated, were the Irish immigrants. In the large towns, it was first to be noticed in the cellars and low lodging-houses, filled with these unhappy persons; and, in its earlier history, the fever could almost be traced from house to house, and from street to street. It did not, like influenza or autumnal diarrhœa, break out simultaneously in all parts of a town, but would restrict itself for weeks to particular localities. Parts of cities inhabited by a class of persons circumstanced in all general respects like to those constituting the earlier subjects of attack, continued for months free from fever, in cases where any great communication with the immigrants did not occur; and this too in instances where famine had sorely prevailed, and had induced its obvious and direct results in the production of scorbutic and other ailments plainly attributable to it. Every one who was much in contact with the sick took the disease; nurses and other domestics resident within fever hospitals systematically became affected; medical men holding union and other appointments that placed them amidst the disease—and such medical men only—were attacked everywhere in unusually large proportion; Roman Catholic clergymen, who have a world-wide fame for their zeal in times of pestilence, perished most lamentably through attendance upon their sick; in Liverpool, upwards of half the number composing the staff became victims of the fever. But the disease never extended to localities, nor to classes, nor to individuals, where communication with infected persons was avoided. Moreover, the severity of any attack would, *cæteris paribus*, generally maintain some notable proportion to the probable intensity of the poisonous imbibition on the part of the subject. The fatality amongst Catholic priests was perfectly unparalleled in some places; we have been informed that, in Liverpool, sixteen became affected; and, out of this number, ten died. We cannot but ascribe this most extraordinary rate of mortality to the fact, that Catholic clergymen come more closely into contact with the infected persons than most others who have to do with them; in auricular confession, the priest must receive the very whispers of the enfeebled penitent, especially in hospitals, and he is thus exposed directly to the worst concentration of the morbid effluvia, existing most likely in the pulmonary exhalation. Indeed, in whatever aspect the events of the late epidemic be viewed, the evidence accumulates, that maculated fever is eminently contagious; and if it be right to assume that smallpox, measles, and scarlatina extend themselves by contagion exclusively, we think it safe to maintain that a like proposition obtains in the present case. The grounds of belief appear to be very much the same in each instance.

That some degree of immunity against repetition is procured by an attack of this fever, seems now to be very generally admitted. The fact is constantly noticed about the hospitals in Scotland and Ireland; and, in this country, we have ourselves observed many circumstances strikingly corroborating it. During the epidemic, a great majority of those who had habitual communication with the sick took the disease; yet we know of several instances where the most assiduous and uninterrupted attention to fever patients was kept up for months, without the disease being contracted; but this was almost always in persons who had had maculated fever already. In one instance—that of a colleague—the affection fully developed had been experienced in Ireland, at twelve years of age; fifteen years later, within our own observation, the subject of such attack devoted more time to attendance upon fever patients than any other medical man of our acquaintance, and yet did not take the disease; though, several times, headache, nausea, and malaise, appeared to threaten something of the kind. We witnessed many relapses—many second attacks of *fever*—but rarely the special characteristics of the epidemic under such circumstances; and where, in second attacks, maculæ became developed, we had in no instance evidence that they had been present in the first. Relapses seemed to result from causes ordinarily operating to induce them in convalescence from acute diseases, as from prematurely taking food, from undue exertion, and from exposure to cold; febrile symptoms would, in this way, be excited, which, however, generally subsided in a few days. When such relapses interrupt convalescence from other exanthematous fevers, as frequently is the case, we do not say the patient has had a second attack of smallpox or measles; neither in the parallel case under discussion ought we to say that *fever* (if maculated fever be meant) has twice been had.

Dr. Tweedie, physician to the London Fever Hospital, is stated to have had fever three times; and Dr. Christison, of Edinburgh, six; but what type of fever? There may be several species, etiologically distinct. At any rate, a simple continued fever may sometimes be provoked through exposure to cold, in persons whose general health and nervous energies have been reduced much below par; the febrile movement may last from a day or two to several weeks; such cases, however, we should as soon consider to be *scarlet* as *maculated* fever. We have ourselves been several times the subject of such fevers; once, at the close of our student's career, after working somewhat hard, we caught cold from sitting several hours in a draught; a slight sore throat was the immediate consequence, attended with a little fever; the throat got well in two or three days, but the fever increased and lasted for a fortnight, and was followed by great loss of flesh and strength. There was no epidemic at the time; we had, so far as we could tell, been exposed to no contagion; we had for some weeks, indeed, been endeavoring to recruit by a run in the country; so that we cannot regard the fever in question as in any way identical with the later attack we sustained. Certainly, we never esteemed it as furnishing even a probable immunity during the late visitation, and we were among the first to break down upon systematic exposure to the contagion.

We had, in the early part of last spring, a somewhat striking illustration of the essential distinctness of simple fever that has no origin in contagion, from the fever that was epidemic. At this period the hospital with which we are connected was occupied almost exclusively by the immigrant Irish, the disease not having yet extended itself to the fixed population; alarm of fever was everywhere prevalent. The medical attendant of an English servant, in an English family, had pronounced his patient to be labouring under fever (and in some sense she was); hereupon, she was removed to the hospital forthwith. In this case, there were, for the first fortnight, no maculæ, nor other peculiarity of the epidemic; at the expiration of this time, the febrile symptoms began to decline, when suddenly a relapse seemed to occur; an exacerbation of fever took place, and presently a copious eruption of spots became manifest, and the young woman went regularly through the stages of this second fever, clearly referable to contagion. In the same way, we had two or three times the opportunity of seeing patients incipiently convalescing from the epidemic fever, take smallpox from contiguity to some person labouring under this latter disease. We have related to some of our professional brethren the fact just stated, and learn that other facts of a like character have been noticed. Dr. Bartlett, to whose excellent work we have before referred, states it as an observation already made, "that a second attack of the disease is very rarely witnessed when the first had been severe, or *when it had been attended by an abundant eruption.*" This latter qualification corresponding so exactly with our own experience as already set forth, we have rendered the passage in italics.

We extract the following literary references from the work in question, on account of their being so strikingly corroborative of conclusions to which we had ourselves been led by our own observation; the quotation runs continuously with the brief extract already made.

"And there seems good reason for believing that such is the case. Dr. Barker, in his Report of the Cork Street Hospital, Dublin, states that he has for some time entertained the opinion that sufferers from fever attended with the petechial eruption,* if they are not altogether secured by it from a second attack, are not at least so liable to it as those who have had a fever of an ordinary kind. 'Though I have frequently made the inquiry,' he adds, 'I have not found a patient in whom this symptom was distinct, who had suffered from the same fever on any former occasion. But whatever may be the result of more minute inquiry, it may be asserted, that the chances of the recurrence of fever diminish in proportion to the continuance and severity of the first attack.' Dr. Bracken, of Waterford, after quoting the above, says: 'It appears to me that this opinion is supported by experience, as well as by reasoning from similar facts. Since I first observed this remark, I have kept the subject in view, and, after some attention to it, I have not been able to ascertain that more than three persons, out of many hundreds who came within my observation, have had relapse or recurrence of fever, after being previously affected with the symptoms in question.' Dr. Trotter says: 'During our extensive and long experience of the origin, progress, and extinction of contagion in ships and everywhere else, I have entertained a strong suspicion that typhous infection *very seldom* affects a person more than once in a lifetime.' Dr. Perry, of Glasgow, in a letter to the editors of the Dublin Journal of Medical Science, says: 'I have for some years entertained the opinion, founded upon an extensive series of observations, that contagious typhus is an *exanthematous disease*, and is subject to all the laws of the other exanthemata; that, as a general rule, it is only taken once in a lifetime, and that a second attack of typhus does not occur more frequently than a second attack of small-pox; and, judging from my own experience, less frequently than a second attack of measles or scarlet fever.'" (Dr. Bartlett's History, &c. Second Edition, pp. 216–17.)

Of the many second attacks that we witnessed, in the way of relapse, during the epidemic of last year, we never noticed all the specialities of maculated fever to be twice displayed. Still, that fully developed instances *may* be exhibited in the same individual more than once, analogy would not permit us to doubt, even though there were no direct evidence upon this point. Scarlatina is sometimes repeated; we have ourselves twice attended the same child in measles, at an interval of only a year; and Dr. Baron, of Cheltenham, in his Vaccination Report, relates authentic instances of *third* attacks of smallpox. And we are confident that, if the distinctive peculiarities of maculated fever were more generally a subject of close observation, immunity after attack would be found to obtain very much in the same proportion, and to be qualified much in the same way, as in the other exanthematous fevers.

The general characteristics of the febrile movement, in the disease now under discussion, have much also in common with others of the exanthemata, particularly in *system* and *periodicity;* and, although in these respects great variation is observable, we do not think that it is to a greater extent than is witnessed in the allied maladies. It is true that systematic writers, in portraying the actual course of variola, rubeola, and scarlatina, set forth the mode of invasion, the periodic changes, and the duration, in a somewhat definite manner; bnt, nevertheless, when these diseases are observed upon a large scale during epidemics, deviations from the assumed *normal* course are constantly seen. The deviations in question are, we think, rightly ascribable in some cases to peculiarities in the epidemic constitution, and in others to some natural or induced idiosyncrasy on the part of the patient. What we deem to be the *normal* state of things in maculated fever, has been already stated; and we have adduced what we consider to be the suitable conditions, under which the natural history of the disease should be traced,—conditions of previous health and comparative youth. If the course, periodicity, and duration of exanthematous fevers are to be deduced from hospital patients indiscriminately, no satisfactory conclusions will be obtained. Disturbing influences of all kinds are at work,—antecedent ill health, local complications, unconnected with the general malady, cachexia, organic mischief, readily superinduced and keeping up febrile symptoms beyond the customary term; all these things, we apprehend, lead to irregularity in the development of all periodic diseases; and when we hear, or read, of fever continuing for some weeks, we cannot but attribute the fact to some such circumstances as those just cited. But, with respect to deviation from the ordinary type, maculated fever is but conditioned as others of the class, and influenced most likely by similar agencies. The severer forms of the disease—aggravated typhus—may be regarded as the analogue of confluent smallpox, of scarlatina anginosa, and of the more virulent type of measles; and the imperfectly developed attacks—the mere febricula of a few days' duration—may be likened to the modified smallpox, to the slight febrile sore-throat, without any rash, so often to be met with when scarlatina is epidemic, or to the mere catarrhal symptoms, sometimes observable amongst children when measles prevail, supposed with good reason to be rubeolar in their essence.

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* Doubtless, meaning the maculæ proper.—Rev.

We have already stated our own induction, that fifteen days constitute the proper duration of the febrile symptoms in maculated fever. This period, however, will sometimes be exceeded. When it is so, however, the cause may generally be detected in some lesion of the abdominal viscera or of the brain. Exactly the same thing is to be seen in other exanthematous fevers; in measles, the febrile excitement is sometimes kept up by supervention of pneumonia, and, in scarlatina, by irritation, resulting from the state of the throat and contiguous glands.

Although the conventionalities of medical men may render the term *typhus fever* in some degree intelligible, we should ourselves much wish that it was abolished, as expressive of a species, however appropriately it might be retained to signify a certain adynamic or ataxic *condition*, occasionally arising in the progress of all fevers, and to which the modern *usus loquendi* affixes the destination of *typhoid* state. It is certain that maculated fever, fully developed, may pass through its several stages, and the patient never become typhoid; whilst other fevers, exanthematous or otherwise, will in their worst forms become so. This anomaly has been felt by some writers, and it has been proposed to speak of the "true typhus," or the "exanthematous typhus;" but, as we say *scarlet* fever from the state of skin in one case, why not follow the example of some of our Irish brethren in another, and systematically say *maculated* fever? Or, if there were no objection to the employment of a new term, why not follow out the analogy of scarlatina, and speak of *maculina?*

It remains for us yet to particularise a little further with regard to the cutaneous eruption, and to show that, in all its characteristics, it maintains as determinate a relation to its proper fever, as the cutaneous affection in corresponding maladies. First, it appears *definito tempore;* secondly, it manifests itself electively on some parts of the surface rather than on others; and, lastly, the spots have certain fixed physical characters. In all these respects, there is a strict correspondence with the exanthematous state of skin in measles, smallpox, and scarlet fever; for the sixth day forms the one upon which the eruption of maculated fever shows itself, almost as regularly, we think, as the second day constitutes that upon which the rash of scarlatina appears, the third that for smallpox, or the fourth for measles. In these latter diseases, it is known by all observers, how frequent are the deviations in this point of view; and the same irregularities are noticeable in the other case. Again, that as the smallpox eruption most generally attacks the face in the first instance, then the neck, trunk, and extremities: as the measles pursue very much the same course, engaging the trunk probably in a more especial manner; and as the efflorescence of scarlet fever is commonly in large patches about the extremities and neck, and but rarely involves the face; so the maculæ, in the present fever, are seen first about the neck and chest, subsequently and more rarely on the abdomen and upper extremities, still more rarely upon the legs, and scarcely ever upon the face. The rule, and variations from it, seem to us to obtain about equally in each of the above instances. We have said that the spots in maculated fever have certain fixed physical characters, in themselves indeed constituting a specific exanthem, irrespective of the question regarding the speciality of the allied fever; they are not *petechiæ* (according to the present restricted employment of this term); they are not *sudamina;* nor are they (we incline to think) the *taches rosées* of Chomel and other French writers. Petechiæ are purple, persistent, not disappearing on pressure, whilst maculæ are red, recede and reappear, and in many cases vanish on pressure; sudamina, which will exhibit themselves about the sides of the neck, shoulders, and axillæ, in several acute diseases, are vesicular, but maculæ do not raise the cuticle. Having ourselves had no recent opportunity of observing the *taches rosées*, as described by the French writers, we prefer to take the following account of the difference between them and the present efflorescence from Dr. Bartlett's work, rather than present the reader with any descriptive contrast of our own:

> "This eruption," says Dr. Bartlett, referring to maculæ, "differs in many respects, and in a very striking degree, from that of typhoid fever. Its color, especially after the second or third day of its appearance, is that of a duller and darker red. The spots are of a dun, dusky, purplish hue; in some cases, they become almost black. They vary in size, from that of a minute point to a diameter of a line, or even of an eighth of an inch. They are less regularly circular or oval than the rose spots of typhoid fever. They are not elevated above the surrounding skin, and disappear but very partially, or not at all, on pressure. They are almost always much more numerous than the spots of typhoid fever, covering, in many cases, the entire trunk and extremities."

We have recently seen a lecture on the epidemic fever, published in the 'Medical Gazette,'* by Dr. Laycock, in which that accomplished and acute physician appears to doubt the specific character of the exanthematous rash. He observes: "Seeing that this eruption has followed upon the loss of the potato crop and a great deficiency of food, if not an actual famine, we must, I think, attribute its appearance to an altered condition of the fluids consequent on the altered or defective diet of the people, and not look upon it as a specific exanthem." But, in opposition to this, the spots were generally the most noticeable and the most persistent, exactly in those persons in whom the dietetic causes in question had had no operation. Almost universally, when a person in tolerably *good condition* took fever, maculæ became developed; medical men and clergymen had always the eruption, so far as our own experience went; we ourselves had a copious, bright red efflorescence, and yet, during the whole period of scarcity, never ceased to employ the potato as usual. It was most frequently in low, scorbutic subjects, that the distinctive spots were absent; petechial *puncta* and *ecchymoses* were common enough in such individuals, but not as a production of the febrile movement.

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* Nov. 5th, 1847.

We conceive, then, that our position is made good, that every feature which attaches in common to the recognised exanthematous fevers, attaches in like manner to the fever under consideration; it is contagious, an attack greatly diminishes susceptibility to its future influence, the febrile movement is systematic and periodic in its character, and the accompanying eruption on the skin is specific.

In partial objection to this view, we have heard it asserted that, as in the febrile exanthemata a remission of fever occurs on development of the eruption, and not so in maculated fever, a general characteristic of the class is hereby wanting in this latter case. In regard to this argument, we dispute the premises; it is true that, in variola, some abatement of fever takes place upon first appearance of the papulæ, but, as these advance to the pustular condition, an aggravation occurs; in rubeola very often, certainly, the febrile symptoms become more intense on development of the eruption; and, to speak from our own experience, we should not say that, in scarlatina, the fever, as a rule, subsided in any degree on the coming out of the rash.

An opinion has prevailed to some extent, that the epidemic of last year was a "famine fever," as if it had been some new disease, generated by the Irish famine, and hence by some denominated the "Irish fever." Our own notions concerning the connexion between famine and fever are already before the reader. In so far as the accumulation and concentration of its proper contagion was the result of *Irish* immigration, induced by the terrible *famine* to which the destruction of the potato crop led, the fever may rightly enough have been designated an "Irish" and a "famine" fever; but, as these terms suggest what we deem to be erroneous notions regarding the origin and true character of the epidemic, we, for our parts, would discard them altogether.

We do not, for many reasons, think that maculated fever has any especial and exclusive association with Ireland; its proper symptoms, we are certain, have, in this country, been overlooked very often. It has prevailed in some of the large towns of England at least since 1837-8; the epidemic fever, in these years, being distinctly maculated. Since that period, sporadic cases have prevailed uninterruptedly within our own sphere of observation; we have before us the private note-book of a most intelligent physician, connected for some years past with a fever hospital in an English town, wherein, for the whole period, records of maculæ occur as characterizing particular cases; yet we have conversed with old and observant practitioners in the same locality, to whom the peculiarities of this fever had never been a subject of notice before the late epidemic.

Notices may be found scattered in various writings, rendering it highly probable that this type of fever has been in existence for centuries. On referring to accounts of several epidemics, we are made tolerably sure that authors are describing the same fever as that by which we have so lately been afflicted. Dr. Laycock, in the Lecture before referred to, has made several citations that bear upon this point; in looking over Dr. Bartlett's work, we find references of like import: we will avail ourselves of these. The following is from Dr. Laycock:

> "Winteringham, describing a 'putrid fever,' which was epidemic in York, in 1728, mentions the appearance in some cases of 'red spots, not unlike flea-bites, on the breast, sometimes interspersed, so that the skin had a marbled appearance.' Hecker describes an epidemic which broke out in Granada, after scurvy had been extensively epidemic throughout Europe, and which, extending to Italy, was described by Fracostoro. He terms it a 'petechial fever,' but it was also termed the Lenticula, Puncticula, or Peticula, Febris stigmatica, and Pestis petechiosa, on account of the appearance, 'on either the fourth or seventh day, of red spots, like flea-bites, or larger, resembling lentils, upon the arms, back, and breast. . . . . . . The patients are described as 'lying upon their backs with an oppressed brain, their senses blunted, and, in most cases, delirious and gloomy; muttering, with bloodshot eyes, commenced on the fourth or seventh day;' that is to say, symptoms of arachnitis. Some were lethargic, others suffered from sleeplessness. . . . . .
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> "Cardano (writing before Fracostoro) described a similar fever, prevalent as a plague in Milan, under the name of pulicular disease, from the resemblance of the eruptive spots to a flea-bite. Rasori has described a similar epidemic, in his 'Storia della Febbre Petechiale di Genova,' an exanthematous fever which devastated that city during the years 1799–1800, when besieged by the French, and when the inhabitants were famished."

Huxham (quoted by Dr. Bartlett) says: "We frequently meet with an efflorescence, also, like the measles, in malignant fevers, but of a more dull and lurid hue, in which the skin, especially on the breast, appears, as it were, marbled or variegated." Sir John Pringle had also remarked the spotted skin in some fevers of his day; his words (quoted also by Dr. Bartlett) are as follows: "There are certain spots, which are the frequent, but not inseparable attendants of the fever in its worst state. These are less usual on the first breaking out in the hospitals: but when the air becomes more corrupted, the spots are common. They are of the petechial kind,* of an obscure red colour, paler than the measles, not raised above the skin, of no regular shape, but confluent. The nearer these spots approach to a purple colour, the more ominous they are, though not absolutely mortal."

As, however, it was not our purpose, in undertaking the present article, to advance to the general history of this fever, we will not multiply citations having a bearing upon this point. The extracts just given may suffice to show, that the recent epidemic was neither a new, nor exclusively an Irish, disease. Had there been much doubt upon this matter, a very little trouble in the way of independent research would, we conceive, have enabled us to remove it.

We shall now conclude with a few observations upon the principles of treatment in maculated fever. From the accounts that have been furnished of our own proceedings in this respect, it will be perceived that we are decidedly adverse to any very active interference, and that we have no confidence whatever in specific modes of management. We conceive, indeed, that a leading duty on the part of the practitioner is to avoid risk of disturbance in the natural processes, and to employ powerful remedies but exceptionally, and in relation to the circumstances of individual cases. The records and statements of the most experienced physicians, in all countries, testify very strongly in favour of negative rather than of positive measures, in the treatment of nearly all the principal forms of continued fever; and with respect to specifics—curative or palliative—the profession, at the present day, are very unanimous in the conviction, that there are not any which can be relied upon. Our own ideas are forcibly embodied and expressed in the following extract from Dr. Watson's lectures, in which Pitcairn is quoted:

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* At the period when Pringle wrote, the term "petechiæ" was not so restricted in its application as at present; the description that succeeds, leaves no doubt that *maculæ* are meant.—Rev.

> "Pitcairn was asked what he thought of a certain treatise on fevers, and declared, 'I do not like fever curers. You may *guide* a fever; you cannot *cure* it. What would you think of a pilot who attempted to quell a storm?' Either position is equally absurd. In a storm you may steer the ship as well as you can; and in a fever you can only employ patience and judicious measures to meet the difficulties of the case."

Whatever may be the extent or the limits of that property in the organism which has long been denominated the *vis medicatrix naturæ*, its influence in fever should be studied and respected in an especial manner; and whatever is recommended or prohibited to be done by the physician, should certainly be at all times conditioned by the laws affecting this recuperative energy of nature.

If the general argument be good, which maintains that maculated fever properly ranks with the exanthematous class, the primary lesion, we conceive, must consist in vitiation of the circulating fluid; and, the matter being so considered, restoration to health must depend upon the proper depuration of the blood. In this view of the case, should we not deem the great agent of cure to be the febrile reaction itself, in so far, at least, as this may promote the eliminative action of the excretory functions? If this be so, we at once recognise the importance of *time* for elimination of the specific virus and the products of its action, that the fever may subside; and we see the uselessness, moreover, of attempting, by some *coup*, to cut it short, when once fairly developed. It seems, indeed, now to be generally admitted, that sound practice will never, without some urgent or striking indication, interfere with a febrile movement that, in its nature, is obviously remedial.

For our own parts, we consider the specific poison of exanthematous fevers to hold some such relation to the blood-vessels as certain indigestible substances hold to the alimentary canal; and the febrile reaction in the one case, we would liken to the vomiting or diarrhœa in the other. Now, what is allowed to be good practice under the latter circumstances? Why, clearly, to interfere as little as possible with the gastro-enteric reaction, so long as it seems to be fulfilling the intention of expelling the offending matter; but where, from any cause, the natural powers appear to be inadequate to the purpose, or where, from local or general debility, there is reason to fear for the tissues, under excess or inordinate continuance of the movement in the alimentary canal, then to supply remedies that tend to arrest the destructive action.

A common principle of treatment is plainly applicable to all exanthematous fevers; on the one hand, to maintain by stimulants the febrile movement, where the unaided powers are unequal to the completion of the depurative action, in which case the patient might otherwise sink; and, on the other hand, to mitigate any undue force in the febrile reaction; and this, rather by judicious hygienic, than by *heroic* measures. Our detailed views in these respects will be deduced from the account afforded of our own proceedings in treatment. Nevertheless, we will here advance some additional remarks upon one or two points.

Although the head and the intestinal canal very frequently exhibit, after death, signs of antecedent inflammation of a destructive character, we would yet submit that such conditions are improperly treated by active depletion. Inflammation, in this class of cases, undoubtedly exists, but it is nearly always of an *asthenic* type; and, in some circumstances, it will be more likely to disappear under the influence of nutrition and stimulation, than of leeches and starvation. If, in the cases referred to, reduction be carried further by artificial means, the patient will often die at once; whereas, if he be kept alive for two or three days by appropriate sustenance, the curative powers of Nature will recover their energy, and the patient get well. This we have seen, again and again; and a most important practical fact it is, for, as Dr. Watson forcibly observes in his 'Medical Lectures,' "we do not so much *cure* these exanthematous maladies, as keep our patients alive while they recover;" and whether, in malignant fevers, the peculiar poison, according to the views of Dr. William Budd, exhibit an elective affinity by determination to the mucous membrane of the intestines, or to the cutaneous tissues, or to both equally, we would contend for identity in the indications of treatment. Who would expect by depletion to prevent ulceration of the integuments, when, from some erythematic condition of the skin, such a result appeared imminent.

We had intended to comment at some length upon the treatment by tartar emetic and opium, in the *delirium tremens* condition, so often to be noticed in the dominant stage of maculated fever. We must pass the subject by, however, with only a few remarks. Dr. Graves, who first employed and recommended the treatment in question, states, in his 'Clinical Lectures,' that the medicine should not be given, in the bulk of cases, before the 10th or the 11th day. But then, as will have been seen from our account, the 11th or 12th day forms precisely the period at which, in cases that do well, the symptoms, in our experience, at least, have subsided spontaneously; and sure we are, that we have seen instances quite as marked as those of Dr. Graves, where recovery took place without any *positive* treatment. Only on one occasion did we venture upon Dr. Graves's plan. The case occurred in private practice; the patient was under thirty years of age, and in good general health before the attack,—an instance, in fact, wherein, according to our own notions, the periodicity of the disease becomes matter of demonstration; altogether, we thought the case a very favorable one for testing the influence of the remedy. All the symptoms described by Dr. Graves became manifest about the 8th day, and, seeing no contra-indication, we at this time exhibited the tartar emetic and opium in a form prescribed by Dr. Graves himself, in his Lectures; anxious to see if the dominant stage would be cut short, and how far the periodicity of the disease, as noticed in other such cases, would be interfered with. Some broken sleep succeeded, but no absolute mitigation of the symptoms; on seeing which, we left it off, after 24 hours' use, curious to notice the result of such disturbing influences. The febrile symptoms were, by at least two days, of longer duration than we had been accustomed to observe in seemingly analogous instances; and we could not help believing that our interference, by an active and powerful remedy, with the depurative efforts of the system, had protracted the process. Certainly, the cases related by Dr. Graves were generally more prolonged than were those of an apparently similar character, that occurred in our own experience; we do not, however, deduce any practical conclusion from this circumstance. To do so would require very extensive data.

We must now close. In the preceding sketch of the history of the last year's epidemic fever, and of its characteristics and treatment as witnessed and practised by ourselves, we have had scrupulous regard to accuracy in the statements we have advanced; and it has been in pursuance of such purpose, that we have detached our deductions and speculations from the mere record of circumstances. In reviewing what we have written, however, we are rendered very sensible of many and important omissions in the affair of fact, and of much incompleteness in the general argument. Many things that we had intended to say, have been left unsaid; and numerous illustrations of our views, we have been compelled to forego. The limits, however, within which we have unavoidably been circumscribed, necessitated these omissions; and, with respect to our argument, let it be regarded, not as decisive or demonstrative in itself, but as a series of suggestions that may serve as *cogitanda.*

ART. II.

Observations on some of the Parts of Surgical Practice; to which is prefixed, an Inquiry into the Claims that Surgery may be supposed to have for being classed as a Science. By JOHN P. VINCENT, late Senior Surgeon to St. Bartholomew's Hospital. With a Plate.—*London,* 1847. 8vo, pp. 364.

IT is pleasant to meet with a book, whose publication can proceed from no other than the most laudable motives;—a book, whose author can have no other end than that of giving to the world the benefit of the matured results of his experience and reflection; and one to whose perusal we can apply ourselves, with the assured conviction that no desire of self-elevation can be operating to produce an unworthy depreciation of the merits of others. And it is further agreeable to be spared the regret, which we are continually made to feel in the discharge of our critical duties, that the writers of medical books in general are not better qualified, by previous education, and by philosophical habits of thought, for communicating to the public the ideas which they have derived from their own professional observation or from their reflection on the facts collected by others.

The whole tone of Mr. Vincent's treatise is characteristic of the gentleman and the scholar; and the qualifications of the latter are more parti-
xhibited in the Introductory Essay, to the examination of which

we shall first proceed. We had expected from the title of this essay, that its object was to inquire whether surgery, properly co-called, has *any* title to the designation of a *science*, or whether it should not properly be termed an *art*. This question was fully discussed in the pages of one of our predecessors;* and we think that it was there satisfactorily shown, by the analogy of other related sciences and arts,—e. g., astronomy and navigation, chemistry and dyeing,—that medical and surgical therapeutics must be regarded as *arts*, being the *practical application* of the *principles* of physiological and pathological *science*. This distinction has long been recognised; for not only is the essential difference between the *ars medendi* and the *ratio medendi* made evident in the writings of the oldest medical authors, but, to come nearer to our own times, we find in the University of Edinburgh (dating back, we believe, from the period of its origin) one professorial chair for the Institutes or theory of Medicine, and another for its Practice. The former was considered to embrace the sciences of physiology and pathology; until the establishment of a separate chair for the latter division rendered it desirable that the teachings of the Professor of the "Institutes" should be restricted to the former. This division has not been so formally made in the teaching of surgery; but a very little acquaintance with surgical literature, or a very small amount of reflection on the objects of a surgical education, suffices to show that the knowledge of the surgeon, who possesses a competent acquaintance with his profession, consists on the one hand of physiological and pathological principles, and on the other of the best methods of carrying those principles into practice, —together with a great accumulation of *empirical* facts, that is, of facts derived from observation and not yet brought under the domain of scientific principles, which must have a considerable influence on his treatment of the greater number of the disorders which he is called upon to cure. The *principles* and the *practice* of surgery appear to us to be as distinct as the principles and practice of medicine; and whilst the practice of surgery is distinct as an art from that of medicine (though it is somewhat difficult to show precisely where the line of division should be drawn), the principles upon which both these arts are founded are essentially the same, and belong to the sciences of physiology and pathology. For example, there is no difference in the nature of the inflammatory process, as contemplated by the surgeon and the physician; a scirrhous growth is essentially the same whether it occurs in the mamma or in the stomach, and thus becomes the object of surgical interference or of medical treatment.

The purpose of Mr. Vincent's inquiry, however, is not exactly coincident with that to which we have referred. He classes *principles* and *practice* alike under the general designation of surgery; and proposes to consider whether their present condition entitles surgery to rank as a science, or whether they are not rather of a nature to give to it a condition but little elevated above empiricism.†

The first part of the essay is occupied with the somewhat abstract inquiry into the real nature of scientific truths, and the mental processes which are required for their attainment. In this inquiry, Mr. Vincent's extensive reading is made evident by the very apt quotations which he introduces; and he shows great acuteness in the analysis of the different habits of thought displayed by minds of different classes. The following is one of the many valuable remarks which we should gladly quote did our limits permit:

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* British and Foreign Medical Review, vol. vi. p. 98 et seq.

† We use this term, not in its ordinary sense, but in the ancient acceptation, founded upon its etymology; referring by it to the system of practice which professes to be founded directly upon experience, without reference to scientific principles.

> "When the understanding is easily satisfied, owing either to the low order of the individual mind, and inability of it to mount higher than the simple perception of first impressions, it gives its own assent to the completeness of the intelligence it has received, and by such an easy process the judgment is formed. This sort of judgment is not only very often low in the scale of intellectuality, but may descend to a degree that excludes the very application of the name of judgment. Opinion is so much the guide of medical conduct, that it is sometimes actually regarded and valued as much as a sound judgment." (p. xix.)

The following is Mr. Vincent's view of the present state of surgery, philosophically considered:

> "Thus, in fact, it may be truly declared that the practice of surgery is little more than a collection of opinions, unstable and fleeting, and generally prescribed by those whose position in the surgical society of the day gives them tone. That opinions have but a small value in a scientific point of view, is proved by their unstableness, and by the readiness with which they are changed. Thus the practice of surgery is apparently not only taught, but pursued, by prescription, and but little on principles of science.
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> "The qualifications of the practitioner who may be placed within the pale of the preceding description, are such as Bacon entitled *empirici;* the knowledge required for such practice is heaped up, and cast, as it were, into the memory, and then any part is taken out separately as it may be wanted, and for a limited purpose, without any regard to all the conditions under which it may exist in relation with other things. This cannot be called scientific practice." (pp. xxix-xxx.)

We fear that this estimate of our author regarding the mental habits of a very large proportion of the practitioners of our profession, is but too well founded; and we believe that he is correct also in the idea that, on the whole, the class of surgeons must be placed below that of physicians in this respect. He considers that the difference may partly be attributed to the different characters of the phenomena with which they have respectively to deal; the morbid processes which are obvious to the senses in the case of the former, being only deducible from the symptoms by an effort of reasoning in the latter. But whilst we admit that there is some truth in this remark, we think that Mr. Vincent has more rightly judged in attributing the result in great part to the superiority of the previous education, which is received by physicians taken as a class, over that of which the general body of surgeons have had the advantage. And it is to an improvement in the systems of early training and mental discipline, rather than to any great changes in our plans of special education, that we look for an elevation of the professional mind. We speak from no small amount of experience, when we say that the value of the special education depends far more upon the character of the student than upon that of the teacher. The very best instructions, falling upon a barren soil, are perfectly fruitless; whilst the earnest and discriminating student is comparatively little dependent upon the guidance of the teacher under whom he may chance to be placed; but draws that information from the dissecting-room, from the wards of the hospital, and from the works of the masters of their respective departments, which the inefficiency of the lecturers on whom he attends, or the unskilfulness of the practitioners whom he follows, may fail to afford him. We believe that in almost every walk of life, the knowledge which a man most prizes is that which he has gained by his own exertions; and that those individuals in whom the advantages of a superior education are most evident, will be found to have derived their superiority, not from the amount of direct instruction which they received and retained during the period of pupillage, but from the mental discipline to which they were subjected, and from the habits of correct observation and correct reasoning which were formed in their minds. This view harmonizes well with Mr. Vincent's idea of a scientific practitioner as contrasted with the empirical. "It is in the intellectual character of the mind, and not in the quantity of the knowledge he possesses, that we are to look for a scientific practitioner."

It is not every one, however, who professes to base his practice on principle,—in other words, to make the ascertained laws of physiology and pathology the foundation of his treatment,—who is really entitled to the designation of a scientific practitioner. For it must be admitted by every candid inquirer, that we as yet know far too little of those principles, to enable us to adopt them by any means invariably as safe guides. In a very large proportion of cases, we are not yet in the possession of any principles of sufficient fixity and definiteness, to enable us to steer an undoubting course; and in very many others, the peculiar circumstances of the case so modify their application, that they might seem of little value. And thus, in fact, it has come to pass that many a routine practitioner has been able to point to his success in practice as at least equal to that of his more scientific brother; and has had apparent ground for his assertion, that a knowledge of all that has been attained in physiological and pathological science is of little or no value by a patient's bedside, in comparison with an aptitude for the recognition of symptoms, and a readiness in the application of remedies to combat them. It can scarcely be denied, that a blind adherence to principles of supposed invariability, is as likely to lead to dangerous errors in practice, as the absence of all knowledge of such principles. Hence the really scientific character is as much shown in the discernment of those cases, to which no known principles will apply, and which must therefore be treated according to the dictates of experience alone, without the guidance of any rationale,—or in the discrimination of peculiarities, which render necessary a modified application of his principles,—as in the clear, straight-forward line of practice which can be pursued, when those principles can be unhesitatingly applied, with the full confidence that no other plan of treatment has the least claim to adoption. And the want of that character may be plainly discernible, even when the practice is professedly based on scientific principles; if these principles be not truly applicable to the case in question, or the individual peculiarities by which their application should be modified be not taken into the account.

How, then, is that combination of mental habits, which we have designated as the scientific character, to be most surely developed? To us it appears that the foundation of it must be laid in the preliminary education; and that the cultivation of the physical and chemical sciences, if rightly directed, is the most valuable discipline which the mind of the destined for the medical profession can receive, after that still wider foundation has been laid, in the study of languages and the relations of numbers, space, &c., and in the general observation of external nature, which must constitute the basis of all sound education. The tendency of the well-directed study of physical science is to exercise alike the observing and the reasoning powers; to encourage, indeed to require, a definite apprehension, whether of facts or principles; to be constantly bringing into practice both *inductive* and *deductive* operations of the intellect; and this with regard to a class of subjects, in which errors, whether of observation or of reasoning, may for the most part be soon detected, so that any long persistence in an erroneous notion is rarely possible. There is every gradation, too, from the most exact and complete of sciences, in which each phenomenon can be predicted, with confidence, because simple general laws have been attained, that are applicable without hindrance or modification to each individual case; to those which most approach the sciences of organization in the very limited character, and in the uncertainty of application, of the principles which have as yet been educed from the phenomena they present. And by following somewhat of this gradation will the mind of the student, as it appears to us, be best fitted for the pursuits on which he is to be engaged during the period of his strictly professional education; whilst the prospective value of the actual amount of knowledge which he gains by this preparatory training is surely not to be left out of view, especially when the barrenness of all such direct benefit from the construction of Latin verses, or the study of the Greek dialects, is duly considered.

We must not any longer dwell upon the topics discussed in this very interesting preliminary inquiry; but must content ourselves with adding that the essay will well repay the attentive perusal of all who have at heart the elevation of professional knowledge, or the higher culture of their own powers; and that it is everywhere pervaded by a combination of sound philosophy with good common sense.

The treatise itself is a collection of desultory observations; and in our progress through its pages we propose to bring under the notice of our readers such subjects as may seem to possess interest, and to exhibit the bent of the author's mind and the character of his practice.

Mr. Vincent, at the commencement of his observations, makes some reflections of an interesting character on the relations of the muscles and their associated actions; chiefly with a view to guide the surgeon in adopting the best positions of the limbs or of the entire body, in injuries and other states where it is necessary to guard against the interference of muscles in the treatment. He remarks that there is a law which is to be constantly attended to in practical surgery. It is, that the relaxation of muscles is to be effected by attending to their position when they are required to throw out their strongest exertions; and not, as usually is supposed, by approximating their attachments. As an exemplification of this law, he gives—

> "That of the fracture of the humerus just below the attachment of the deltoid muscle. We find in the first days of this kind of fracture, that owing to the disturbance which the absence of the integrity of the bone gives to all the muscles engaged in moving it, the deltoid will raise the upper portion and give great appearance of deformity to the limb, and apparently baffle the aim of the surgeon

to get a straight union; and this will be the case if he brings the lower portion away from the side of the body to meet in apposition the upper projecting part. Instead of doing this, he has only to take measures that the lower end should hang easily by the side of the patient, and he will find the upper portion fall into the straightest position he could possibly desire. When the upper arm is hanging quietly by the side of the body, then the deltoid is in a state of least possible action. In this way of reasoning it is that I find this fracture unite in a remarkably straight form. Upon the principle of relaxing muscles by approximating their attachments, the very reverse practice to this would be adopted, which is, the raising the arm to the level of the acromion; this I have seen done in such cases, but most fruitlessly." (pp. 11–12.)

The chief reason why the plan of elevating the arm in the treatment of the fracture here alluded to does not answer is, because the position is an inconvenient one, and cannot be steadily maintained, unless the patient keeps his bed; and, besides, we lose the advantage of the weight of the limb, which tends to prevent overlapping of the ends of the bone. In the treatment of fractures of the patella and olecranon, surgeons constantly act on the principle of relaxing the muscles by approximating their attachments with good effect.

Mr. Vincent makes some sensible observations on the influence of the muscles, first in producing a dislocation, and, secondly, in contributing to the reduction of the bone. He notices the want, in old dislocations, of the consentaneous action of the muscles, which, when the head of the bone is brought by the efforts of the surgeon into a situation in which the muscles can all act, in recent dislocations draw the bone into its socket. He gives a case of dislocation of the thigh of six weeks' duration, in which he adopted the usual plan of extension from a fixed point, and readily brought the head to the natural range of the joint; but no contrivance could shoot the head of the femur into the acetabulum. The assistance to be derived in recent dislocations from "a power independent of the operator, which can, in spite of the force he employs, and in opposition to the direction of it, thrust the bone into its place," has been long recognised by surgeons. And it is true enough that in luxations of six weeks' standing, we find no assistance from this combined action of the muscles. Our author states that in vain the surgeon "makes his extension day after day, and fails if he conducts the traction from a fixed point. He must now make his extension from a moveable point, that may be capable of changing its place in a circuit of considerable amplitude." A case is related in which the humerus had been luxated seven weeks, extension was conducted in the usual way for a long time, and with the fullest force, and no reduction resulted. Mr. Vincent drew the limb across the chest obliquely, and by this means returned the head of the bone into the glenoid cavity with very little effort. We wish he had told us how, in a case of old dislocation of the thigh in a muscular individual, he manages to make "his extension from a moveable point that may be capable of changing its place in a circuit of considerable amplitude."

We cannot agree with our author that little reliance can be placed on the efficacy of what are called constitutional means in the reduction of dislocations. How is it, we would ask him, that when a patient faints, a dislocation which has previously resisted powerful efforts becomes easily reduced? He remarks, "that in old dislocations these expedients are quite nugatory." Mr. Vincent, of course, has had no experience in the use of ether and chloroform,—agents which have a remarkable effect in staying muscular action, and which will be found, we believe, of valuable service in aiding the surgeon in the treatment of dislocations. We know of cases of dislocation of six and ten weeks' standing, which have been remedied without difficulty by the assistance of ether.

Mr. Vincent's practice in the treatment of fractures of the clavicle, is to avoid all bandages, which, by constraining the muscles, only excite them to undue action. He says, "All I do is, to place the patient on his back on a flat bed, and the bones immediately recede into their proper position, and uninterruptedly are kept so." We believe that in many cases of fracture of the clavicle, tight figure-of-8 bandages and other complicated contrivances may be dispensed with very well, and that union without displacement will ensue, simply by supporting the arm and preventing its weight drawing down the distal portion of the bone, as well as by subjecting the patient to the awkward ordeal of the flat bed. We are equally sure, however, that the latter plan will not obviate the displacement and disfigurements which occasionally occur in oblique fractures of this bone. Mr. Vincent's treatment of fractures of the lower jaw is also peculiar. He objects to all bandages; "the obvious method is to keep the patient's chin upon his chest, when the muscles will get quite passive, and will then in the least possible way displace the bones. The bed is the proper place to carry out this measure." Mr. Vincent must surely have witnessed cases of this injury, in which, without having recourse to contrivances "having all the merit of the thumb-screws for producing pain," proper union has taken place simply by supporting the lower jaw in a splint of pasteboard or gutta-percha, and in which there was no necessity for subjecting the patient to the annoyance of keeping in bed, with his chin confined to the chest for two or three weeks.

On the subject of fracture of the ribs, our author states, "if the fracture extends to several, there generally follows a fatal termination as the result." Is there not some mistake here? We can call to mind so many cases, in which several ribs were broken, and which yet ended favorably, that we are much surprised at the above statement.

We can bear testimony to the justice of the following remarks:

> "A correct mechanical investigation of the power of muscular structure to resist the sudden application of violence, might prove that the resistance of a muscle in action is little less than that of hard matter. In the passage of shot we know how readily this structure deflects balls; and I have been called upon to reflect on the impunity often displayed in the way the wheels of carts, &c., have passed over the abdomen of both children and adults." (pp. 48.)

Mr. Vincent objects to the reason assigned for the femur breaking in old age, viz., that this part assumes with the shaft of the bone an angle less oblique than in earlier age,—as mechanically incorrect; inasmuch as the force effecting the injury, when the neck is at right angles to the shaft, must be met by the resistance of a greater part of the bone, than when the neck is more oblique. This is certainly the case in a fall on the trochanter; but our author must be well aware that fracture of the neck of the femur in old people does not usually happen in this way. As Sir Astley Cooper pointed out, in London the injury most frequently occurs from a slip off the kerb-stone, the whole weight of the body being suddenly thrown upon the neck of the femur. This force operates with great disadvantage, when the neck passes off at a right angle instead of being oblique, and thereby causes fracture. Mr. Vincent states that the explanation of this fracture will be found in the want of that preserving power, which arises from the combined and associated action of those muscles that surround the joint. Surely, our author here carries his views in respect to the associated action of the muscles too far. At any rate, he seems altogether to lose sight of the atrophied condition which usually exists in the neck of the femur in old people, and which is, in truth, the efficient cause of the frequency of this fracture, as may easily be ascertained by a visit to the pathological museum of St. Bartholomew's Hospital, a collection particularly rich in diseases of bone. Mr. Vincent regards the position on the side—the injured one—as on many accounts the best adapted for this accident. He states that he has "treated cases by this method, and they have turned out much better in restoring the powers of the limb than the plan usually adopted." But as he afterwards admits that the age of the subjects of this accident compels us to adopt the position on the back, we need not adduce the objections we entertain to the plan which the author's theoretical views would lead him, if possible, to carry out. We must pass by Mr. Vincent's observations on the influence of muscular action in preventing the proper coaptation of the bones in fractures of the shaft of the femur in different positions of the limb, with the remark that although his theoretical views of the influence of the muscles in displacing the bones are, in the main, correct, he does not seem to take into account various counteracting causes that modify and interfere with the modes of treatment, which a regard simply to muscular action would lead the surgeon to adopt, to the extent which we should have expected from a practitioner of Mr. Vincent's great experience.

Mr. Vincent objects to passive motion in order to restore the functions of a joint impaired by injuries, observing that "there is always a tendency in the conservative process to restore the perfection of a joint; and the efficacy of this agent can always be relied on without the contrivance of artificial means." In the treatment of these cases, the surgeon must of course be careful not to exercise the joint too early; but we have met with so many instances of stiff-joint, not only from adhesions consequent upon the inflammation succeeding the injury, but also from contraction and structural shortening of the muscles, that we are convinced that the reliance entertained by our author on the conservative process is quite groundless; and we believe there are few cases in which greater good may be effected by the judicious aid of the surgeon, than those which our author is content to leave to the efforts of Nature.

We fully coincide with our author in his objections to the use of bandages in the treatment of fractures of the patella, and of rupture of the tendon of the rectus and cruræus muscles. Bandages often do harm by causing adhesions and tilting-up of the patella, and are of no service in approximating the broken parts when the limb is in a proper position.

Mr. Vincent has had several cases of luxation of the internal semilunar cartilage under his care; and, as the reduction is usually very difficult, we may mention his mode of remedying this accident, which is, "to place the patient on his affected side, with the limb bent, and then to rotate gently the tibia on its axis. In this position the joint is loose, making no pressure on the cartilage, and it has the best chance of quietly slipping into its place." Mr. Vincent enters rather fully into the consideration of the causes and treatment of fractures of the leg, and his observations on the subject are sound and judicious.

It has never happened to Mr. Vincent to have a case of foreign body in the trachea; but should such a case come under his management, he would not think of making an opening into the trachea to extract it, provided it was under one condition, that is, that it moved up and down in the tube. He says, "recorded cases have proved that this operation is not necessary." Now this we must be permitted to deny. There are cases on record in which a moveable body, detained in the trachea and producing serious symptoms, has been removed or ejected through an opening in this tube, to the great relief of the patient. Our author's expectation, that by keeping the patient quiet in bed, the body would easily pass off, is opposed to the results of experience.

We quote the following case as an example of judicious practice which surgeons would do well to imitate:

> "A child was brought to St. Bartholomew's Hospital with a pebble in the meatus of the ear. I found it of an oblong form, and firmly wedged in. I could get the blades of a small pair of forceps to grasp it when passed over the short diameter, but I could not make it stir. Having the fear before me of doing mischief by using force, I directed the mother to bring the child in a fortnight. She did so, and I found the pebble quite loose, so that it might be removed by only a shake of the head. The body was coated with cerumen; and, of course, interstitial absorption had been going on under the influence of the power of relief setting it free." (pp. 95–6.)

More danger is likely to arise from persevering efforts to extract a foreign body firmly impacted in the ear, than from leaving it. Severe inflammation of the internal ear, and even death, have ensued from the rude force of the surgeon in cases of this kind. We have had occasion to comment on the undue reliance which in many cases Mr. Vincent places on Nature's efforts, or what he terms "the conservancy of function;" and the too little benefit that he is disposed to ascribe to the effects of surgical skill; but we must in justice add, that in no instance is he chargeable with officious and meddlesome surgery. Most strictly does he follow the injunction, "Primo non nocere."

Our author's reflections on the constitutional effects of injuries and their proper treatment, and on compound fractures, are excellent, and will amply repay the inexperienced surgeon for the time bestowed in their perusal. The practical remarks on abscesses in the vicinity of the rectum, equally claim his attention.

In the observations on the disorders of the urinary organs, we find much with which we coincide, and also much with which we disagree. In treating of retention of urine from enlargement of the prostate, he states, that, "by the regular employment of the catheter, we find that the muscular structure of the bladder will gradually recover its susceptibility to the stimulus of the urine." We should rather say, its power of expelling the urine. It is the mucous coat of the bladder which is chiefly susceptible to the stimulus of the urine; the muscular structure being excited to action by the sense of distension and the stimulus of the will, the exercise of the latter being influenced and regulated principally by impressions on the mucous membrane. In his observations on these affections, throughout, he does not take sufficiently into account the conditions of the mucous lining of the bladder. We agree with Mr. Vincent in attaching but slight importance to what has been said about the curve best d for an instrument to glide easily into the bladder. "The method ng it is of more importance than the curve." He is of opinion that the elastic catheter has not the advantages of the silver instrument; thus, we know best by the latter "the situation and the degree of induration of a stricture, the hardness of the prostate gland, and all the peculiarities that may exist in the entrance of it into the bladder." On the use of the catheter in young persons, our author remarks:

> "The surgeon should always bear in mind, that, in young children, the bladder lies very high in the pelvis, and that there is a long track of the membranous part of the urethra where the textures are very thin, and the pelvis being very narrow, in any state of the parts, the instrument may readily pass into the rectum. This I have seen very often done by those who do not attend to the points I have laid down. Therefore, in young subjects, the operator ought always to pass his finger into the rectum to guide the instrument through the sharp turn, which the narrow pelvis causes, when it has arrived under the pubis. Now this happens commonly without the surgeon being aware of it, because it is really an accident followed by no inconvenience; the puncture into the rectum closes very quickly. This is another instance of the power of conservancy for preserving functions." (pp. 205-6.)

We are glad to find Mr. Vincent in favour of the practice of puncturing the bladder above the pubis in cases of retention, in which the catheter cannot be passed. He states that he has done "it often, and very rarely lost a patient by it. Where the patient was not very old, and tolerably healthy, it has never failed of restoring him to health." He adds, that he has never seen any infiltration of urine follow this operation, that was of consequence enough to retard the cure; and the wound has readily healed, because the urine soon found a passage through the urethra. We have witnessed so much evil from forcible catheterism, and derived so much advantage from pursuing the treatment advocated by Mr. Vincent, that we strongly recommend our readers to peruse the views set forth in pages 208-210. We have heard surgeons boast of never having been foiled in passing an instrument—which may be true; but it is also true that they have inflicted serious and even fatal injuries by the barbarous practice of forcibly overcoming all obstacles to its progress.

Mr. Vincent has great confidence in turpentine as a remedy for arresting hemorrhage, by altering the peculiar disposition in vessels to bleed. He gives it internally, and applies it locally. In the management of cases of bleeding, he insists on the importance of keeping the bleeding vessel free from all coagulum. The reasons given for this are not satisfactory. We find it stated, "that if a divided artery be in contact with a layer of fibrine, it has a strong affinity and aptitude to shoot into it, and it is possible that a clot of coagulum has a modified effect of this sort upon the orifice of an artery, so as to keep it from contracting and closing." Mr. Vincent can hardly mean to assert that a vessel, of a size sufficient to pour out blood to any amount, would be disposed to shoot into a layer of fibrine; yet this sentence scarcely admits of any other interpretation. We do not believe that a clot operates in the disadvantageous mode supposed by our author. That all coagula should be removed before the application of a styptic, we admit; but it would be wrong to wipe away the coagulum in order to apply a styptic, if no bleeding were going on. Our own experience in the treatment of wounds of the deep palmar arch enables us to speak with confidence of the plan of making graduated pressure limited to the point of the bleeding vessel, as recommended by Mr. Vincent.

The subject of scrofula is rather fully treated of; and our author's observations relating to it deserve to be read with attention. He is disinclined to resort to operations for the removal of scrofulous complaints, as it is a state that unfolds its character in early life, and languishes as the period of existence declines. He shows that the constitution in general is not much disturbed by local scrofulous diseases, and that, when the diseased part is removed in early life, scrofula is very apt to be developed in some fresh situation. On this point Mr. Vincent expresses himself very forcibly, observing that, where he has had the opportunity of watching patients who have had scrofulous joints removed, they have for the most part lost their lives by phthisis. He has no hesitation in saying that the removal of scrofulous limbs, when the patient has appetite and sleeps well, is far from maintaining the scientific character of the profession. In all this we entirely agree with him; but are happy to add our belief that sounder views of practice are beginning to prevail on this subject, and that surgeons are less inclined than they used to be, to perform amputation in cases of scrofulous diseases of the joints. Mr. Vincent particularly alludes to the tendency which there is in scrofula of varying its local manifestations; observing that this change from one spot to another will even take place several times, before any alteration of the structure occurs in a particular part. On the fugitive character of scrofula we may quote the following passage:

> "Some years ago I attended a young gentleman who had a scrofulous affection of the knee. By rest, and the usual attention which these cases require, it evidently soon improved; and then the knee, almost suddenly, appeared to be well. He now felt pain in one of the lumbar vertebræ, which was aggravated upon pressure. This local affection was treated as for disease of the vertebræ, by which he was greatly relieved, and very soon all pain and fulness about the part had left. The knee now again became enlarged and painful, and, from the lingering way in which it continued to go on, the local manifestation seemed settled in this joint.—I had an infant of about two years of age under my care, who had an unequivocal scrofulous affection of the knee. One day, on my visit, I found the swelling of the joint quite removed; but the mother stated to me that the child did not seem so well as it had done, and drooped its head. Not many hours passed before it was evident that the child was labouring under hydrocephalus. The care of the patient was transferred from the surgeon to the physician, and in a short time the patient died of hydrocephalus.—I had in the hospital a youth of about twelve years of age, who had from infancy a purulent discharge from the meatus of one ear. This would stop, and then he had an attack of strumous ophthalmia. After some continuance, this disappeared, and then some of the absorbent glands in the neck became enlarged and inflamed. This affection also had its period, and then an impetiginous eruption would appear on the scalp. Each of these local affections, which may all be placed in the class of scrofulous disease, would abide a limited time, although not exactly in the order I have placed them, and afterwards recede, another local expression making its appearance, and proving that the activity of scrofulous manifestations was not subdued, and that, if the state of disease did not permanently fix its locality, yet it preserved its active energies to infringe on the natural state of healthy structures. These cases appear to me to prove, that it is one of the laws of scrofula to be changeable in the parts it fixes upon as the seat of its local expression, and that it will have scope to exercise its energies." (pp. 268-270.)

Mr. Vincent's theoretical notions on counter-irritation are not very intelligible, and bespeak an exploded pathology. His objections to glass beads for issues, and his reasons for preferring peas by which is kept up "an oscillation of varying energies," will amuse rather than instruct.

Without adopting all Mr. Vincent's views on joint-disease, we recommend the perusal of his remarks on the subject, especially on inflammation of the synovial membrane of the knee, (pp. 288-94.) They are evidently the result of a close observation of the symptoms, and of considerable experience in the treatment, of these affections. He is very partial to the use of issues in synovial disease of the knee, and also in hip affections. In cases of hip disease in children of a purely scrofulous character, we believe that issues are productive of more harm than benefit. The mild counter-irritation obtainable from iodine applications, and the ceratum hydrargyri c., better known as Scott's ointment, has appeared to us far preferable to issues in these cases. But in many cases the mildest rubefacients only keep up irritation, and disturb the general health without benefiting the local disease.

Mr. Vincent is of opinion that the effects produced on the brain by injuries of the head, differ according to the direction in which the blow is given. We must refer our readers to the work for the explanation of this circumstance. The diagram referred to does not serve much to elucidate the description. The conclusion arrived at, is, that "if the blow is in such a direction that it would transmit the impulse to the base of the cranium, the cerebral symptoms will be severe; but if the direction of it transmits the impulse across the brain, then they may be very slight." "It is not the part stricken, but the direction in which the blow is given, that gives rise to the difference in its effects." After giving as an illustration of his views a case of fracture of the skull, he mentions as a further example, the case of a patient with a tumour in the centre of the forehead that had made its way through the bone. It continued increasing until he died; but, during the whole time, he had not the least symptom of affection of the brain. It was a malignant tumour of the dura mater, which besides projecting outwards had also pressed the brain inwards, so as to form quite a hollow for its reception. The whole of the pressure was from before backwards, in a line at right angles to the vertical line. To this he attributes the absence of cerebral symptoms. This case seems to us inapposite, and the conclusion drawn from it erroneous. The effects of the gradual pressure of a tumour differ greatly from those consequent on mechanical violence. The inconvenience of the pressure in this case would not be experienced exactly in the direction described; but would be more evenly diffused throughout the brain, and the depression was occasioned by the atrophy, and not by displacement, of the cerebral tissue. We should ascribe the absence of cerebral symptoms partly to the situation of the tumour, by which a part of the brain, not immediately connected with motion or sensation, was interfered with, viz. the surface of the convolutions, and partly to the aperture in the bone permitting the extension of the tumour externally.

Our author constantly applies his knowledge of mechanics in the explanation and treatment of surgical cases. He endeavours to show that the figure of the diaphragm is such, that, when its fibres are exerted, greater force is applied to some parts of the boundaries of the abdomen than to others, thus determining the seat of hernia at the lower part. Our space does not admit of our giving an account of the principle upon which this is accounted for. We can only remark that the explanation is not satisfactory. In thus endeavouring to account for the bulging of the abdomen, so often observed above Poupart's ligament, he seems to overlook altogether the force of gravity in favouring the yielding at this part. Besides, too little importance is attached to weakness of the abdominal walls in different situations as predisposing to hernia. He strongly recommends the employment of the taxis upon patients in a warm bath with the body in the bent position. We have heard Mr. Vincent's pupils extol his great success in the reduction of hernia, and we can bear testimony to the excellency of the plan advised, which, if we are not mistaken, is generally adopted in the hospitals of the metropolis. Here, too, artificial anæsthesia may often be advantageously induced.

In London we know of no larger field for the study of syphilitic disease, than is afforded by the wards of St. Bartholomew's hospital. Mr. Vincent's practice is greatly influenced by the doctrines so generally prevalent during his early professional career, and he still holds fast to the mercurial treatment in all stages of the disease. The length to which our review of this work has extended, forbids our discussing many interesting questions noticed and suggested in the pages before us; and this we the more regret, because, although we disagree with the author in many of his views of treatment, and believe that he relies too little on the powers of the constitution in ultimately expelling the disease, there is no part of his work which evinces more strongly the practical character of his mind and the extent of his experience, than the remarks on syphilis. We cannot recommend these observations for the instruction of students, but we are sure they may be perused with advantage by practitioners.

The important subject of carcinoma of the breast and other parts is briefly disposed of in the last few pages of the work. Notwithstanding all that has been written on this disease, there is a great want of established principles of treatment for the guidance of practitioners; indeed, in no part of surgery is the imperfection of our art more strikingly displayed than in this class of affections. We do not find much in Mr. Vincent's observations to assist in removing the opprobrium.

Numerous and various as are the topics treated of in this work, it is hoped that enough has been extracted and abridged to afford a fair notion of its contents. In justice to our young readers, we have ventured to express our opinion freely on points on which we have found occasion to differ from the author; but we hope to have done so in a spirit consistent with the respect due to a gentleman whose high character, courtesy, and kindness have long won the regard of the sons of St. Bartholomew. Those who know Mr. Vincent merely by reputation, as having filled the office of surgeon of a large metropolitan hospital for a long series of years, and consequently enjoyed great opportunities, may feel disappointed with a work which puts forth no remarkable improvements in pathology and practice. Such was not the object of the publication. Its aim was to furnish the fruits of a matured experience in the treatment of ordinary injuries and diseases, for no purpose, indeed, of vanity or self-interest, but from the conviction that the results would contribute to the advancement of practical surgery. And knowing, as we do, the reluctance which the seniors of our profession,—who, perhaps too lowly estimating their abilities, refrain in early life from rushing into print,—afterwards feel to thus coming before the public, we are thankful for a book which contains many valuable remarks, useful hints, and observations which few engaged in the active duties of their profession can read and reflect on without profit.

ART. III.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXX. Second Series, Vol. XII. —*London*, 1847. 8vo, pp. 252.

THE book before us has rather a meagre appearance, in comparison with the later volumes of these Transactions. We have no doubt that the Council of the Society have wisely attached greater importance to quality than to bulk, and have this year been compelled to make a smaller selection than usual from the numerous communications read at the meetings. Even the present volume, as we shall presently show, contains papers whose slight value and importance scarcely entitle them to a place in the Transactions of the first Medical Society of Great Britain.

I. *Two remarkable cases of encephaloid disease of the heart, with observations;* by Prescott Hewett, Lecturer on Anatomy at St. George's Hospital Medical School.

In the first of these two cases, the leg of a man, aged 40, was amputated on account of a tumour of the left foot, of an encephaloid character. The patient afterwards went on badly, and died on the sixth day. The body was examined; and, in addition to marks of extensive recent inflammation in the left pleura and pericardium, the heart, much increased in size, presented dilatation of the right cavities, with hypertrophy of the muscular structure of the left ventricle.

> "In the right auricle was a large growth, which, proceeding from the appendix, occupied the greater part of the cavity of the auricle, and, passing through the auriculo-ventricular opening, projected into the ventricle, reaching nearly to the bases of the columnæ carneæ of the valve." (p. 2.)

The shape and appearance of this growth are minutely described. Its structure resembled exactly the encephaloid disease of the foot. No carcinomatous disease was discovered in any other part of the body.

In the second case, a woman, ætat. 59, was admitted into St. George's Hospital, with a tumour of the right breast, as large as the egg of an ostrich. On its outer side was another tumour, of the size of a pullet's egg. The patient presented no symptoms of disease beyond that of the breast. But, at a consultation of the surgeons, it was determined not to meddle with the disease. The tumour increased, ulcerated, and became fungous, until about six weeks after her admission, when she was suddenly attacked with urgent dyspnœa, and partial syncope, the pulse at the wrist being scarcely perceptible. These symptoms increased, and she died on the following day. At the examination of the body, the tumour of the breast proved to be of a decidedly encephaloid nature. Several of the absorbent glands were affected, and a tumour was found in the left rectus abdominis. The lungs were healthy; but the heart was enlarged by dilatation of its cavities, chiefly of the left auricle. The auricular surface of the mitral valve was, for the greater part, covered over by a soft, pinkish deposit, of an encephaloid character. This species of vegetation projected some distance, both into the auricle and into the ventricle, running along the surface of the tendinous cords, and covering the extremities of the muscular columns; and by it the auriculo-ventricular opening was all but clogged up, there being merely an opening of the size of a quill left for the passage of the blood. No encephaloid disease existed in any other part of the heart, or in the abdominal viscera.

Mr. Hewett notices the rarity of these forms of carcinomatous affections of the heart, and calls attention especially to the latter case, as in this there existed a well-marked specimen of encephaloid disease of the free surface of the endocardium,—an affection so rarely met with, that Dr. Walshe, in his treatise on Cancer, alludes to only one case, and even that is of a doubtful character. The chief interest of these cases arises, however, from the practical consideration of an external tumour of a carcinomatous character being accompanied with disease of the heart of the same nature, without any symptoms being present to indicate that the organ was seriously affected. They tend to strengthen the views pretty generally entertained, of the extremely unfavourable results of operations for the removal of encephaloid disease, and, consequently, to discourage their performance.

II. *Case of tumour in the groin, where the testicle had not descended, and operation for its removal;* by J. Moncrieff Arnott, F.R.S., President of the Society, Surgeon to the Middlesex Hospital.

A man, 43 years of age, had a tumour in the right groin. The history and symptoms of the case showed pretty clearly that it was a disease of an undescended testicle: but Mr. Arnott was unable to determine its precise nature, whether hydrocele, or hæmatocele with a thickened tunica vaginalis, cystic sarcoma, or malignant disease. The tumour was punctured with a lancet at its lower part, but only blood escaped. The integuments were then divided, the inguinal canal and tunica vaginalis laid open, and an enlarged testicle exposed. This was removed, the spermatic cord being cut across, close to the internal ring. A section of the mass presented the ordinary appearance of medullary sarcoma, without any trace of the natural structure of the testicle. In a postscript, we are informed, that, on the twelfth day after the operation, the patient was attacked with erysipelas of the face and head, of which he died in three days. On examination, a small deposit of encephaloid substance was found on the right spermatic cord, just within the inner ring; and a large mass in the root of the mesentery, which, owing to his being fat, had not been detected during life. There were no deposits in the liver or lungs.

Mr. Arnott refers to a case, published in the 'Medical Gazette' for January 15th, 1847, of an undescended testicle, the seat of medullary sarcoma, which was removed by Mr. Storks. The operation was performed in January, 1846, and the patient recovered and lived till April 1st, 1847, when he died in St. George's Hospital, of the same disease, developed to a great extent in the abdomen. Mr. Arnott alludes to some cases of diseased testicles in the groin, removed by operation, which are described by Mr. Curling, in his work on the Testicle; and also gives the details of a case, probably of undescended and diseased testicle, communicated to him by Mr. Hodgson, of Birmingham. In the latter case, death, from inflammation of the tumour and gangrene, followed the operation of tapping; but the interest of the case is much lessened by the circumstance that permission could not be obtained to examine the body.

We are obliged to Mr. Arnott for this interesting communication. Since the time of Pott there has been no account of an operation, in this country, for the removal of a diseased testicle from the groin, until the occurrence of Mr. Stork's and the present case. These and the other cases referred to, afford little encouragement for a repetition of the operation, at least when the disease is of a carcinomatous nature; since it is more difficult, even than when the gland is in its proper position, to determine beforehand the limits of the disease.

III. *Penetrating wound of the neck, accompanied by profuse arterial hemorrhage, treated successfully by ligature of the common carotid artery;* by F. Le Gros Clark, Assistant Surgeon to St. Thomas's Hospital.

In this case, the patient's life was saved by the prompt performance of an important operation. A policeman wounded in the neck and bleeding profusely, was admitted into St. Thomas's Hospital. When Mr. Clark came to the surgery, Mr. Solly, after ineffectual attempts to find the bleeding vessel, had just succeeded in controlling the bleeding by pressing his fingers deeply into the wound. It was agreed that, as another gush of blood might prove fatal, the former should place a ligature on the common carotid, while the latter retained his command of the bleeding vessels. The operation was accordingly performed, and the patient recovered without any untoward symptom. Mr. Clark observes, that this case "presents an illustration of a remark which occurs in Dr. Burrows's recent work on 'Disorders of the Cerebral Circulation,' that cerebral symptoms rarely supervene when there has been considerable loss of blood prior to the application of a ligature on the trunk of the common carotid artery." (p. 19.)

IV. *Some instances of the contrast between delirium tremens and inflammation of the brain, as regards the quantity of phosphoric acid excreted by the kidneys;* by H. Bence Jones, M.D., F.R.S., &c., Physician to St. George's Hospital.

It is to be remarked, that the author is not here taking the varying proportions of earthy phosphates, excreted in the disease in question, as the ground of contrast, but those of phosphoric acid itself, however combined. Hence, in instituting the comparison, he does not weigh the precipitate obtained in each case by means of ammonia alone, but that which occurs upon the addition of ammonia to urine which has been previously treated with a few drops of solution of chloride of calcium. The amounts of insoluble phosphates obtained by these two processes are not only very different, but seem to bear no fixed proportion whatever to each other. The comparison has reference to the absolute quantities of phosphates excreted in the two diseases, which in delirium tremens sinks far below the natural standard, as well on account of the diminished quantity of urine voided, as of the smaller proportion of phosphates contained in it. In inflammation of the brain, a large increase in the proportion of phosphates is observed, without this diminution in the secretion of urine. The whole number of cases brought under review are six; whereof three are instances of the former, and three of the latter disease. The bearing of the whole paper will be best understood from the following passages:

"It will be seen that, in about twenty hours, in one case of inflammation, 58 ounces of water, specific gravity 1024·8, were excreted; while in a case of delirium tremens, in eleven hours, the whole quantity excreted was four ounces, specific gravity 1019·1.

"In the three cases which are here related of inflammation, the average quantity of phosphates is 8·26 per thousand urine, specific gravity 1025·3.

"During the acute stage of the three cases of delirium tremens, the average quantity of phosphates is only ·67 per thousand urine, specific gravity 1020·4." (p. 22.)

These results are so striking, and, if confirmed by further observation, of such obvious practical importance, that it becomes necessary to consider a little more in detail the circumstances which attended them, and the possible sources of fallacy which may vitiate the conclusions they appear to warrant. The sources of fallacy to which we refer, arise from the non-exclusion of the influence of treatment in either class of complaints; and the complication of the cases of inflammation of the brain with phthisical disease. The power of opium to diminish and modify the action of the kidneys is so well established, that it requires to be ascertained what extent of influence, if any, it may have had in conducing to the peculiar circumstance observed in these cases. In one of them (the third), however, a very small proportion of earthy phosphate was obtained from the urine, at a time when only twenty-five drops of laudanum had been taken, which proportion was increased almost seventy-fold under the continued use of larger doses, prior to any marked abatement in the disease, or any material change in any of the conditions, except that of the patient having taken mutton and potatoes. With the increased proportion of phosphates, there was an increase also both in the quantity of urine and in its specific gravity.

In two of the cases of inflammatory affection of the brain, and probably in all three, phthisis was a coexistent disease; and it may be a question how far this may have influenced the particular result. It appears, however, that the urine underwent a very marked diminution in its amount of phosphates in the second case of delirium tremens, and that tubercular deposits were found in the lung in this case also. The modifying influence of mercury, which was certainly administered in one, and probably in all the cases of inflammation, must be also disproved or subtracted from the general effect, before we can attribute the increased excretion of phosphates to cerebral inflammation alone.

We conceive that Dr. B. Jones has rendered it highly probable, that by a new and easy method, we may distinguish with certainty between disturbances of the cerebral functions arising from inflammation, and those produced by causes of a totally different nature; but before we can avail ourselves of this important aid, we need more numerous observations, and the removal of some possible sources of fallacy, to assure us of its trustworthiness.

V. *An account of a case of encephaloid disease of the endocardium;* by Edward Latham Ormerod, M.B., Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital.

This paper furnishes us with a somewhat detailed account of the case of a man, 48 years of age, who, after excision of the right testicle in August, 1846, was received into St. Bartholomew's Hospital in the following November, in an extremely weak and emaciated state; and in whom a tumour was observed at the lower end of the left sterno-mastoid muscle; another in the epigastrium, and another in the right umbilical and lumbar regions. Pus-globules were discovered in his urine, and he sank in twelve days after his admission. After death, the right ventricle of the heart was found "nearly filled up by some roundish masses of a dull red colour, and soft consistency," for a further description of which, as well as for that of the other morbid appearances observed, we must refer to the paper itself. No particular diagnosis of the nature of the case seems to have been made during life. It was impossible to give any but a general opinion as to the existence of malignant disease pretty extensively diffused, and to form any definite opinion as to its seats and connexions. We are consoled for this want of the means of forming a correct and minute diagnosis in such cases, by the reflection that, if all which the knife revealed after death had been clearly made known during life, such knowledge would have been utterly unprofitable. Papers like the present, therefore, appear to us to possess such interest only as arises from their indicating the liability of all organs to be the seat of these growths, and the influence which this circumstance may have in the performance of surgical operations.

VI. *Case of internal strangulation of intestine relieved by operation;* by Golding Bird, A.M., M.D., F.R.S., Assistant Physician to Guy's Hospital; and John Hilton, F.R.S., Assistant Surgeon to Guy's Hospital.

The subject of this case was a gentleman 20 years of age, who laboured under insuperable constipation, and all the symptoms of internal strangulation. The history of the case is minutely related by Dr. G. Bird, who came to the conclusion that the obstruction depended upon a knuckle of intestine becoming strangulated in some manner under a band of false membrane. The character of the vomited matters, and the empty state of the cæcum and colon, referred the seat of the obstruction to the small intestines. The amount of urine secreted, showed that the absorbing surface of the small intestines could not have been very considerably interfered with, and induced Dr. Bird to conclude that the seat of strangulation was not many inches from the ileo-cæcal valve. Coupling this with the previous dragging sensation, it was rendered highly probable that the stricture would be found in the right iliac region. On the fifteenth day from the commencement of the illness, Mr. Hilton was sent for to perform an exploratory operation, and to endeavour to remove the obstruction. We must refer our readers to the paper, for an account of the different steps of the operation, which are fully detailed by Mr. Hilton. After a troublesome groping amongst the intestines, and the division of a band connecting two portions of small intestine, with no effect, the operator ascertained that six or seven inches of ileum had passed through an annular opening, formed in part by another portion of the same small intestine, and in part by some old membranous adhesions to the brim of the pelvis over the external iliac blood-vessels. Not being able to return the distended intestine through the opening by pressure, he made gentle traction on the intestine at the opposite side of the opening through which it had passed, and after a little perseverance, succeeded in liberating it from its incarcerated position. The intestines were replaced with some difficulty. The operation lasted about an hour. The hiccough and vomiting ceased, but the patient sank, and died about nine hours afterwards.

In this case, owing to the objection of friends, the operation was performed at so late a period (the fifteenth day from the commencement of strangulation), that there could have been very little hope of a favourable issue. It appears that Mr. Hilton experienced no slight difficulty in finding and liberating the strangulated intestine; and we know that, in two or three cases, the abdomen has been opened under similar circumstances, without the surgeon having succeeded in his search for the source of obstruction. We believe that in some instances the impediment is sufficiently indicated by the symptoms, to justify the performance of an exploratory operation; but the present case does not much assist us in determining the propriety of the practice.

VII. *Case of elephantiasis;* by George Southam, Dispensary Surgeon to the Manchester Royal Infirmary.

This is a remarkable example of a disease not often met with in an extreme degree in this country. The patient was an unmarried female; and the disease, which had existed about twenty years, commenced when she was in her eighteenth year. There is an excellent lithographic representation of the disorder, as it appeared for the last four years of the patient's life. The measurement round the calf of the leg was 2 feet 9 inches, above the knee 3 feet 4 inches, and at the upper part of the thigh, including the nates, 5 feet 6 inches.

> "On examining the body twenty-six hours after death, the enlargement was found to have been caused by the deposit of a dense, white, lardaceous substance, interspersed with fat, in the subcutaneous cellular tissue. When cut into layers, a small quantity of sero-sanguinolent fluid oozed out, and a few small vessels might be seen traversing its structure; but beyond their presence, it did not present any marks of vascularity. The cellular tissue beneath the superficial fascia was not at all affected. The muscles, though smaller than usual, preserved their natural appearance and situations. There was no enlargement of the bones or disease in the joints. The principal venous trunks were much larger than natural, distended like injected arteries, and when divided transversely, were patulous. Their external coat was thickened, and except in a few places, the middle and internal one could not be traced, both having apparently been converted into a thick, fibrous substance, disposed round the vessels in laminæ, not unlike what are observed in aneurismal tumors." (pp. 71-2.)

All the smaller veins in the diseased structure, when divided transversely, resembled arteries filled with coagula. Several of the principal veins were impervious. The saphena was converted into a thick fibrous cord, and a large organized coagulum was found at its connexion with the femoral vein.. The diseased state of the veins did not extend beyond the groin; those of the pelvic cavity were healthy. There was no particular disease elsewhere.

Mr. Southam remarks that this examination—

> "Tends to show that the disease commenced in the veins, being probably the result of repeated attacks of inflammation of the capillaries, which gave rise to the pain and febrile symptoms in the earlier stages of the complaint." (p. 73.)

He supports this view of the origin of elephantiasis, by reference to the opinions of other writers who have observed the disease, and states, in conclusion,—

> "That the immediate cause and pathological changes of elephantiasis bear an

intimate relation to those of phlegmasia dolens, and the induration of the cellular tissue in newborn children; the apparent differences depending on the degrees of venous obstruction and on the remote influences which have originated it."

VIII. *On tubercular pericarditis, with pathological and practical remarks;* by George Burrows, M. D., F. R. S., &c.

It appears that an Italian musician, aged 24, was admitted into St. Bartholomew's Hospital on the 4th of January 1844, suffering from considerable debility, with slight cough, his sputa being at times slightly tinged with blood. He had improved in health up to the 22d of the month; when, in consequence of more blood being detected in the sputa, a fresh examination of the chest was made, and a harsh double friction sound was heard all over the cardiac region. Under the use of mercury, these friction sounds disappeared on the 6th of February, having lasted for fifteen days. On the 4th of March he was losing flesh, had still a cough, the expectoration showed occasional streaks of blood, and his cheeks exhibited a permanent flush. The symptoms of phthisis continued to advance; but pleurisy supervened on the 22d of April, and the patient died on the 10th of May, with great effusion on both sides of the chest. Besides the effects of pleurisy found after death, and some groups of tubercles in both lungs, "the surfaces of the loose and reflected pericardium were rendered entirely and firmly adherent by a layer of lymph, studded with minute tubercles. The external surface of the pericardium was also adherent to the pleura by a false membrane, in which were deposited tubercles similar to some in the false membrane lining the pleura."

In a second case of a lad, 19 years of age, who had severe dysentery at the Millbank prison, and in whose body, after death, ulcerations of the colon, and numerous tubercles in the lungs were discovered, "the two opposed surfaces of the pericardium were firmly attached to each other over a large extent; and in one part, where the connecting false membrane was thickest, three or four small yellow tubercles were distinctly recognised imbedded in it." These were all that were actually seen, but the existence of more is said to have been probable. Of the third and last case we need give no description, since the patient did not die, at least not under the author's observation; so that the existence of tubercular pericarditis in this instance is matter of bare hypothesis. We pass on to point out the remarks, pathological and practical, built upon the broad foundation of these three cases, of which, as we have seen, one only is admissible as an example of the disease under discussion; for four tubercles in the false membrane uniting the surfaces of the heart, no more constitute a case of tubercular pericarditis, than half a dozen in the apex of the lung would prove the existence of phthisis pulmonalis.

Dr. Burrows having given the reins to his imagination, and having stated that he has no data to enable him to estimate the frequency of tubercular affections of the pericardium in phthisis, declares it to be probable that false membranes on the pericardium, which Louis reports as found in three only out of 112 fatal cases of phthisis, must have existed in many more instances under his (M. Louis') notice. These three cases, however, might have been tubercular, for Louis does not state whether tubercles were looked for in the false membranes. Be this as it may, our author discourses upon the circumstances under which, and the individuals in whom, tubercular pericarditis is to be looked for; but the value of his conclusions can scarcely be assigned, when it is considered that they rest entirely on but one trustworthy case. When general inferences such as these are drawn from such data, they should be styled, we think, plausible prepossessions, rather than dignified with the name of conclusions. Dr. Burrows goes on to ask some questions. "What relation do the tubercles and inflammatory fibrinous exudation in the pericardium bear to each other? Which lesion is cause, and which effect? Or do both lesions result from contemporaneous morbid actions?" He inclines to the belief that tubercles are the cause of the inflammation in the serous membrane on which they are deposited; and doubtless they may often be deposited without inflammatory action: yet this does not convince us that they are not in other cases the result of inflammatory processes employed upon vitiated blood; tubercle being deposited simultaneously with coagulable lymph, in consequence of causes which, in a healthy state of the circulating fluid, would cause lymph alone to be secreted.

The practical remark, "that the ordinary treatment of pericarditis, by repeated abstraction of blood, and by bringing the system under the full influence of mercury, is quite inappropriate," is here offered only as a deduction, contingent upon the "correctness of the explanation of the origin of this variety of pericarditis." This is but poor support for this useful practical suggestion, the wisdom of which has, however, been abundantly attested by that army of martyrs, whom the Bouillauds and Taylors in recent times have offered on the altar of insatiate Sangradoism.

Dr. Burrows enjoys, and deservedly enjoys, a very high reputation; but we think he will be rash if he draws upon it by many papers as crude and hasty as the present.

IX. *Successful removal of an ovarian tumour complicated with pregnancy;* by H. E. Burd, Senior Surgeon to the Salop Infirmary.

The patient was a woman about 25, the mother of three children. The history of the case, and characters of the tumour, showed it to be ovarian disease. The measurement over the umbilicus was 45 inches, and from the ensiform cartilage to the pubes 22 inches. Her dyspnœa was urgent, and she was unable to lie down. There was nothing to indicate the existence of pregnancy. It is unnecessary to describe the mode of performing and the steps of the operation. Notwithstanding the extent of the incision, it was found necessary to pass a trocar into the largest cyst; from which three gallons and a quart of fluid were drawn off, reducing the tumour to a convenient size for passing through the aperture. On drawing the tumour forward, so as to examine its pedicle, the uterus was brought into view in a gravid state, and was supposed to have reached the third or fourth month of pregnancy. The operation was performed on the 15th of September. On the 17th, abortion took place; the labour was easy, and the child was born alive. No hemorrhage ensued, and the woman bore her labour better than could have been expected. The frequent occurrence of collapse was the most important and alarming symptom after the operation, requiring opium, ammonia, and brandy. The absence of peritoneal inflammation was remarkable. On October 8th, three of the ligatures came away. On November 6th, the wound was quite healed, and the patient able to walk about.

This is one of the most remarkable cases of operation for the removal of an ovarian tumour, that has been yet published. Though this patient survived the perils of the operation and subsequent abortion, we entertain no doubt that such a complication must add considerably to the risks of the proceeding. The dangerous symptoms of collapse which ensued appear to have been treated with judgment and decision.

In the account which is given of the operation, it is mentioned that the pedicle of the ovarian tumour was extremely thick and broad, requiring to be tied in three different compartments with strong twine ligatures. The bulk of the pedicle made it so difficult, even with this division, to draw the ligatures sufficiently tight, that when the tumour was separated from the pedicle, the upper and outer one slipped, and had it not been from prompt assistance, a fearful hemorrhage would have ensued. Another strong ligature, was, however, immediately passed through the pedicle, and being tied, prevented further bleeding. We call attention to the accident here related, because fatal bleeding, owing to the ligature girting the pedicle having lost its hold after closure of the wound, has already occurred in more than one case of this operation.

X. *Case of enlargement of the left mamma ;* by W. E. Image, F.R.C.S., Surgeon to the Suffolk General Hospital. *To which is added an anatomical and pathological description of the tumour;* by T. G. Hake, M.D., Physician to the Suffolk General Hospital.

The subject of the enlargement was a young woman, æt. 21. About two years since, she observed a red mark, about the size of a shilling, just above the nipple, and that the breast was enlarged. The swelling continued afterwards to increase until it measured fifteen inches round its base. In addition to blue nævus-like spots near the nipple, the general surface of the breast presented a general bluish or slate colour. By pressure the leaden hue disappeared, and the tumour could be reduced. At this time the patient complained of fulness and heaviness in her head; and on the pressure being withdrawn pallor and faintness supervened. There was no pulsation and no murmur. With the exception of occasional faintness, of an alarming kind, the patient appeared in good health. Pressure, applied with an air-cushion, was kept up for three months, but no advantage resulted from its adoption; the disease continued to advance, and the nævus-like discoloration increased to six or seven times its former size. The nipple became almost obliterated ; and the integuments at the spot primarily affected appeared so attenuated, as to excite fear of its speedy rupture. Mr. Image took the patient to London, in order to obtain opinions as to the best mode of meeting the exigencies of the case by surgical art. Some high authorities suggested amputation of the entire breast ; others the introduction of single threads through the breast, with a hope of obliterating the structure of the tumour ; but a preponderance of opinion was in favour of strangulating the tumour by ligatures, which was the course adopted. Two flaps were dissected back, and double ligatures carried through to the base of the tumour by means of long and strong needles, so as to meet each other at right angles, in the centre of its base. The ligatures, eight in number, were then tightened. She lost 14oz. of blood, and became faint, and vomited. Further bleeding occurred, amounting in all to about 30 oz. or 36 oz.; the symptoms of collapse continued, and she died twenty-two hours after the operation.

A minute description of the structure of the tumour is furnished by Dr. Hake. This we may allude to when considering the paper of Mr. Birkett, on the structure of a nævus. Mr. Image's case is one of great rarity and interest. We find no fault with the mode of treatment finally adopted, but cannot help expressing a regret that the plan was not resolved upon and carried into execution at an earlier period, when the tumour was of smaller size, and the patient was less liable to suffer from hemorrhage and to feel the effects of the shock. The nature and formidable character of the disease were sufficiently obvious in April, yet the operation was deferred till September following. The application of pressure could not have been expected to do more than arrest the growth of the tumour, and its continuance for three months was a waste of time likely seriously to affect the result of the case.

XI. *Case of Cyanosis, with a description of the appearances presented on dissection;* by F. Le Gros Clark, Assistant-Surgeon to St. Thomas's Hospital.

It appears that the patient, a shoemaker, aged 19, had long been out of health; his chief symptoms having been dyspnœa, lividity of the face, pain at the region of the heart, &c. On examination of the body, the heart was found unusually large, the increase in size being especially due to the abnormal capacity of the right auricle. This cavity was largely dilated. On comparing the walls of the two ventricles, it was apparent that the usual relation of development was reversed, the parietes of the right cavity being nearly twice as thick as those of the left; this disproportion resulted almost exclusively from hypertrophy of the former. The long axis of the ventricle was shortened in the direction of the arterial opening, and instead of communicating directly with the pulmonary artery, the anterior part of the base of this cavity presented two small circular apertures, neither of which would admit, without distension, the passage of a small-sized goose-quill. One of these outlets opened into a small but thick muscular sac, of oval form, which measured about an inch in its longest diameter, and gave origin to the pulmonary artery. Above, and slightly to the left of this orifice, and about seven lines distant from it, was the second outlet from the right ventricle, precisely similar to the other in form, character, and size. This opened into the aorta, or rather into the angular aperture between two of its semilunar valves. The systemic arteries were generally of rather small calibre; the bronchial were unusually large and tortuous.

Mr. Clark remarks, in reference to this case, of which we have given an abridged account, that "the more prominent symptoms are satisfactorily accounted for by the post-mortem appearances; as it is evident that the currents of venous and arterial blood must have been mixed in the aorta, and that the lungs must have been deprived of a large share of their supply from the right ventricle. But the sum of the calibre of both the arterial outlets from this cavity was not equivalent to that of the normal aperture of the pulmonary artery; and thence the hypertrophy of the ventricle," (pp. 118-19,) and dilatation of the auricle. He adds, "it is impossible to overlook the connexion between the large size of the bronchial and branches of the internal mammary artery, and the obstructed pulmonary circulation; a coincidence which seems to justify the supposition that, naturally, the lungs are not exclusively dependent for their nutrition on the bronchial arteries." (p. 119.) We may remark that enlargement of these arteries has been noticed in cases where the pulmonary artery has been found wholly wanting, which leads us to infer that the increase in size in this case was not so much for the purposes of nutrition, as to compensate for the diminution in the calibre of the opening into the pulmonary artery. Mr. Clark regards, we have no doubt correctly, the primary cause of all the mischief to be a congenital malformation.

XII. *Observations on the coexistence of variola and scarlatina, with remarks on the coexistence of other eruptive fevers;* by J. F. Marson, Surgeon to the Smallpox and Vaccination Hospital, London.

The scope of this paper is apparent from its heading. Its tendency is to show the exceptions to, or limitations of, the law laid down by Mr. Hunter, that two specific actions cannot exist at the same time in the same part or constitution. The statements of the author scarcely admit of abbreviation or analysis, but he has himself furnished a brief summary of them, which we quote for the information of our readers.

> "Thus, either from personal observation or from the writings of others, I present examples of the simultaneous occurrence of variola and scarlatina, variola and rubeola, variola and pertussis, variola and vaccinia, rubeola and scarlatina, rubeola and vaccinia, rubeola and pertussis, varicella and vaccinia, pertussis and vaccinia."

This is a good and interesting paper, which will long be consulted by those seeking for information on the subject of which it treats.

XIII. *Case of malformation of the heart, in which death resulted from obstruction in the trunk of the pulmonary artery;* by T. B. Peacock, M.D., Physician to the Royal Free Hospital, &c.

We learn that William Holland, a lad 15 years of age, had met with an accident twelve months before his death by a fall from a cart, and that since that time he had been gradually getting thinner and weaker, was constantly chilly, complained occasionally of palpitation, difficulty of breathing, and pain in the region of the heart; his hands and face were always very blue, but especially so in cold weather, or when he was suffering from affection of the chest. His appetite was generally defective, and he occasionally vomited his food. Besides this, he had, eight days before his admission into the Royal Free Hospital, bruised his left knee by a fall, and had since suffered from constant pain in the joint, and for two or three days before admission in the right knee also. The day after the accident he began to experience pain in the left side of the chest, and difficulty of breathing; and when admitted, he was much collapsed. His cheeks of a deep purple colour, and his lips blue; his pulse was 124, and extremely feeble. A loud systolic murmur was heard, of a blowing character, most intense at the cartilage of the third left rib near the sternum. It was here prolonged throughout the whole period, so as to render the second sound inaudible. It diminished in intensity along the upper part of the sternum, in the subclavian and carotid arteries, and on the left of the spine posteriorly. The murmur became shorter and more feeble below the level of the nipple, and at the point of pulsation of the apex and lower part of the sternum was followed by a very clear second sound. Notwithstanding the free use of stimulants he sank in a few hours.

The lungs were engorged with blood. The heart was 10 ounces in weight, and measured 8¼ inches, of which the right ventricle constituted 4¼. The right auriculo-ventricular aperture opened as usual into the sinus of the right ventricle; but this portion of the cavity was separated from the infundibular part by a thick muscular septum, defective only at its centre over a space of sufficient size to admit the fore-finger, and perforated by one or two very small pores near the apex. The cavity thus formed communicated with the aorta by an orifice 30 lines in circumference, situated at its posterior extremity. Its walls averaged 5¼ lines in thickness, were unusually firm and solid, and in places had undergone the fibro-cartilaginous degeneration. The infundibular cavity of the ventricle was smaller, and gave origin as usual to the pulmonary artery. This vessel was very small at its orifice, and was provided only with two extremely thick and opaque valves, by the projection of which its calibre was farther contracted. The coats of this artery were much indurated and thickened, and its canal was entirely obstructed by fibrinous coagula. At the sides of the vessel these coagula were of a dirty white colour, and were laminated, and firmly adherent to the valves and diseased lining membrane. The obstruction occupied the whole trunk of the vessel. The left ventricle was of small capacity, and the aorta communicated with it by an orifice of the same size as that by which it arose from the right ventricle.

The author proceeds to show that this particular lesion of the heart, viz. that in which there is a septum in the right ventricle, as well as communication with the left, is very rare if not unique. He says nothing of the means of diagnosticating the affection; and in truth neither the symptoms nor signs are peculiar, if we except the statement that a murmur accompanying the impulse of the heart was at a particular spot prolonged throughout the whole period, so as to render the second sound inaudible. Now this remark, it seems to us, must be erroneous; simply because it is impossible for a sound produced by contraction of the heart to mask one which occurs during its dilatation, as the cause of the one must have ceased before that of the other can exist. Dr. Peacock has all the merit of having in this paper brought to light a new variety of incurable malformation of the heart.

XIV. *On subacute inflammation of the kidneys;* by John Simon, F.R.S., Assistant Surgeon to the King's College Hospital, and Demonstrator of Anatomy in King's College, London.

> "An examination of the process," says the author, "by which vesicles are substituted for the normal tubularity of the gland, will lead me to explain the origin of those large renal cysts so well known in the dead-house; but first, in order to elucidate that earlier stage of the disease in which the tubes and malpighian tufts are destroyed, I shall attempt to define more exactly the nature of nephritis, and shall describe those primary lesions of structure which attend it."

Having alluded to the fact that true glands are but processes or involutions of mucous membranes, he finds evidence of inflammation of the former in derangements of their secretory functions, analogous to those which are admitted as proofs of its existence in the latter.

An important circumstance, however, distinguishes the two cases; for whereas the simple mucous membranes can discharge from their cavities the vitiated secretions shed on their surfaces, the narrowness of the canals and complexity of structure of glandular bodies cause their morbid products often to accumulate within them. Inflammation of the kidneys is stated to be subacute in an infinite majority of cases; and dependent on some humoral derangement of the whole system, resulting from faulty digestion, from the suppressed function of other organs, as the skin, or the liver, or from the agency of some fever poison, as that of smallpox, erysipelas, typhus, scarlatina.

The *materies morbi* seeks its discharge by means of an increased activity in the functions of the kidney; it stimulates it, and the result is not so much an increase of the watery secretion, as an augmented cell-growth in the tubules of the gland. This acceleration of function is incompatible with maturity of the secreted products; hence the urine, examined microscopically, presents various abortions of cell-growth, from the pus-globule to the healthy gland-cell. Mingled with these are seen fibrinous threads or cylinders, often containing some of these cell-forms, or not unusually a few blood-discs. Crystals of lithic acid, or of oxalate of lime, are occasionally found imbedded in these fibrinous moulds of the urinary tubules.

But if these matters are in part discharged, giving evidence by their presence in the urine of incipient disease of the kidney, even before this is shown by its albuminous condition, they are in part retained, and plug up, distend, or destroy the tubes in which they were formed. It can very rarely happen, continues Mr. Simon, that a patient will emerge from the perils of acute albuminuria with his kidneys quite free from permanent injury; a certain proportion, perhaps a very small one, but almost certainly some, of the tubular structure will have perished. This destruction of a part necessitates a greater functional activity of the remainder; and this continued overwork is in fact a chronic congestion bordering on inflammation. Hence the diseased condition tends to a spontaneous increase; and hence, too, exposure to the causes of the original disease tells with a fearful and accumulative advantage on kidneys already in functional embarrassment.

In the post-mortem examination of chronic cases, the kidneys may or may not be found materially contracted and deformed. It happens very frequently that the organ has preserved its full size, and presents its ordinary colour. Between such kidneys, and those which are all knobbed and puckered and wrinkled, there is not the essential difference which their appearance would at first suggest.

We have thus sketched the first part of Mr. Simon's paper; and here he digresses to give an account of the formation of cysts. On this subject he says: "A section of cysted kidney carefully examined, may show an immense number of these minute vesicles, a number quite disproportionate to that of the larger cysts, visible to the naked eye; so that sometimes by a single one of the latter class seen on the surface of the kidney, I have found myself guided to a disease which is substantially a vesicular transformation of the ultimate structure of the gland." He dissents from the opinion that these cysts originate, either in distended tubules, or dilated malpighian capsules; but holds them to arise as a consequence of the rupture of the limitary membrane of the obstructed tubules, whence that which should have been intratubular cell-growth, continues with certain modifications as a parenchytic development. The effused gland-germs, he says further on, are the last phenomena of the original disease, and the first of the attempted compensation; since the transparent nucleated cysts, with their clear sharp outlines, are not mere dropsical epithelia, but are organized for secretion into their own cavities, so as at least to withdraw from the blood, if they cannot eliminate from the body, the materials which fill them.

Returning to the consideration of the uncontracted kidney, which has been the seat of inflammation, Mr. Simon remarks that the tubes have burst, and a great portion of their contents has been removed by absorption, and the malpighian bodies have dwindled to a few, so that the size is maintained by the adventitious cyst-growth filling the interstices of the organ. The main difference between the uncontracted and contracted specimens of kidney lies in the greater or less amount of interstitial cyst-development; the most dwindled are those in which least of the new growth has arisen or has survived.

Mr. Simon proceeds to state that two essentially different forms of degeneration of the kidney have been included under the name of Bright's disease; both characterized by the common circumstance of the presence of albumen in the urine, and both described by the respected physician whose name has been identified with them. For the class of cases treated of in the present paper, he suggests the name of subacute inflammation of the kidney. Its tendency is to the obliteration of tubes, to the development of parenchytic cysts, and finally to the contraction of the kidney. Cold and intemperance, the various fever-poisons, the irritation of gout, of rheumatism, of the oxalates,—these are its causes.

Secondly, there is the degeneration of the kidney, first illustrated by Dr. Johnson, commencing in a fatty engorgement of the epithelium, on which subacute inflammation is often engrafted, but which is in its origin non-inflammatory. For this disease he suggests the name of scrofulous degeneration. The cyst-development is observed in this second form also; but whether as a direct consequence of the primary morbid change, or induced by the secondary inflammation, the author considers doubtful. The diseases may be distinguished during the life of the patient, according to Dr. Johnson, by more or less oil entangled in the fibrinous coats, or gorging the cells which descend in the urine; a phenomenon which does not belong to pure subacute inflammation.

As regards the treatment of these diseases, the use of diuretics is prescribed, whilst the local abstraction of blood, the use of sweating-baths, the guarding against gastric sources of renal irritation, and the specific treatment of dropsy and other secondary affections, are briefly recommended.

Mr. Simon asserts his belief that subacute nephritis exists as frequently as pulmonary consumption, that in two-thirds of the cases it is latent, and that it is often unrecognised even in the dead body, because rarely impressing on the kidney those changes of aspect which would arrest the casual observer.

It will be observed that in the preceding paper Mr. Simon speaks of two stages or two forms in the inflammatory affection of the kidney,—its uncontracted, and its contracted condition; but in neither of these states does it resemble the mottled kidney. The mottled or fatty kidney, he says, is essentially scrofulous, and constantly found as a complication of phthisis. He adds a note to show that the mottled kidney is not subject to contraction. He makes this conclusion probable by a reference to 214 examinations of the kidneys by M. Louis, in phthisis, in no one of which was any diminution of size reported. And again, out of 116 similar cases recorded at St. George's Hospital, in two only is their smallness mentioned. This conclusion is farther fortified by Mr. Prescott Hewett's coincident belief that the mottled and contracted kidneys are of different pathological families.

There is not much of inference or argumentation in Mr. Simon's paper; its facts are all matters of ocular demonstration. He seems blessed with an eye which, like that of imagination, penetrates whatever it scans, and encounters neither impediment nor obscurity in its unobstructed gaze. It is this circumstance, perhaps, which, while it gives distinctness and precision, imparts a slight degree of inflation to his style, which some might think scarcely appropriate to the realities of science. In the following passage, at page 149, which we quote at length, will be found an illustration of our meaning. The author is speaking of changes latent in an uncontracted kidney, which has suffered from inflammation:

"In the first instance, then; in commencing the microscopical examination of the cortical substance, we partially find a similar state of tubes to that described in connexion with the subacute attack, a state, namely, of unequal distension and of blocking up by their own accumulated products. In the cases which have lasted a long time, these products will often be found to have undergone material alterations from the combined effects of pressure and absorption. The contents of the epithelial cells will have lost much of their natural fine granularity; so that the cells will appear, even when viewed singly, to have acquired a marked increase of solidity and substance. But—more than this; in many parts hardly a trace of tubularity will be found; the tubes have been burst; their contents have been interfused amid the matrix and blood-vessels, and their débris may be found on opposite sides of a preparation—here black and bloated, there pale and collapsed.

"Between these trophies of disease there is a new manifestation. The interspace is crowded with a profuse development of cysts, apparently foreign to the healthy structure of the part."

It is almost unnecessary for us to say that the present paper will well repay the perusal of the student of nephritic disease. It contains some new views on the subject of the development of cysts; a subject which must be considered still open to debate. It affords, moreover, a useful confirmation of the correctness of our information upon those points in which the author agrees with contemporary observers; and is of especial value in bringing into prominent view the remote consequences of subacute inflammation of the kidney.

XV. *On the inflammatory diseases of the kidney;* by George Johnson, M. D., Medical Tutor in King's College, and Physician to the Public Dispensary, Lincoln's Inn.

The author in the present paper carries out his intention, announced in his former memoir on 'Fatty Degeneration of the Kidney,' of making the inflammatory diseases of that organ the subject of a separate communication. These he divides into two classes; the first including those which result from a local cause, as retention of urine, the mechanical irritation of a stone, or a blow upon the loins; the second embracing those resulting from a constitutional cause, or from some abnormal condition of the blood. A citation of the note published with his former paper shows that, at the time of its going to press, he had recognised the condition of the kidney consequent on scarlatina as distinct from Bright's disease, that he attributed it to an inflammation exciting an increased development of the epithelium lining the urinary tubules, and that he had found this material in part accumulating and choking up the tubes, in part washed out and appearing in the urine.

In continuation of this subject, he now says that the chief difference between those cells which are the product of inflammation, and those which exist in health, consists in the former being generally of smaller size and more opaque and dense in their texture. During this gorging of the convoluted tubes of the cortical substance, the malpighian bodies are transparent and seem quite healthy; while the straight tubes of the pyramids contain an increased number of cells, probably not developed in them, but brought down from the others. Some of the tubes contain blood, presumed to have escaped from the malpighian tufts within the capsules; but it is an important omission that we are not told whether these bodies are in this case, as in that just named, transparent and healthy, or themselves also filled with blood. The context which precedes and follows leads to the inference that they are healthy, and militates against the presumption here pointed out.

The author incidentally notices the confirmation of Mr. Bowman's theory regarding the separation of water in the malpighian bodies, and the secretion of the solid ingredients of the urine in the tubules, afforded by his examination of the kidneys of two persons who had died of jaundice, when he found many of the urinary tubes stained of a deep yellow colour by the bile in their epithelial cells, this colour ceasing abruptly at the neck of the malpighian bodies.

The changes occurring in the kidney are clearly indicated by the condition of the urine, in the sediment of which may be seen with the microscope blood-corpuscles with epithelial cells, in great numbers, partly free and partly entangled in cylindrical fibrinous coats of the urinary tubes, and very commonly numerous crystals of lithic acid are present.

We give the theory of the effects upon the urine in the author's own words:

> "The presence of albumen and blood in the urine is obviously a result of the turgid condition and the active congestion of the entire vascular system of the gland. As this condition of the vessels in any of the tissues of the body commonly leads to extravasation of serum, or even of all the constituents of the blood into the surrounding parts, so the structure of the kidney, and particularly that of the malpighian bodies, is obviously such as renders the admixture of blood or serum with the urine a necessary consequence of the before-mentioned condition of the vascular system.
>
> "The imperfect elimination of the urinary constituents, their accumulation in the blood, and the consequent deterioration of that fluid, are the obvious and necessary consequences of the choking up of the tubes of the kidney, and of the obstacle thereby offered to the performance of their secretory function."

That nephritis may prove rapidly fatal is no new discovery; but it is very interesting to be told that "under judicious treatment the majority of these cases recover, the noxious matters are effectually eliminated, the vascular congestion and the desquamation of the urinary tubules simultaneously diminish, the cells which were thrown into the tubes are gradually washed out, and the kidney is completely restored to its original healthy condition."

To the form of disease here described, of which that following scarlatina may be taken as a type, it is proposed to give the name of "acute desquamative nephritis."

The predisposing cause of the disease in all cases is the presence in the system of abnormal and irritating products, the result of malassimilation. It is highly probable that exposure to wet and cold would never produce that form of disease under consideration, if the person so exposed had been previously healthy and well nourished. It is the *quality* of the blood sent to the kidney, and not the *quantity* of this material, which produces the disease in question.

The next form of inflammatory disease is essentially chronic, having a duration, in most cases, of many months, and, in some, even of several years. It is almost confined to persons who are in the habit of partaking freely of fermented liquors; and very commonly seen in those who have suffered from gout; sometimes, but rarely, in those who have been habitually temperate and abstemious.

In this form of disease, the kidney is never much enlarged; in the earlier stage, the size of the organ is natural, and the structure of the cortical portion appears confused, as if from the admixture of some abnormal product; there is also some increase of vascularity. As the disease advances, the cortical portion gradually wastes, the entire organ becomes contracted, firm, and granular, the pyramidal bodies remaining comparatively unaffected, even in the most advanced stages; and simultaneously there is a decrease of vascularity. The symptoms of this disease are dropsy, commonly not excessive, with urine generally abundant, and of very low specific gravity, and sometimes, but not always, albuminous.

On placing thin sections of the kidney under the microscope, some of the tubes are seen to be in precisely the same condition as that described in the case of acute desquamative nephritis. There is an appearance in the urine, too, of casts of the tubules, which are said to be characteristic of this form of chronic desquamative nephritis; but, except perhaps in the absence of blood-discs and the disintegration of the epithelial cells, we are at a loss to know in what they differ from those described under the first head.

We proceed to trace further changes in the organ, and are told that it is quite certain that, as a general rule, the malpighian bodies remain unaffected, both in structure, and in their office of transuding the watery constituents of the urine, until the whole of the disintegrated epithelium has been washed out of the tubes; as shown by the basement membrane of long convoluted tubes becoming entirely denuded, while in the corresponding malpighian body the vessels are quite perfect.

It appears probable, not only that the malpighian body continues to transude water, but that the whole length of a convoluted tube, thus deprived of its proper epithelium, becomes a secretor of water also. We think this probability sufficiently apparent upon the face of the case, and only wish that our author had abstained from giving two further reasons for it. For when he says that this is rendered probable by the appearance of the tube itself, we can discern no possibility of distinguishing by the appearance whether the tube has been filled and distended by fluid, secreted partly within the malpighian capsule, and partly from its own surface, or entirely by that proceeding from the former body. Again, he adds that this probability is still further increased by the fact of the tubes becoming, in some cases, dilated into cysts, which usually contain a simple serous fluid without any of the solid constituents of the urine. But two pages further on, he gives as part of the evidence of the simple serous cysts being dilated tubes, that as the inner surface of the tubes has the appearance of being endowed with the power of secreting water, so the cysts usually contain a simple serous fluid. His argument may, then, be briefly stated thus. It is very probable that the tubules secrete water, because they become dilated into cysts; and, again, it is very probable that cysts are dilated tubules, because tubules secrete water. This is very like reasoning in a circle.

The mention of cysts having been thus somewhat incidentally introduced, we are told that it has long been supposed that simple cysts, which are so commonly seen in connexion with some forms of renal disease, are, in fact, dilatations of the urinary tubes. The author, after avowing his ignorance of other satisfactory evidence on this point, proceeds to adduce his own, of which we shall give his summary:

> "The evidence, then, of the simple serous cysts being dilated tubes is the following: 1st. The tubes are often seen much dilated and thickened. 2d. As the inner surface of the tubes has the appearance of being endowed with the power of secreting water, so the cysts usually contain a simple serous fluid. 3d. As an accumulation of oil occasionally occurs in the tubes, so the cysts are in some instances filled with the same material. 4th. *There is no reason to suppose that these cysts have any other origin.* It appears probable that the malpighian bodies could not become dilated into cysts, because an accumulation of liquid within the malpighian capsule would necessarily compress and obliterate the vessels of the malpighian tuft, and so would cut off the further supply of fluid."

The reasoning in the last sentence is necessarily inconclusive. For as the capsule and the tubule are parts of the same cavity, and filled by a common fluid which exerts the same force at all points, if the malpighian tuft can go on secreting under a pressure sufficient to distend the tubule, there is no reason why it should not do so under one adequate to distend the capsule, unless it be shown that from difference in structure or form, a greater force is required in the latter than in the former case. Now this has not even been attempted. Moreover, as far as the influence of form is concerned, every one conversant with hydrostatics knows that a cylindrical tubule would be less distensible by a fluid under a certain pressure, than a hollow spherule of much larger diameter.

If we examine in detail the arguments adduced in support of the opinion that the cysts are dilated tubes, we shall find some weight due to the first three of them. Nevertheless, the force of the second and third depending on the identity of the contents of the cysts and denuded tubules, is somewhat weakened by the observation of Mr. Simon, that xanthic-oxide had been found by him in the former bodies. We conceive that the existence of so elaborate a product in the cysts would weigh much in favour of his theory of their origin, in preference to Dr. Johnson's. The fourth assertion, although conspicuously set forth in italic characters, could only be available as an argument after every other conceivable explanation had been shown to be inapplicable or untenable. Now this has been attempted with respect to but one, and that with no eminent success, as we think we have already proved. The nature, then, of the cystogenetic process must still be considered as open to doubt, and as requiring farther elucidation.

Another change consequent, we are told, upon the destruction of the cells which line the urinary tubes, is a diminished supply of blood and gradual wasting of the tube. Hence there is a gradual diminution in the bulk of the cortical portion until the entire organ becomes small, contracted, and granular. Dr. Johnson, as well as Mr. Simon, believes that the abundance of fibrous tissue visible in these cases is not a new production, but nothing more than the atrophied remains of the basement membrane of the tubes, with the healthy fibrous tissue apparently more abundant in consequence of the wasting of the tubes. It seems to us to be debatable matter which of the two events, viz. the non-reproduction of epithelial cells, or the diminished supply of blood, is anterior in the order of succession,—which cause and which effect. Mr. Busk indeed assigns the adhesive capillary phlebitis as the principal cause of the contraction and destruction of the glandular tissue; and again speaks of chronic adhesive inflammation of the venous plexus and tubuli uriniferi as causing partial obliteration of the former, and contraction and obliteration of the latter, or their infarction with solid albuminous matter. It is plain, therefore, that he considers the change in the vessels of the circulation a primary and not a secondary phenomenon. We are far from wishing to make any captious objection; but we must avow ourselves unable to discern in the non-adhesive and non-ulcerative inflammation of the tubules, as described both by Mr. Simon and Dr. Johnson, any sufficient explanation either of the non-reproduction of epithelial cells from the basement membrane, or of the diminished circulation giving rise to the subsequent wasting of the kidney. The latter author, indeed, speaks of the complete restoration of the organ to its former functions after suffering from acute desquamative nephritis, in which the infarction of the tubules appears in no essential particular to have differed from that occurring in the chronic disease.

At the end of the present paper is an appendix to his former one on the Minute Anatomy and Pathology of Bright's Disease.

He had before treated of that form of fatty degeneration in which on microscopical examination he found no increase in the number of epithelial cells, nor inflammatory products of any kind, but simply great accumulation of oil-globules in the epithelial cells, and consequent distension of the urinary tubules. In the second form of fatty degeneration, the cortical portion of the kidney is soft and pale, and interspersed with numerous small yellow specks. The kidney is generally enlarged, sometimes to double its natural size. In some cases the cortical portion is somewhat atrophied and granular, but in neither form of fatty degeneration does that extreme wasting and granulation occur, which is so frequent in chronic nephritis. In a case described to illustrate this disease, the patient, it is said, continually passed fibrinous casts and epithelial cells, many of which were completely distended with oil-globules; and in proportion as they became filled with oil, they appeared to lose their angular outline and become transparent and globular or oval. This modification of disease is, in fact, a combination of two diseases; namely, of the desquamative nephritis, and of fatty degeneration; each having its independent source. The nephritic condition is manifested by an increase in the number of epithelial cells; the tendency to fatty degeneration by a filling of many of these with oil.

The author then has distinguished and described four conditions of the kidney.

1. Acute desquamative nephritis.
2. Chronic desquamative nephritis.
3. Simple fatty degeneration.
4. A combination of fatty degeneration and desquamative nephritis.

He leads us also to expect from him future notices of its other inflammatory affections.

The indications of treatment which he points out are two:—first, to prevent the further formation or development of those products, the excretion of which by the kidneys is productive of serious structural changes (we wish he had told us how); and second, to relieve the kidney as much as may be of its excretory duty, by exciting to action other eliminating organs, as, for instance, the skin and bowels. He believes, and as we think justly so, that in cases of this kind, and more especially perhaps in cases of chronic nephritis, the only safe and useful diuretic is pure water.

The present paper, like its predecessor by the same author, furnishes valuable additions to our knowledge of the pathology of the kidney, and must always be consulted by such as investigate the diseases of that organ with a view either to their own information or to a farther advance of the study. If, in our notice, we have remarked upon apparent defects in his explanation of some of the phenomena observed, we have been actuated only by a wish that a recognition of doubtful points may lead to their more complete elucidation, and that so our scientific progress may be sure as well as rapid. The anxiety to obtain credit for priority in the discovery of any new fact is now so great, that the Horatian precept, that what has been written should be submitted to some competent judge, *nonumque prematur in annum*, has long been a dead letter. Hence the office of a critic often becomes more invidious than it might otherwise have been.

Note in reference to the subject of the papers of Mr. Simon and Dr. Johnson, on the inflammatory diseases of the kidney.—Ed.

1. We are here told that the existence of an inflammatory affection of the kidney, distinct from the fatty disease, was spoken of by Glüge in 1841. Vogel, in 1843, described colourless coagula of cylindrical form, as found in the sediment of the urine, the diameter and shape of which correspond exactly to the secreting tubuli of the kidneys. They contain, according to him, particles of epithelium, and sometimes also rust-coloured granules.

2. Franz Simon describes the sediment as consisting of mucous corpuscles, blood-discs, dark globules, and the cylindrical bodies, composed

of a substance which he regarded as the altered epithelial lining of the tubules.

Haller gives a very similar account of the urinary sediment in Bright's disease, and shows that the epithelium of the tubuli uriniferi is thrown off in a tubular form.

Scherer, in 1843, describes the same characters of the urine.

3. The tubuli of the kidney, after death from Bright's disease, were found filled with amorphous matter by Valentin, and afterwards by Henle.

Valentin, in 1837, describes the tubuli as filled with a yellowish-gray mass, the walls and interstices of the canals being healthy.

In 1838, he says that the disease consists in exudation of albumen in abnormal quantity, of which part is discharged and a part fills the tubules in a solid form.

Henle, in 1842, found the tubuli in part filled with what he regarded as coagulated fibrine; though this matter was exuded in greater quantity in the interstices of the tubuli.

Concerning the partial obliteration of the tubular structure, and infarction of the tubuli with albuminous matter in scarlatina, jaundice, &c., we are referred to Mr. Busk's paper in the preceding volume of the Transactions, from which we have already made extracts in our notice of Dr. Johnson's.

It would seem from this note that the Germans have somewhat outrun us in the race of discovery; but it is satisfactory to observe so many points of agreement among independent observers.

XVI. *An account of the structure of a nævus;* by John Birkett, Demonstrator of Pathological Anatomy at Guy's Hospital.

The nævus described in this paper was removed from the back of the hand of a young man. It was congenital, subcutaneous, and about the size of half-a-crown. Mr. Birkett's account of its structure is so concise, that it scarcely admits of abridgment. He states:

> "The following elements composed the growth, viz. 1. Areolar, uniting or fibrous tissue. 2. Epithelium. 3. Capillary vessels and vessels of larger calibre.
>
> "1. *On the fibrous tissue.* The mass, before careful dissection, presented an irregular growth, apparently lobulated, consisting of fibrous tissue which contained fat. When the fat and the uniting tissue were dissected off, several lobes, to the number of twelve, were recognisable. These lobes varied in size and in length, from between an eighth and three quarters of an inch, and were attached by a kind of neck to the corium, but they were perfectly free upon all other sides.
>
> "Each lobe possessed a distinct and proper capsule, which at the neck became intimately blended with the fibrous tissue of the true skin. The lobes externally resembled fibrous sacs depending from the corium, but both transverse and longitudinal sections exhibited clearly the internal structure of each. Short, thick, flat, delicate, and strong fibrous bands intersected each in every direction without much regularity, although in some of the lobes a somewhat characteristic arrangement was discernible. In these a dense and well-defined central point existed, from which the bands or septa passed off in a radiating manner, to the envelope or proper investing sheath." (pp. 193–4.)

Transverse sections of an inflated lobe exhibited the reticular character of the interior. Sections of other undried lobes display these cells, meshes, or interstices very clearly. The cells communicate with one another on all sides, and they vary greatly in their size. They open at their peripheral extremity into large spaces, or what might be termed reservoirs, in the corium. At this point, however, the different lobes forming the abnormal growth do not at all appear to communicate with each other.

2. *Of the septa, or bands, or laminæ.* The septa vary considerably in diameter. They are composed of a most delicate fibrous tissue, wavy and regular, a pavement or tesselated epithelium being arranged over their surface. The nucleus fibres of Henle, the yellow fibrous element of other authors, are found scattered and mingled with the tissue of the bands.

Vascular arrangement. Mr. Birkett was unable to inject the specimen from the small arteries supplying it. They could not be traced into the septa, nor did they appear to open into the cells, although, perhaps, both these arrangements exist.

> "Upon dividing that portion of the proper envelope of the lobe, which at its neck intimately blends with the corium, a large space, or, as I have before termed it, reservoir, is seen, through which delicate filaments traverse from the corium to the septa of the lobe. Minute examination proves that these are delicate vessels, somewhat looped, and in many instances they are distended with blood-corpuscles. They appeared to be lost upon the septa.
>
> "These reservoirs communicate with veins, which are usually found more or less distended in the proximity of the abnormal growth.
>
> "Each lobe appeared to possess two or three small arteries. I did not find one vessel supplying two lobes, but each lobe received its own distinct vessel or vessels. These vessels presented the characteristic appearances of the vascular tissues, when acted upon by acetic acid, and examined by a magnifying power." (p. 195.)

This is the best account of the minute structure of a nævus that we have met with. Though the description is lucid and clear, we consider it an omission that there is no plate to illustrate the text. Mr. Birkett, after objecting to these tumours being termed vascular and erectile, remarks, that they must be classed with the fibrous tissues, being developed probably like them, nourished by arteries which may differ greatly in size, and possessing cells which are in communication with larger or smaller veins. We regret that we cannot find space for the "practical considerations" resulting from the author's researches, for although no novelties in treatment are suggested, his views are sound and sensible.

Dr. Hake's description of the minute structure of the enlarged mamma removed by Mr. Image, and noticed at page 341, leads us to infer that the disease was essentially of the same character as the growth just described.

XVII. *A case in which a large pouch was formed in the œsophagus in connexion with contraction of the canal;* by W. C. Worthington, Senior Surgeon to the Lowestoft Infirmary.

The author, after briefly relating the particulars of two cases of a sac or pouch in the œsophagus, without any stricture of this canal, one recorded by Mr. Ludlow in the Medical Observations and Inquiries, and the other by Sir C. Bell, gives an account of a case which came under his own observation. The symptoms generally were of the character commonly remarked in cases of stricture of the œsophagus. It was noticed, however, "that during a meal a portion of food appeared to be swallowed, and for
retained, but was shortly afterwards returned very little changed;
lation taking place in a way similar to the rumination of animals."

Upon examination of the body, a pouch was discovered projecting from behind the œsophagus, opposite the cricoid cartilage, and hanging down between the trachea and œsophagus, and the cervical vertebræ; it measured 3½ inches in length, and 2½ inches in circumference, and was in shape not unlike the finger of a glove. Nearly two-thirds of this pouch were covered by muscular fasciculi derived from the pharyngeal constrictors, the fibres of which were more developed than in health. The pharynx was much dilated, and the two cavities when expanded were capable of holding nearly two pints of fluid. On a level with the commencement of the pouch existed a stricture, that would admit only a large-sized urethral bougie.

The explanation which Mr. Worthington gives of the formation of the œsophageal pouch, is similar to that commonly assigned for the production of pouches in the bladder in cases of stricture in the urethra; and, we believe, is the correct one. "As soon as an obstruction to the free passage of food along the œsophagus commenced, the action of the pharyngeal muscles, propelling the alimentary bolus against the stricture, distended the part intervening between these muscles and the stricture; and as the distension increased, one portion of the parietes of the part yielded more than the rest, and at last formed the pouch found on dissection." (p. 205.) The regurgitation of the food is the chief symptom by which such a pouch would be recognised during life. The only point in respect to treatment worthy of notice is the suggestion of Sir C. Bell, viz. to endeavour to introduce a tube into the œsophagus, through which the patient might be fed, so as to prevent the passage of any food into the pouch. Dilatation of the pharynx is commonly observed in cases of strictured œsophagus, but the formation of a pouch being of rare occurrence or being seldom recognised, Mr. Worthington's case is well deserving of record; and will direct attention to the subject.

XVIII. *On the contractility or irritability of the muscles of paralysed limbs, and their excitability by the galvanic current, in comparison with the corresponding muscles of healthy limbs;* by Robert B. Todd, m.d., f.r.s., Professor of Physiology in King's College, London, &c. &c.

This paper records the results of experimental researches undertaken by Dr. Todd, with the view of testing the correctness of Dr. Marshall Hall's assertion,—that in cerebral paralysis the irritability of the affected muscles becomes augmented, whilst in spinal paralysis the muscles supplied with nerves from the diseased portion of the spinal cord quickly lose their irritability, and that the augmented or diminished irritability of paralysed muscles (as tested by galvanism) affords a means of diagnosis between cerebral and spinal paralysis. Dr. Marshall Hall also inferred from these supposed facts, that the spinal cord is the source of muscular irritability, and that the ordinary acts of the brain serve to exhaust this irritability, which will accumulate in the muscles when the influence of the brain is withdrawn by disease.

Feeling dissatisfied with these views, which appeared to him inconsistent with many ascertained facts in the physiological and pathological history of the brain and spinal cord, Dr. Todd determined to repeat the experiments as opportunity occurred. The galvanic current was developed either by the electro-dynamic machine, which was supplied with a single cell of Daniell's constant battery; or by the magneto-electric rotation machine, which does not require a battery, and which is on that account peculiarly convenient for medical purposes. In most of the experiments, the limbs to be compared were immersed by the hands or the feet, as the case might be, each in a basin of water, which was connected with one of the wires of the galvanic apparatus. "The current," says Dr. Todd, "thus passed through both limbs at once, and both were similarly and simultaneously exposed to its influence." We cannot feel satisfied with this mode of experimenting, for reasons which we shall presently mention; but in the first instance we shall concisely state the general results which he obtained.

In the *first* place, we have a notice of thirteen cases of paralysis—mostly hemiplegia,—the cerebral origin of which was sufficiently evident from the general symptoms, and in which the excitability of the muscles of the palsied limbs (the arm being usually chosen) was clearly *less* than that of the muscles of the unaffected parts. In several instances, it is stated, the same results were obtained when the galvanic current was directly applied to the muscles; and in some of the cases, in which the paralysis had been of long standing, and the palsied muscles were much wasted, scarcely any contractions could be procured by either method of application. "It may be considered, therefore, as proved," says Dr. Todd, "that the cutting off of the brain's influence from a muscle does not (in every case at least) lead to the augmentation of its irritability."

Dr. Todd, however, does not deny that there are cases of paralysis, in which the muscles respond very regularly and vigorously to the galvanic stimulus, and even display a greater amount of vigour than the muscles of the healthy limbs; but he states that in these cases the muscles of the palsied limbs always exhibit some degree of rigidity, and that the vigour of their action in obedience to the galvanic stimulus will be proportionate to the amount of rigidity, within certain limits. It is not, however, in all cases in which rigidity accompanies paralysis, that this condition exists; but only in those in which it presents itself along with, or very shortly after, the paralytic seizure. "The early rigidity of the palsied muscles accompanies a state of irritation of the brain, and will disappear when that irritation is subdued. Red softening, a tumour, meningeal disease, inflammation around a clot, are all capable of producing this state of muscle." The grounds of this statement are not mentioned; and as we believe that it is new in some of its particulars, we should like to know more of the evidence on which it rests. On the other hand, rigidity following the paralytic seizure at a distant interval is attributable, according to Dr. Todd, to the gradual contraction of the healthy brain around the softened portion or the apoplectic clot; which explanation, like that which attributes the early rigidity to red softening of the brain, seems to us to require more evidence in its behalf. In other cases, again, continues Dr. Todd, "while the paralysis is pretty complete, the galvanic stimulus excites equally the muscles of the sound and those of the healthy limbs. These are generally cases of apoplexy occurring in persons previously healthy and not advanced in years. The muscles are healthy and well-nourished; and, after the first shock has subsided, they respond readily to the galvanic stimulus, but not more so than those of the sound limb." Cases are then quoted in illustration of these statements; and we then find the following summary of the experimental results:

> "I have thus referred to three classes of cases, in each of which the paralytic limbs respond differently to the galvanic stimulus. In the first class, the stimulus produces little or no contraction; in the second, it causes vigorous contractions, and even of a more lively character than those in the sound limbs; and in the third, contractions are excited of a more or less vigorous kind, but which exhibit little or no difference from those of the healthy limb.
>
> "In the first class of cases, the paralysed muscles may be more or less wasted, or they may present no difference in point of nutrition from those of the sound limb. In my tenth case there was no difference as regards the nutrition of the muscles, between the sound and palsied limbs; yet the electric current excited scarcely any contractions in the latter.
>
> "In the second class, the paralysed muscles at first exhibited no loss of nutrition —on the contrary, they are manifestly firmer than the healthy muscles; if the palsy yields to treatment, they lose this increased firmness or rigidity, and assume the natural condition; if, on the other hand, the disease obtains the mastery, the muscles waste, although in some instances they continue to maintain their rigidity.
>
> "In the third class, the muscles retain their normal condition, and will continue to do so if the patient is not slow in recovery." (p. 222.)

From a review of these results, Dr. Todd concludes that the effect of the galvanic stimulus upon a paralytic limb is always feeble when there has been much wasting of the limbs; but that, when the nutrient condition of the muscles has been but little impaired, its effect may be greater or less, according as the *vis nervosa* of the nerves supplying them is in an exalted or a normal condition, and is more or less readily called into action, therefore, by the galvanic stimulus. He is consequently of opinion that,—

> "It is in vain to found any distinction between cerebral and spinal palsy, upon any difference that may exist between the paralysed and healthy muscles as to their excitability by galvanism. The very same states of muscle and of nerve exist in spinal palsies as in cerebral. There is this difference, however, that as in spinal palsy the nerves are more nearly related to the seat of the lesion,—sometimes, indeed, implanted in it,—they participate more directly, and therefore more completely, in the effects of the lesion than in cerebral palsy; and when these effects are depressing, the nervous force is more completely depressed, or when they are irritant, the nervous force is more exalted, in spinal than in cerebral palsies. In a case of intra-spinal disease, which occurred about twelve months ago in King's College Hospital, the lower extremities were in a state of continued tonic contraction more intense than I had ever seen them in tetanus. These muscles responded readily and very forcibly to the galvanic stimulus. Yet the influence of the will was completely withdrawn from them, and the paralysis of voluntary motion and of sensation was complete. Muscular rigidity to a less degree is by no means uncommon in spinal palsy." (p. 226.)

With regard to the well-known tendency of strychnine to induce convulsive twitchings in the palsied limbs, earlier and more strongly than in the healthy ones, Dr. Todd remarks that this shows itself as much in cases in which the lesion is spinal, as in those in which it is cerebral; and he considers it attributable to the attraction of the poison in greater quantity to the seat of the lesion, than to the corresponding part on the other side,—according to the principle some time ago pointed out by Dr. William Budd.*

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* Medico-Chirurgical Transactions, vol. xxv, p. 199.

Although our own views on this question for the most part accord with those of Dr. Todd, yet we do not think that either his experiments or his inferences are so satisfactory as to be beyond the reach of well-founded objection. They have, in fact, been already attacked by an acute partisan of Dr. Marshall Hall:* and we shall endeavour to show what are the strong and what the weak points of each combatant.

In the first place, no fact adduced by Dr. M. Hall in the least degree invalidates the conclusion founded upon a great variety of phenomena, that the contractility of muscles is an endowment inherent in their own substance, and not derived from any extrinsic source; and that its amount depends upon the activity of the nutrition of the muscular substance, which again is in close relation with the amount of use to which the muscle is subjected. The well-known experiments of Dr. J. Reid have fully accounted for the fact, that muscles paralysed in regard to the brain by section of the spinal cord retain their contractility longer than those which have been cut off from the influence of either brain or spinal cord by division of their nerves,—without the adoption of Dr. M. Hall's inference, that the spinal cord is the source of their contractility; for it was pointed out by Dr. Reid that the frequent exercise of the muscles by reflex action, in the former case, would account for the comparative persistence of their contractility, through its influence in sustaining their nutrition; whilst, in the latter case, the same effect might be produced without any influence whatever from the spinal cord, the muscle being called into occasional exercise by the direct application of the galvanic stimulus, and its nutrition and contractility being thus sustained,—the contractility being restored by rest after temporary exhaustion by violent exercise, and hence being evidently dependent upon the nutritive operations, and not upon the spinal cord from which the muscle had long been completely separated. So far, therefore, we fully agree with Dr. Todd.

In regard, however, to the question of *fact*,—whether the muscular irritability of limbs paralysed by cerebral or by spinal lesions is greater or less, respectively, than that of the corresponding healthy limbs, we think that Dr. Todd has not understood Dr. M. Hall aright, and that his experiments are not adapted either to disprove or to confirm Dr. M. Hall's statements; the peculiar sense in which the term irritability is employed by Dr. M. Hall not having been sufficiently kept in view. By physiologists in general, muscular irritability is spoken of as the greatest or highest, when the application of a stimulus produces vigorous contractions, as in the muscles of a recently killed bird or animal; and it is considered to vary directly as the activity of the respiration in animals of different classes. On the other hand, the measure of irritability adopted by Dr. M. Hall is *not* the *amount* of contractile force generated by a given stimulus, but the degree of feebleness of the stimulus necessary to excite contraction; and thus, as the muscles of a frog may be caused to contract energetically by a galvanic current so feeble as to produce no effect whatever on the muscles of a bird or mammal, he speaks of the irritability as higher in the former. So in the case of certain mammals, the left ventricle of the heart, which ordinarily requires the stimulus of arterial blood to throw it into contraction, will act on the stimulus of blood almost venous during the condition of hybernation; and hence Dr. M. Hall states that the degree of muscular irritability in the different animals or in diffierent states of the same animal, varies *inversely* with the amount of respiration. Both these statements are perfectly correct in themselves; but the term irritability is employed in one case with reference to the *amount* of contractile power manifested, without reference to the facility with which it may be called into play; whilst in the other it denotes the *facility* with which it may be called into exercise, without reference to the amount of muscular force generated. Whether or not it was wise in Dr. M. Hall to depart so widely from the ordinary method of using this term, we shall not now stop to consider; but in every statement of that gentleman's into which it enters, the peculiar sense in which he uses it must be borne in mind.

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* Dr. W. Tyler Smith, in Lancet, Nov. 6, 1847.

Now Dr. Todd has tested the comparative irritability of the muscles of the sound and paralysed limbs by a *powerful* galvanic combination; and has judged of its degree by the comparative vigour of the contractions induced. On the other hand, Dr. Hall has employed a very feeble galvanic combination; and has formed his judgment rather upon the relative slightness of the stimulus requisite to produce obvious contractions in the two cases. Thus, as Dr. W. Tyler Smith justly observes, "the experiments related by Dr. Todd and Dr. M. Hall are essentially different; and for anything which yet appears, *both* series of experiments and facts may be perfectly correct." We know that the irritability of the muscles of a bird or mammal is *higher* than that of a frog, in the sense usually received; whilst in Dr. M. Hall's sense it is *lower*. And we can easily conceive that, in cases in which the nutrition of muscles has been so much impaired by long disuse that the contractions excited in them by a powerful stimulus are much feebler than those of a sound limb, the former may be more excitable than the latter under the influence of a feeble stimulus. An interesting case of hemiplegia is related by Dr. Smith, which would seem to establish this view; for although the nutrition of the affected side had suffered considerably, so that the muscles were wasted and lax, a feeble galvanic current, which produced no effect whatever upon the sound limb, moved the corresponding paralysed limb very distinctly. The method of experimenting in this case, and we presume in the remainder of those alluded to by Dr. Hall, appears to us much superior to Dr. Todd's; for the latter gentleman placed the two limbs in two separate basins of water, and connected these with the two poles of the electric apparatus, so that the current must have passed *directly* along one limb, and *inversely* along the other; whilst the former placed both limbs in the same basin, and transmitted the current from a central point (as, in the case of the arms, from a penny-piece placed on the upper part of the sternum, or, in the case of the legs, from the basin in which the hands were immersed), so that it passed along both limbs in the same direction. We do not say that the result *would* have been different in Dr. Todd's cases, if Dr. M. Hall's method of experimenting had been adopted; but the results of Matteucci's experiments show such a marked difference between the effects of the direct and of the inverse currents, that no experiments can be satisfactory in which this difference is left out of view.

We consider the distinction drawn by Dr. Todd, between the actual contractility of a muscle, and the facility and energy with which it may be excited by a galvanic stimulus acting through the nerve, as a perfectly valid one; and as deserving careful attention. For although it may be safely asserted, that a muscle whose nutrition has been greatly impaired cannot develope a high amount of contractile force, yet it does not at all follow that a muscle which retains a nearly normal condition, should be capable of being thrown into vigorous contraction by a galvanic stimulus applied to its nerve. For it must be remembered that, in any such experiment, it is *not* the electricity which acts on the muscle, but the *vis nervosa* excited in the nerve-trunk by the passage of the electric current; and the readiness with which this condition may be induced in the nerve-trunk depends upon its own state of nutrition. It has been experimentally shown that, after death, or after the separation of a nerve-trunk from its centres, the excitability of the nerve-trunk is lost much sooner than is that of the muscle; so that an electric current transmitted along the nerve-trunk shall have no influence, whilst the muscle can still be called into contraction by the direct application of the stimulus to itself. And we believe, therefore, with Dr. Todd, that the state of nutrition of the muscles and of the nerves which supply them, has much more to do with the degree in which muscular contractions may be called forth by a galvanic current transmitted through the nerves, than has the spinal or cerebral origin of the paralysis. But, on the other hand, it must not be forgotten that the state of nutrition of the muscles and nerves, may be partly dependent upon the seat and nature of the lesion; for if it be such as allows of the persistence of the reflex actions, experience as well as theory indicate that the excitability both of nerves and of muscles is kept up; whilst, if the conditions of reflex action be interfered with, this excitability gradually diminishes as the nutrition becomes impaired,—although, even then, it is quite possible that such contractile power as remains may be called forth with unusual facility, in consequence of the absence of the ordinary exhausting effect of exercise. We see no reason to discredit the statements of Dr. M. Hall, as confirmed by Dr. W. Tyler Smith, on this last point; and if their views be correct, the peculiar action of strychnia is more satisfactorily explained in accordance with it, than it is by the hypothesis of Dr. Todd. It is in harmony with one class of facts, to which Dr. Todd makes no allusion, but which seems to us to have a very important bearing on the question;—namely, the increased facility with which (as every physiologist knows) reflex actions may be excited by ordinary stimuli, after the influence of the brain has been suspended by division or injury of the spinal cord. We can scarcely suppose that the phenomenon is due to the removal of any antagonizing influence; since we are not ourselves conscious of exerting any influence of this kind to prevent such reflex movements as are excited by very slight causes in the legs of many patients, rendered paraplegic by disease or injury of the spinal cord. We apprehend, therefore, that there must be an increased excitability of the nerves or muscles (probably the former) in such cases; and this fully accounts for the peculiar action of strychnine already referred to.

It will thus be seen that, whilst we accord with Dr. Todd in the general correctness of his conclusions, and dissent altogether from the opinion of Dr. M. Hall, that the spinal cord confers irritability on muscles which the action of the cerebrum exhausts, we by no means consider that the experimental results obtained by the former, disprove the correctness of those of the latter, which may very probably in our apprehension be equally true, although they do not warrant the deductions which have been drawn from them.

XIX. *Fatal case of dysphagia produced by a polypous growth in the œsophagus;* by R. Arrowsmith, M. D., Senior Physician to the Coventry and Warwickshire Hospital.

In this case extreme dysphagia, causing death by starvation, was produced by a polypous growth, somewhat larger than a walnut, attached to the mucous tissue by a short, thinnish, fibrous base. The growth commenced about half an inch from the posterior commissure of the glottis, and extended for the same distance in a straight line in the axis of the œsophagus. The structure of the tumour is said to be "vascular and homogeneous;" but the author was evidently unable to make out its real character. We suspect that it was a fibrous tissue. This tumour formed a mechanical obstacle to the perviousness of the œsophagus, and, by passing under the epiglottis during attempts to swallow, prevented the closure of the glottis, and thus allowed fluids to pass into the trachea. The tumour was not discovered during life. Another smaller growth was found nearly two inches lower down the œsophagus.

ART. IV.

Traité de la Salubrité dans les Grandes Villes, suivi de l'Hygiène de Lyon. Par les Docteurs J. B. MONFALCON et A. P. J. DE POLINIERE, Membres du Conseil de Salubrité de Rhône.—*Paris*, 1846.

A Treatise on Salubriousness in Large Towns, together with an Essay on the Hygiene of Lyons. By Drs. MONFALCON and DE POLINIERE, Members of the Council of Health of the Rhone.—*Paris*, 1846. 8vo, pp. 552.

THE progressive evolution of the science and practice of public hygiene in the United Kingdom is evidently in relation with our municipal and political institutions. It is manifest that a more effective provision for a better sanitary condition of our towns and villages will depend in a great degree upon the amount of hygienic knowledge possessed by those of the middle class to whom legislation and the local government of the people is confided. When we consider the sad proof of the total ignorance in which these functionaries are now placed, as displayed by the recent debates on the Health of Towns' Bill, the prospect thus afforded of hygienic measures seems gloomy. Municipal authorities are proverbially immersed in a mediocre stupidity. The more enlightened members are overborne by the more astute, who find it to answer their purpose better to represent the selfish cupidity of their constituents—the holders of property, and especially small property—than to take steps for the amelioration of the condition of the large population intrusted to their care. Hence we find an especial reluctance on the part of municipal authorities to lay out money for the better sewerage and cleansing of the streets; to the regulation of lodging-houses; to prevent the sub-letting of tenements; to interfere with slaughter-houses, or manure heaps, or to supervise manufactories that are nuisances, either from the smoke or deleterious emanations they emit, the stenches with which they poison the air, or the defective interior arrangements by which the health of the unfortunate men doomed to work within them is injured or their lives shortened.

In the work before us we have the experience of the Council of Health of a large manufacturing town in effecting these necessary measures; the difficulties of the attempt are not concealed, nor the origin of these difficulties; and, in addition, the modes of meeting those difficulties are set forth. The authors state that for twenty years they have been attached to that council, and for thirty-eight years have been in relation with the civil and military hospitals of Lyons, under the various offices from *élève interne* to physician and directing administrator. With this experience, the work is avowedly addressed, not only to physicians and councillors of health, but specially and above all, to mayors, to members of municipal councils, and to all those functionaries whose duty it is to watch over the preservation of the public health. It is eminently a practical work on the subject, and differs materially in this respect from those of Frank, Levy, &c.; indeed, the authors claim the merit for it, that it is the first of its kind. We shall confine our notice of the work to the practical points only, and endeavour to put our readers in possession of such information as may enable them the more effectually to take their part in the hygienic reformation now in progress, and which belongs as emphatically to the medical profession and medical science, as the great religious reformation belonged to the clergy and to theological science.

Monfalcon and his colleague first lay down general principles; and the more important to us of these is that which asserts the necessity for the intervention of authority in matters regarding the public health. However good may be the measures recommended, and however great their necessity, if the public authorities do not carry them out, they remain without result. If practical public hygiene had not become a legislative question in France, it would have never existed. Indolence, ignorance, and avarice would be always encroaching on public convenience and public health, unless checked by authority. This has been indeed acknowledged in England for centuries; and the only question which was really in debate during the last session of Parliament, was as to the extent to which compulsory supervision by the government and legislature should be carried. The representatives of municipal authorities resisted the central authority, and virtually pleaded that they needed no control, and were therefore free as corporate bodies from those charges of indolence and ignorance and avaricious incapacity, to which, undoubtedly, many of their constituents, and even some of their individual components are liable. There can be no doubt whatever that local authorities need supervision and control; all experience has established this. Whether, however, there shall be inspectors appointed for this purpose, or whether the jurisdiction of the ordinary courts of justice might not be made available, admits of question. We incline to the latter view, and think that if fine and imprisonment were meted out by those courts to such municipal authorities as may be found guilty of a dereliction of their duty towards their fellow-citizens in the matter of sanitary arrangements, a salutary dread of punishment would lead them to eschew indolence and ignorance, and the Blackstone of public hygiene would be as much in request by civic grandees, as the great legal commentator by country justices.

In France, at the close of the 18th century, the laws regarding medical police were in a condition as confused as the same class of laws of this kingdom. A number of ordinances existed, but there was no unity, energy, or completeness in the legislation, while the law was at the same time intolerably arbitrary and almost powerless. Every mayor and prefect interpreted the laws as he pleased; sometimes manufactories essentially insalubrious or annoying were freely permitted, sometimes industrial pursuits of the most inoffensive kind were pestered with useless restrictions.

Under these circumstances, the Council of Health for Paris was created on the 7th of July, 1802. Its duties were to supervise the manufacturing establishments, reputed as nuisances or noxious to the public health; to examine into all complaints made against them; to modify the processes carried on in them, or, if necessary, to suppress them, on the ground of being absolutely dangerous. This council had to make an annual report. Its establishment was an improvement, but its power was limited to the metropolis, and, besides, its decisions were arbitrary and founded on no fixed principles. Agitation, of course, continued, for industry was fettered, and the use of capital restricted by the will of the council.

Urged by this continued agitation, the French Government of the day applied in 1804 to the Institute for information, and a report was returned setting forth the elements of legislation on the subject. In this the reporters fixed in what proportion certain industrial processes were noxious or obnoxious, and thus gave the Council of Health some determinate principles by which its decisions should be regulated, and to manufacturers a knowledge of what was required from them. This first report was defective in several particulars; it did not state what manufacturing processes should be prohibited entirely in inhabited localities, what others might be permitted if subject to constant inspection, and left others out of consideration altogether.

A decree of Napoleon, dated October 15th, 1810, supplied these defects. It divided manufacturing processes into three classes, and stated the reasons for placing each in one class or the other. This decree was rendered the fixed code of manufacturing industry by a royal ordinance, dated 15th January, 1815. Sixty-seven distinct manufactures were registered under the three classes at the outset; at the present day there are more than three hundred and fifty. So much have the arts advanced in France.

A law being established, and fixed principles for carrying it out being promulgated, it remained to determine to whom that duty should be intrusted. The government, feeling its own incapacity, established councils of health or consulting committees, by whose advice the decisions of the constituted authorities should be regulated; and this is the present condition of France as regards public hygiene. Our authors observe:

> "Public hygiene is a new science of an elevated kind in our social economy; made up of observation entirely, and rejecting all the illusions of theory, it is always practical; its highest development is the establishment of councils of health. These committees have for their noble mission the amelioration of the material condition of the labouring classes; their special object is the protection of the health of the citizen from certain manufacturing pursuits which might affect it, and to render noxious or insalubrious workshops innocuous both to the workmen and the adjoining residents. It is hardly credible how rich this field, so little explored, is in discoveries; and how much certain persons have extracted

from the matters scattered amidst the filth of our streets, the sight of which can only inspire disgust. The cleansing of cesspools and sewers is become a valuable science; the business of the knacker—so hideous at first sight—when examined in all its details and applications, is found to be one of the most interesting points in public hygiene, and a fertile source of commercial wealth. Whilst chemistry finds means to get rid of a large portion of repulsive organic remains, whether in the manufacture of gelatine or of phosphorus on a large scale, hygiene, acting through the council of health as its organ, determines the amount of respirable air necessary to each person, and regulates the capacity of prisons, hospitals, barracks, and theatres, in proportion to the number of persons they have to contain. It declares by the same voice what qualities belong to salubrious water, by what signs the adulteration of indispensable articles of consumption, as meal, bread, wine, sugar, milk, may be detected, and regulates in all their details those conditions, without which the health of the population is compromised, and the ordinary duration of life shortened.

"Being the natural protectors of industry, as well as of the public health, the Councils of Health are identified with the carrying on of the arts and manufactures; they study these, not only to render them the more salubrious, but sometimes also the more productive. One of their most important objects is to acquaint the masters of the workshop with all the experience which civilized nations have acquired in reference to this double relation." (pp. 26-7.)

The authors add to these duties of the Councils of Health the supervision of new public buildings, the visitation of prisons, barracks, and hospitals; the promulgation of instructions for the recovery of drowned or suffocated persons, the direction of dispensaries for meretricious women, the publication of vital statistics, and the repression of quackery. It does not appear that many of the large towns of France enjoy the advantage of these Conseils de Salubrité; Paris, Nantes, Bordeaux, Lyons, Marseilles, Lille, and Rouen are those mentioned by Monfalcon and his colleague. Nor does it appear that these councils co-operate with each other; for the authors take occasion to suggest that there should be a regular interchange between them of their annual reports. It is remarkable, indeed, that so simple and necessary a step as this should not have been adopted spontaneously.

We do not propose to follow our authors through the details which they give as to the ventilation, warming, lighting, and construction of houses and streets, nor as to the management of water-closets, cesspools, sewers, and public conveniences. With all our irregularity and wilfulness, we English seem to be in most respects equal to our neighbours in these points, and in many respects superior, although there is certainly a wide difference between our best and worst conditioned towns. Of the latter, we fear many are not to be compared with towns of the same size in France. We are glad to see that the Reports of the Registrar-General, and of the Health of Towns' Commissions, are freely laid under contribution by the authors of the book before us. Something may be learnt in return from their work, especially as to the construction, disinfection, and cleansing of water-closets and cesspools, but particularly the disinfection.

Neither do we propose to review the practice of the writers as to the hygiene of workshops, colleges, prisons, hospitals, theatres, &c., but proceed rather to notice that part of their work which considers the hygiene of manufactures. We have already observed that the law has divided manufactures into three classes, each being annoying and insalubrious in different degrees. Those of the first class are not permitted at all near dwellings, and can only be established by a royal ordinance, issued by the Conseil d'Etat. In this category are included manufactories of the nitric, sulphuric, and hydrochloric acids, and of lye-ashes; melting establishments using a naked fire; workshops for the preparation of taffetas and varnished tissues; the premises of knackers, tripemen, and catgut manufacturers; those also in which are prepared animal black, glue, Prussian blue, blood-manures, "orseille" (a kind of dye), and starch; and factories of phosphorus and lucifer-matches or fulminating compounds. The reasons for placing these in the first class is the danger of fire, their actual injuriousness to health, or the intolerably fetid odours which they emit, although not actually noxious. They can only be established after prolonged and numerous formalities. The demand for permission to do so is first addressed to the prefect, and is then posted, by order of the communal mayors, in those places situate within a radius of six kilometres (about 13,000 feet) round the proposed locality. It remains posted for one month, and during this period the mayor receives objections, and enters them in a special register. The local authorities then draw up a report *de commodo et incommodo*, and transmit all the documents to the prefect. The prefect transmits the file of papers to the Council of Health, which appoints a commission of inquiry to visit the spot and hear the objections; their report on the facts is then discussed by the council, and it afterwards returns all the documents to the prefect, with its opinion as to the propriety of granting or not granting the required permission. If there be an opposition to this permission, as is ordinarily the case, the matter is referred to the council of the prefecture. The opinion of the latter is not a judgment, against which the condemned manufacture has no appeal; it is communicated to the proprietor, who may either desist or persist in his speculation; if the latter, all the documents are referred to the Minister of Commerce, and a royal ordinance is granted or withheld. If withheld, and the manufacturer have already constructed his premises, he is required to pull them down, or not to use them for the purpose intended: this often occurs.

The second class of manufacturing establishments comprises those, of which the removal from an inhabited locality is not strictly necessary, but which can only be permitted after it has been clearly shown that no process will be adopted in them which will either inconvenience or injure the neighbouring holders of property. Lime or plaster-kilns, high pressure steam-engines, gas-works, tanneries, foundries, hat-factories, manufactories of sulphate of iron and zinc, of sulphate of soda in close vessels, of phosphorus, of imitation-trinkets, bituminous mastic, chandleries, whether for tallow or composition candles, and workshops for cleansing verdigris from copper, are all in this category.

None of these are actually injurious to the health, but many are disagreeable, and annoy either with smoke, noise, stench, or the danger of fire. The demand for permission to establish any one of them is addressed to the prefect or vice-prefect, who transmits it to the mayor of the commune, that he may make an inquiry *de commodo et incommodo*. The Council of Health then gives its opinion as in the preceding class, and the prefect issues his decree. If permission is refused, the applicant can appeal to the council of the prefecture, and from thence he can go to the Conseil d'Etat. The same course is open to the opponents.

The third class comprises lime-kilns that are in operation one month only in a year, potteries, brick and tile-works, manufactories of gelatine and isinglass, crucible foundries, dye-works, &c. The method of obtaining permission is the same as in the second class. They are sanctioned near habitations, but are subject to the inspection of the prefect who grants the permit.

There is nothing absolute in this classification; an improved process may remove a business from the first to the third class. New manufactories are classified by the prefect, subject to the judgment of the Minister of Commerce, and these are placed in supplementary lists.

That a commercial establishment shall be protected by the permission granted, it is necessary that the proprietors keep to the conditions imposed upon them. The authorization expresses what articles shall be produced, by what processes, and in what quantity; and, if the works be extended or the processes altered without permission, the authorization is forfeited. An establishment of the first class cannot add any of the second or third class, nor can it be transferred from one locality to another. It does not appear that these regulations are very strictly adhered to. It is remarked that the Councils of Health have had more than once to lament that persons have made no scruple in manufacturing other products than those they are licensed to manufacture, and by processes notoriously insalubrious.

The Conseils de Salubrité have simply to decide whether a manufacturing process be insalubrious or obnoxious—"insalubre ou incommode." If it give off noxious emanations, as the sulphuric or nitric acids, or the oxyde of carbon or sulphuretted hydrogen, it is both insalubrious and disagreeable. It is *dangerous* when there is risk of explosion or conflagration.

Should the vegetable products of the surrounding district suffer injury, as by the manufacture of sulphuric acid from pyrites, the process is declared insalubrious. Insalubrity is the greatest objection.

To be disagreeable, or obnoxious, or a nuisance, is the next in order; this is to be "incommode," a word having a very elastic meaning. In cases of this kind, sentence is passed according to individual circumstances, and the rights of capital are often respected, on the one side or the other, as may be most just. Various processes may be deprived of all unpleasant smells or smoke by chemical or mechanical processes, and the Conseil de Salubrité may direct that these processes be adopted in certain instances of this class of manufactures. These directions are, however, evaded by avaricious or needy manufacturers, and some difficulty has arisen in restraining their proceedings, and not, at the same time, fettering commerce.

As an illustration of the kind of establishments placed under the supervision of the Councils of Health, we will take the knackeries, which are brought to a state of perfection far beyond anything to be seen, we believe, in this country. At these places horses are principally received, but all other animals are taken in, as dogs, cats, &c. A living horse being taken to the knackery, it is first tied to a stake, and a knife is plunged into the thorax. The blood is carefully collected, and as soon as the animal is dead of the hemorrhage caused by dividing the large vessels, it is skinned, dismembered, and the flesh cut into large slices is put into a caldron and boiled for twelve or fifteen hours. The boiler contains a liquid mass, after removing the solid parts, consisting of three layers—fat, a gelatinous fluid, and a deposit of blood and organic remains. Every portion of the animal is applied to some useful purpose. The skin is worth from 9 to 15 francs; the hairs of the mane and tail sell for from 10 to 30 centimes. The blood is used by sugar-refiners, or is given to poultry and pigs; it is worth from 2 to 3 francs. The fat is often worth as much as 25 francs, or £1. The flesh is eaten at Paris very generally; the knackers never taste any other, and they are quite careless whether the horseflesh they eat is brought to the yard alive or dead, or of what disease it died! The viscera sell, the hoofs sell, the nails and shoes sell, the dung in the large intestines sells; altogether, a horse that is sold to the knackers for 10 or 15 francs will bring from 60 to 100.

It is remarkable that Monfalcon and Polinière agree with other writers in maintaining that these horrid centres of putrefaction are not insalubrious; nothing, they say, is better established, and the principle holds good as to the emanations of all putrefied organic remains. They have ascertained that the workmen enjoy good health and a long life; they have seen women at Montfaucon who nursed "des enfants superbes," and who used the carcase of a horse for their cradle. In short, there is not the slightest doubt that the common prejudice is erroneous. It is difficult to reconcile these statements with others, as to the great noxiousness of emanations from graveyards; in what do the putrid emanations from these differ from the emanations from putrid horses, dogs, and cats? Is it in the more free exposure to the atmosphere? or is the notion respecting the noxiousness of graveyards a prejudice too? This point wants clearing up.

D'Arcet, Parent-Duchâtelet, Payen, and others have so much improved the knackers' processes, that it is probable the abomination of Montfaucon will be done away with, and a central establishment erected, adapted to new processes of disinfection. The knackery at Gravelle, conducted by MM. Payen, Buran, and Cambacéres, is so arranged that it might be in the heart of Paris, according to the opinion of the Council of Health, without producing the slightest inconvenience. Animal charcoal is used as the disinfecting agent. A knackery is a sort of slaughter-house or abattoir; and we shall now notice the latter establishment, as the question of public abattoirs is being agitated in England, and especially in the metropolis. In France they are only of recent origin; there, as in England, the opinions of enlightened and thoughtful men long remained unfruitful. A thorough reform was to be effected in one of the most important trades of a large town; inveterate habits were to be overcome, and old establishments suppressed. Our authors think that it is sometimes useful to compare the past with the present; and the former is thus described with reference to the old system of butchers' shops and slaughter-houses. We subjoin it because it is strictly applicable to those of the United Kingdom at the present moment. This is, indeed, a sad reflection.

> "Nothing enables us the better to appreciate our present condition than a recollection of the past. A few words on the ancient slaughter-houses may not therefore be out of place. Around the butcheries (*boucheries*) secondary establishments were collected of a very obnoxious kind, such as triperies, and depôts of recent animal matter. Herds and flocks of oxen and sheep were met at all

hours of the day, obstructing the most populous streets. The blood of the slaughtered animals overflowed the kennels, and dung-carts were seen stationed in the most elegant quarters of the city to receive organic remains of every kind. Some of these slaughter-houses, in consequence of their situation, were frequented thoroughfares, which could not be traversed without horror and disgust. The public health was also compromised in another way, for the oxen sometimes escaped from the place to which they had been driven for the purpose of being slaughtered, and in their rapid flight spread disorder and terror." (p. 226.)

The abattoirs are thus described: the cabins appropriated to abattage (*Anglicé*, felling or knocking down) are flagged, and built strongly with stone. They have a slope and trenches to carry off liquids, and their temperature is lower than that of the surrounding atmosphere, although not sufficiently low. Each has a water-tap, a trough to receive the blood, beams and pulleys to hoist the carcases, and hooks for the joints. The contents of the stomach and intestines are washed away by an ample supply of water, and pass into a large sewer with a good fall. In the second story are grated drying rooms. The joints are carried in covered carts to the butchers' shops, which are described as neat, and even elegant buildings. It appears that swine are not received at the abattoirs, but disturb every quarter of Paris with their death-screams.

The first part of the work concludes with the official list of insalubrious establishments, the reason of their being on the list, and the date of their entry; they are arranged alphabetically.

The second part of the work is a monograph on the hygiene of Lyons. There are numerous interesting details, and it is singular to remark how closely the French manufacturing town of Lyons resembles the British Manchester or Glasgow. Quackery, for example, is just as rampant in the former, despite of the laws against it, as it is in the latter, unrestrained by laws. Handbills are stuck up at every corner; indecent advertisements occupy a whole page of the local newspaper, promising the cure of several diseases for a few francs. One quack boasts a royal patent, another is a doctor of all the several faculties in the kingdom. An infallible elixir or a purifying vegetable syrup will cure the most inveterate ills. A vulgar clothes-dealer will excel the most expert practitioners in fame as a bone-setter; indeed, it is popularly supposed that the regulars know little or nothing of injuries to limb. Gold these quacks seek and get; and one has as large a practice as twenty doctors put together! and so Monfalcon and Polinière go on lamenting the neglect of the scientific practitioner by the public, in favour of the boasting, ignorant pretender.

It appears that the local authorities could suppress the roguery and indecency of these charlatans, if they would condescend to execute a not very stringent law. They rarely punish, however, and when a penalty is actually inflicted, it is only laughed at.

The inspection of the prostitutes does not prevent the spread of venereal diseases; the handbills of the quacks sufficiently show this. There are 300 registered prostitutes in a population of 200,000; 70 or 80 more are in maisons de tolérance; there are 20 clandestine establishments, and about 300 prostitutes "in chambers." Of the registered prostitutes, about a fifth are minors, although it is illegal to act as such under the age of twenty-one. Nine physicians have the duty of inspecting them, and there is an hospital for their reception when infected; but the formalities requisite to admission are such as to prevent them applying for relief. The proportion of the healthy to the diseased has been gradually increasing, as in Paris, where it rose from 9 to 1 to 54 to 1. To meet the feelings of prostitutes and working men affected with syphilis, a secret dispensary has been established, so that there is no fear of exposure, and the patients can be treated at their homes. So far the results are excellent.

We here terminate an imperfect notice of this interesting work; those interested in the current questions of public hygiene will find much information in it; they will also learn how much has to be done before many of the preventible causes of diseases can be removed, or even seriously modified.

Art. V.

1. *Correspondence on the Subject of the "Eclair," and of the Epidemy which broke out in the said Vessel.* Presented to the House of Commons by Command of Her Majesty, in pursuance of their Address on the 23d January, 1846.—*London.* Folio, pp. 94.
2. *Report on the Fever of Boà Vista.* By Dr. M‘William. Presented to the House of Commons, in pursuance of their Address on the 16th March, 1847.—*London.* Folio, pp. 112.
3. *Letter addressed by Sir William Pym to the Lords of the Council, relative to a Report on the Fever at Boà Vista, by Dr. M‘William.* Presented to the House of Commons, in pursuance of their Address of May 14th, 1847.—*London.* pp. 16.
4. *Report of a Special Committee of the House of Assembly of the State of New York, on the present Quarantine Laws.—Albany,* 1846. 8vo. pp. 313.
5. *Report on the Climate and Principal Diseases of the African Station, Compiled from Documents in the Office of the Director-General of the Department, and from other Sources, in compliance with the Direction of the Right Honourable the Lords Commissioners of the Admiralty, under the immediate Direction of Sir Wm. Burnett,* M.D., K.C.H., F.R.S. By Alexander Bryson, M.D.—*London,* 1847. 8vo, pp. 266.
6. *A Dictionary of Practical Medicine.* By James Copland, M.D., F.R.S. Parts X. and XI.—*London,* 1847.

We resume this subject from our last number; and will here, for the sake of making our argument perfectly clear, recapitulate our conclusions.

We have proved, we trust unequivocally, that the yellow fever is sometimes contagious; but we have denied that this contagious fever is as yet known to possess any peculiar diagnostic marks which can distinguish it from fevers which closely resemble it. In order therefore to see whether we are justified in generalizing our observation, and in concluding from the few examples of contagion we have chosen, that the epidemic yellow fever is always contagious, and is to be distinguished by this character, although it presents no other pathognomonic mark, we may adopt one of two ways: we may enter into a general and complete examination of evidence, a course objectionable for many reasons, or we may examine into the mode in which this contagious virus is said to originate. For example, we find it asserted that the malarious yellow fever, derived we will say from the coast of Africa, will give rise, under certain conditions of ventilation, temperature, production of effluvia from crowding human beings together, &c., to a reproducing and self-multiplying virus. This is the opinion of Sir W. Burnett,* to cite the name of one who has taken an active part in this controversy. Now it is evident at a glance that the determination of this question is a necessary step; in fact, the whole bearing of the discussion rests on this point. From an imperfect consideration of it much confusion has arisen. Not in yellow fever only, but in other cases, we find some writers admitting the generation of a virus in this way, others denying it, and neither seeing that the point is yet doubtful. But those who deny like Bancroft the contagious nature of the yellow fever, and those who admit it unconditionally like Sir W. Pym, both find it necessary to disallow this presumed generation of virus.

From this common point each party starts; the same admissions are to be conceded to each, and if these be overthrown, it follows that the deductions and results on both sides fall with them.

As illustrative of this subject, we will recall to the recollection of our readers the line of argument adopted by Bancroft.† In his 'Gulstonian Lectures' of 1806, and in his Essay published five years subsequently, he arranges his materials in the following way: Part I. contains a few remarks on the nomenclature, the symptoms, the diagnosis, and the treatment of yellow fever. Having thus defined the disease he is treating of, he proceeds to Part II.; he here proposes for discussion two problems: 1st, Are all fevers contagious? 2d. Can a fever strictly contagious be generated by an accumulation of filth, or of putrefying or putrid matters, or by the crowding of healthy persons into confined, ill-ventilated, and unclean places?

The affirmative to the first question was maintained by Cleghorn, Clarke, Fordyce, &c., even with regard to intermittents, "although," writes Fordyce, "these are not nearly so apt to produce the contagious matter, or at least to propagate it as continued fevers." To combat this assertion, Bancroft cites several instances of severe marsh fevers, not being communicated to the attendants, and he therefore decides in the negative.

Entering on the second question, he starts with this definite and precise opinion:

> "Everything which I have been able to discover or to ascertain respecting the nature and properties of contagion induces me to consider each of its several species as a peculiar and morbid quality or power, imparted to certain animal secretions, in consequence of some particular, though unknown actions excited in the living body when actually disordered, by the very same species of contagion previously in like manner elaborated in another body whilst labouring under a similar disorder from a similar cause. (Op. cit. p. 104) . . . . . . Contagion is therefore a natural though a morbid and specific secretion, and wholly inimitable either by nature or art." (p. 104.)

He believes it to be wholly impossible that any co-operation of circumstances can ever generate a new contagion.

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* Official Report on the Bann, p. 26.

† An Essay on the Disease called Yellow Fever, &c. By E. N. Bancroft, M.D. London, 1811.

> "Such matters spontaneously decomposing and returning to their natural inorganic and harmless combinations necessarily obey their respective chemical attractions, and the products resulting from this sort of obedience are as certain and constant as the formation of sea-salt by combining soda with muriatic acid. There is no chance, therefore, nor even possibility, of their generating anything so wonderful and so immutable as contagion, which, resembling animals and vegetables in the faculty of propagating itself, must, like them, have been the original work of our common Creator, and must have been continued in existence by the energies of a living principle, through which it has been transmitted from generation to generation." (Op. cit. p. 109.)

He afterwards compares this doctrine of the production of contagion to that of "equivocal generation," and concludes that one is as unphilosophical as the other. Then, passing from general reasoning to facts, he proceeds to adduce numerous instances where the apparent conflux of all the necessary conditions for the generation of contagion has been unattended with such a result, and sums up as follows:

> "From the preceding facts and considerations, I think it may be safely inferred that filth, crowding, putrid human effluvia, and deficient ventilation, though favourable to the retention and accumulation of febrile contagion, where typhus fever exists, or has existed, do not of themselves generate, or enable the human body to generate that contagion." (Ibid. p. 156.)

Part III follows, on the causes of yellow fever. He starts with the assertion that certain soils acted on by the sun produce certain vapours called marsh miasmata, but he expressly mentions, in several parts of his book, that by this term he does not intend to restrict the development of these vapours solely to marshes. He then adduces proofs of his assertion, and refers to Lancisi, Pringle, Lind, Clarke, &c., for others. The susceptibility of the body for marsh miasmata is then considered, and great influence is attributed to temperature. The yellow fever of new comers is referred to these two causes. (Ibid. p. 276.)

The next clause of the argument follows in Part IV, which is an admirable and laborious exposition of the relation of marsh fevers to each other, and of the identity or affinity which they bear to the yellow fever. The identity of the whole range, however different in type, is considered to be proved. We might arrange the arguments of the whole work in somewhat of a syllogistic form.

1. Fevers derived from marshes (vide Part III) are not contagious, and cannot by any possibility become so, for a contagious virus is generated only by a reproduction of itself. (Vide Part II.)
2. Yellow fevers are identical with marsh fevers, and arise when a high temperature is added to the febrile miasma. (Vide Part IV.)
3. Therefore yellow fevers are not and cannot be contagious.

Having arrived at this conclusion, Bancroft enters, in the Appendix, into the examination of evidence, more particularly as to the alleged introduction of the pestilence into Grenada in 1793, by the Hankey, from Boullama. The evidence of this and other cases being shown to be very inaccurate, he finds no difficulty in denying, even on this ground, the contagion of any form of yellow fever.

No one will deny the power or the philosophy of this work of Dr. Bancroft. We are of opinion, indeed, that it has never been answered.

It was, in fact, impossible that it should be answered by any of the thorough-going contagionists, because they could not dispute the fundamental arguments against the production of a virus from the usual alleged sources of deficient ventilation, crowding, putrid effluvia, &c. And in 1811, if this first position was granted, then we hold that Dr. Bancroft had by far the best of the argument, and that his conclusions were inevitable. We do not think that justice has of late years been done to Bancroft by the contagionists. Although, at the present day, we do not agree with him, because Time, the great solver of difficulties, has given us additional facts to work with, still we regard his work with admiration, as containing a profound and subtle argument, wisely conceived, and laboriously and admirably executed. We have said that Time has given us new facts; we refer more particularly to the cases of the Bann and the Eclair, in which we find it impossible to deny contagion. This admission reduces us to an alternative; either we must question Bancroft's fundamental position of the non-generation of a contagion *de novo*, or, admitting this, we must give in our allegiance to the contagionists, and admit the existence of a specific yellow fever.

"There are few things," says Bancroft himself, "more obscure or fallacious than the subject of febrile contagion." (Ibid. p. 488.)

> "I know well," writes Tommasini, "how great is the difficulty, not to say the impossibility, in which the prudent observer is placed, who seeks to resolve by facts these questions so multiplied and subtle. I do not deny that this rigour of analysis appertains more to theoretical pathology than to practical medicine. But it is not less true that, without these researches and the distinctions which I have indicated, observations and facts will become sterile in our hands, and we shall never pass the point where now we find ourselves touching the knowledge of epidemic influences." (Op. cit. p. 337.)

At this point the question of the contagion of yellow fever merges itself in the comprehensive subject of contagion generally. Of all the more undefined contagions, such as of typhus, erysipelas, plague, and cholera, the inquiry may be made as to the possibility of their generation *de novo*. Even contagions of comparative simplicity, such as cowpox, rabies, and glanders, are subject to the same question. And although the characteristic and undoubted contagious properties of smallpox, measles, or scarlatina, and the well-defined and pathognomonic symptoms which mark their action, forbid apparently the application of the inquiry to them, still we can only avoid this by assumptions almost as great as that of origin *de novo*. But into this great question we shall not presume to enter; we will refer only to some familiar facts which seem to be of importance in the discussion.

We may remark, *in limine*, that there is nothing unphilosophical in the opinion that a certain accidental union of circumstances may develope a particular chemical agent; and Dr. Bancroft and others of his opinion willingly admit this in the case of miasmata from soils; but they lay great stress on the fact that these do not multiply themselves. Are we, however, in the present state of our knowledge, entitled to draw this marked distinction? As we judge of these agents only by their effects, do we feel certain that they may not be all reproductive or self-generating, only in degrees of infinite variety? For we know that undoubted contagions are sometimes excited to unusual activity, or are at other times nearly deprived of reproductive powers, simply by variations in the conditions of the media which surround them. And to draw our illustration from the cases before us, the believers in the contagion of yellow fever have always stated that the contagion varied remarkably in its power of development. Thus Gillespie describes a fever in the Majestic man-of-war, which he calls "causus;" it presented all the symptoms of yellow fever, black vomit, yellow suffusion of eyes and skin, &c. &c., and on board appeared decidedly contagious, yet when the sick were landed at Gros Islet it appeared not to be contagious. (Op. cit. p. 49.) Sir William Pym states that "the contagious powers of yellow fever are destroyed by a circulation of moderately cool air."* The epidemic of 1793 in Philadelphia was checked suddenly by rain and cold weather. In 1810, at Gibraltar, the disease was arrested by a cold north wind.

It is from this peculiarity of facile destruction that M. Rochoux draws his distinction of contagion and infection; the former term being applied to those agents which are capable of multiplying themselves by reason of an inherent organism, and the latter to agents which require for their development certain additional conditions, such as contaminated and impure air, &c. The propriety of such a distinction is very questionable. Is there any contagious agent which does not require certain accessories? —they all demand a predisposition of body,—they all require certain conditions of the atmosphere. Smallpox is hardly communicable in a very dry and cold air; it is developed rapidly in a moist and warm atmosphere. Yellow fever is arrested by cold; plague by cold and dryness as well as by intense heat and dryness. Cholera has been arrested, both by great dryness and great humidity of the atmosphere, &c. &c. We find it assumed as a fact by several of the older writers, and even some of the present day, that certain contagions are propagated only in a "corrupted atmosphere." Thus Mead believed the plague to be contagious only in such circumstances, and Hosack, in his later works, makes the same observation of plague, dysentery, and yellow fever. But it is quite obvious that this is merely another mode of stating that one contagion is more readily reproduced than another; the conditions of existence and propagation vary, but this is no valid ground of distinction, it is merely a question of degree. No one could deny the contagion of smallpox or cowpox, because in some places they cannot be inoculated. Few will now deny that epidemic plague is contagious, yet there seems no doubt that sporadic plague is not so; and the difference is simply an artificial one, created by ourselves, because in both instances the poison is the same, and is self-reproductive, but in one instance only is this sufficiently intense to be appreciated by us.

The old expression, "epidemic constitution," merely expresses that a poison meets with such abundance of the accessory or assisting conditions that it propagates itself with unusual rapidity. That these conditions are occult must be conceded, but can they be denied? A remarkable instance of the rapid development of a very specific contagion occurs to us in illustration; Hilary† described rabies as common in the West Indies;

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* Medical Repository, vol. vi, p. 209.

† Observations on the Changes of the Air, &c., in the Island of Barbadoes, p. 245.

Mosely, having never seen it for a series of years, was led to doubt the correctness of the observation; but in 1783 it broke out with "great violence" at Hispaniola, and in the month of June at Jamaica, where it raged till March, 1780. "Dogs were seized with it that had no communication with others, and some dogs not brought on shore went mad on their arrival in the harbours of the island."* Hooping-cough had not been seen in Barbadoes by Hillary for many years; suddenly one year it broke out, "but by the strictest inquiry," says Hillary, "I could not make out that any child or elder person had brought it hither."† And numerous well known instances of the rapid development and spread of smallpox and plague will occur to every one. Now, if at different times there is this remarkable difference in the power of an agent to generate itself, so that at some times it shall appear, as in the case of sporadic plague, to be absolutely non-contagious, may it not be questioned whether the broad line of distinction drawn between such agents and others which appear at all times incommunicable, if such can be found, has not arisen rather from an attempt to limit and define, that which really is illimitable and undefinable, than from any obvious and incontestable division drawn by the hand of Nature. And in support of this view, we may refer to the remarkable fact that there is no agent or poison evolved from any source which has not at times exhibited such strong signs of reproduction, as to have led observing men to consider it contagious. Influenza does not spread by contagion as a general rule, yet there are not wanting examples which argue the possession of this property. Erysipelas is in all probability at times contagious. Asiatic cholera belongs to the same class; and, as the very subject we are now discussing proves, marsh miasmata have been presumed to be contagious when the subjects exposed to their influence were placed under certain circumstances. If, therefore, we admit for the moment that this is really the case, we have only two suppositions: either that such marsh miasmata are really and at all times self-reproducing, only in a degree quite inappreciable unless under extraordinary conditions (as was indeed the opinion of Fordyce, Lind, and others): or that under such conditions the marsh miasmata generate a second and different agent. And this leads us at once to Dr. Bancroft's second argument, as to the possibility of this conversion of one agent into another, and the production of a specific contagion. We may remark here, incidentally, that it appears to us, had Dr. Bancroft lived to the present day, and witnessed those cases of the Eclair and the Bann, he must, if he had remained consistent with the great principles of contagion laid down by him, have abandoned altogether his conclusions respecting yellow fever, and ranged himself under the banners of Sir William Pym. We can really see no alternative; between these two apparent extremes of non-contagion and of sempiternal contagion, there is in reality more analogy and agreement, than exists between either of them and the doctrine of occasional contagion, or the conversion of malarious into contagious yellow fever.

But to return to our argument. The first objection which arises to Dr. Bancroft's course of reasoning, as well as to the opinions of many others since his time, is, that the case has generally been to some extent prejudged. Dr. Bancroft convinced himself that in the cases of the more definite contagions there is no production without the presence of a similar substance, and applies this to all reproducing viri. This presumption has, perhaps unconsciously, biassed the whole subsequent argument; the contagion of yellow fever has been judged by the rules deduced from smallpox, and the observations made upon it have been compelled violently to conform with an inapplicable standard. Inapplicable, because in the present state of our knowledge we are bound only to draw deductions from facts, and not to argue from analogy. And it has certainly not been demonstrated that because smallpox never arises *de novo*, and cannot be generated by filth, human effluvia, &c., other contagions may not be so formed. "Variety," says Sir Gilbert Blane, who was a believer both in the contagion of yellow fever and in the origin of this and other fevers from vitiated human effluvia, "is as characteristic a feature as uniformity, particularly in all that relates to organic beings and animal life. Mere analogy can only be held as presumptive evidence, and may serve as a fair ground for rational suggestion and conjecture, but must ever stand subordinate to facts and observations. It is little better than gratuitous assumption, therefore, to say that every infection must conform itself to that of smallpox, and the history of other infections militates against such an assumption."

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* On Tropical Diseases, &c. By B. Mosely, M.D. London, 1803. P. 48.

† Ibid., p. 46.

Secondly. Dr. Bancroft and those who profess the same opinions have attached great weight to negative evidence. A great part of Dr. Bancroft's "second part" is occupied with examples of cases, where apparently all the conditions have been present which are commonly supposed capable of generating typhus, and yet this disease has *not* originated. But we doubt the value of this negative evidence, when we are confessedly in ignorance of the conditions necessary for the generation. And we doubt the applicability of an argument drawn from a Russian prison to the case of a tropical fever, or one from a Parisian abattoir or graveyard to a slave-ship with the hatches down.

Thirdly. It is possible to bring forward very strong positive evidence of the occasional generation of a poison. From the general history of epidemics, this generation may be reasonably inferred. And although we allow that the facility with which the older writers admitted the possibility of the generation of contagion has destroyed much of the value of their evidence, we find in this very facility itself an argument for the truth of the doctrine, as there is generally some substratum or groundwork of truth in an opinion which claims, or has claimed, ancient origin and general belief. We will therefore surrender the case of the Black Assize recorded by old Anthony Wood, or the one related by Holinshed, or the story of the Décade frigate, or of the black hole at Calcutta;* but this grand fact we will not surrender, that whenever a country has furnished the necessary conditions of deficient and impure food and human effluvia from the habits of the inhabitants, or from their accidental collection into masses, then, as a matter of course, an epidemic and often a contagious disease has followed. When the "vile landsknechte" landed with Richmond at Milford Haven, the sweating sickness accompanied their disorderly and licentious march. The fatal and revolting lues which, at the termination of the fifteenth century, spread, as Hecker remarks, "like lightning over Europe," was developed by the depraved and mercenary army of Charles VIII. And even the regular and unequalled discipline of modern times has failed to alter the inevitable laws of nature, and in the petechial and malignant fevers which followed in the track of Napoleon's armies we recognise similar causes marked by similar results.

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* It was unnecessary trouble to argue against the generation of fever in this case. No one knew better than Dr. Bancroft that, in the tropics, the English typhus is almost unknown; and, besides, a single night's crowding could never be supposed capable of elaborating a contagious poison.

When we examine the histories of epidemics, and discover how numerous are the forms of disease of greater or less virulence and extent of spread, which during the last three centuries have appeared in Europe, and the ways in which these diseases have been combined and have followed each other,* is it not more likely that each of them was generated anew from a peculiar combination and mutual influence of human events and atmospheric vicissitudes, than that the poisons of each affection should have remained diffused and quiescent in the atmosphere, until in some fortuitous arrangement of events they found their conditions of development ?† The strongest arguments for the spontaneous generation of poisons, in contradistinction to the doctrine of reproduction from similar preexisting poisons, are to be found in the sad records of these fatal plagues. But we feel it necessary to leave untouched this extensive subject, and to pass on with this brief allusion to the argument which can be drawn from it.

Without reference to epidemics, it is possible to bring forward very strong positive evidence of the occasional generation of a poison.‡ Thus

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* For example, as Dr. Hancock has so clearly shown, how remarkable is the prevalence of a malignant fever before and after an epidemic of plague, and how clearly the disappearance of other diseases before a great pestilence proves the condition of some "epidemic constitution" of the atmosphere, as yet unrecognised by us.

† For this supposition becomes necessary if the other be disallowed, as has been clearly shown by Dr. Robert Williams, in his remarks on the typhoid poison. He believes this poison "exists at all times diffused through the atmosphere of certain countries." This proposition he thinks is in fact proved, because, "if it be unphilosophical to admit the agency of two causes in explanation of the same phenomena, the theory of a spontaneous generation of the poison, however plausible, is negatived." (Elements of Med., vol. i, p. 33.)

‡ To one epidemic of recent times we must make a short allusion. We refer to the plague or epidemic fever with lymphatic swellings which arose in Rajpootana, in India, in 1836, and which has become known by the name of the Pali Plague. It commenced on this occasion at a small village called Taiwali, near Pali, in Marwar. Its importation could not be proved, or rather was completely disproved; but there was abundant evidence of its being possessed of infectious properties. But the fact we wish at present to insist upon is this, that the contagious property was apparently originally produced by certain causes, which, being removed, the poison ceased to be contagious; that is, its reproduction was reduced to a degree inappreciable by us. For evidence of this we refer to the several works on this subject, particularly to Mr. Ranken's Report. A most interesting point about this disease is its relation to Levant plague. The people among whom it originated, the Rajpoots, are among the most dissolute and abandoned in India; the lower classes are in the highest degree licentious and improvident. Under the protection which the English rule affords to life and liberty, population has increased faster than the means of support. The miserable hut, which was the den of four persons fifty years ago, now shelters, says Mr. Ranken, "six human beings within a space barely sufficient for the accommodation of four." Their houses are surrounded by thick, prickly bushes and cactus, both to seclude their women and as a means of defence. The English officers who have penetrated into their villages, describe in terms of unmitigated disgust the filth and misery prevailing everywhere. Bullocks, dogs, and horses live in the inclosures and houses, and the stench from the collected effluvia and ordure of man and beast is almost insupportable. The personal habits of the poorer classes are filthy in the extreme—the ablution and bathing so freely practised among the other nations of India are unknown here—the dirt is ingrained in their skins; they wear great quantities of clothes and seldom change them: men have been seen in Kuttiwar whose clothes

Professor Coleman, of the Veterinary College, mentions that, in the expedition to Quiberon in 1795, some transports crowded with horses had their hatches shut for a considerable time in consequence of a storm; several of the horses were suffocated, and shortly afterwards glanders appeared among the remainder. No contagion is more specific than glanders, and it is almost as easy to suppose smallpox to be generated as this disease. The same observer states that during the American war sheep were sent from England to America, but in a few weeks a febrile disorder broke out, and they all died. A typhus fever broke out on board the Diamond man-of-war while on a cruise in the West Indies, at a time "when none of the circumstances were present which commonly produce that disease."* In the case of hospital gangrene, particularly when occurring after campaigns in hot countries, the development of a contagious virus can hardly be disputed. Erysipelas furnishes another example of a disease generally non-contagious being able under certain conditions to develop a contagious poison; at least there is a strong presumption of the fact, and the evidence in its favour since the paper of Dr. Wells† has been gradually accumulating.

The well-known case of the fever on board the Bahama in 1814 seems to us tolerably conclusive of the generation of infecting typhoid fever. The Bahama was fitted up at Chatham in 1811 for the reception of prisoners of war. In the winter of 1813-14 the number of prisoners was very great; the weather being very severe they took every means of excluding the external air; they were very dirty in their habits, and had a great quantity of dirty clothing. In February 1814 fever broke out, and assumed an infectious character, as was proved by some cases carrying it into the Fyen, a temporary hospital ship, and communicating it to her crew. Sir W. Burnett concludes that this fever originated on board, "became concentrated from the total want of ventilation, and that it afterwards assumed an infectious form."‡

But in order to adhere to our own rules, and not to draw conclusions concerning the yellow fever from poisons which may be governed by very dissimilar laws, we will select a few illustrations from the history of this fever which seem to argue the generation of its efficient cause. We have already cited some instances of this kind from Gillespie. The case of the Kent man-of-war seems a tolerably certain one. On the 1st of July the Kent left Port Mahon with convoy, her crew were in good health, and Port Mahon was healthy. On the 6th of July fever appeared, which was called by the surgeon "causus icterodes." It appears to have originated from the unhealthy anchorage selected at Port Mahon, but to have acquired contagious properties during the voyage; for when the sick were shifted into transports, the crews of

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had positively rotted away without having been once taken off. In this country, as might have been anticipated, malignant fevers and plague have prevailed from a remote period. It would be an important point, if it could be done minutely, to inquire into the similarity of the habits and customs of the people of Egypt as compared with those in Rajpootana. Judging from published accounts, the diseases appear similar, and identical causes seem to be in operation. The removal of these causes in both instances is the only way of arresting the disease. To employ quarantines in the case of Rajpootana would be to use the antiquated expedient of an ignorant age.

* Sir G. Blane, op. cit. p. 216.

† Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. ii.

‡ An Account of a Contagious Fever which occurred among the Danish and American Prisoners of War at Chatham in 1813 and 1814. By Sir W. Burnett. London, 1831.

these transports were attacked with the same fever. The crews of the other vessels not brought in contact with the sick were not attacked. The contagion in this case is admitted by Sir William Burnett.* The instance of the Scout sloop-of-war may be quoted; she acquired fever at Port Royal, evidently the common ardent or inflammatory remittent fever. To get rid of this she left for the Havannah, but instead of the cruise checking the disease, as it usually does, the fever increased to an unprecedented degree; black vomit was common, and in several cases there were buboes as in plague. Under these disastrous circumstances the Scout, being unable, from her distressed condition, to return to Port Royal, made for New Providence, and landed her sick on Hog Island. The rains had just commenced in this place, and the type of the disease in many cases became changed, and terminated in intermittents. The proofs of contagion were unequivocal. Sir William Burnett does not mention them in his Report, bnt says, "The instances of contagion mentioned by the surgeon appear to me so unequivocal that it is altogether unnecessary to say a word on this head."† The late Dr. James Johnson, also, who was no prejudiced witness in favour of contagion, perused these documents, and was convinced of the presence of a reproductive poison.

An instance of a similar kind is to be found in one of the early writers on this subject. Mr. Arthy‡ describes a ship leaving Jamaica on the 5th of June, 1796, and having an epidemic yellow fever passing through the ship's company, appearing seven days after leaving the land, and gradually increasing. He evidently attributed this to contagion, as he makes the following remarks in reference to this and similar cases:

> "For it sometimes happens, through the greatness of the number of seamen sick of the yellow fever on board a ship at one time, and through the unusual malignancy of the fever at particular periods, that it spreads by contagion to those seamen who have been constantly on board and not in the least exposed to the common causes of yellow fever, so that the fever runs completely through the crew, and many of them die of relapses as well as of first attacks." (Op. cit. p. 232.)

That the fever on board these ships was of malarious origin can hardly be doubted; that it became contagious appears both from the evidence adduced by the medical observers, and also by the peculiarity of its course and spread. As a general rule, when a ship leaves an unhealthy anchorage on account of fever, a few days' sail eradicates the disease; at the end of the usual incubative period, from ten to twenty days, fresh cases cease to appear. But in other cases, from some peculiarity not yet known, the fever spreads through the ship's company, perhaps extending gradually from one part to another, as in the case of the Bann, and manifesting, as in the case of the Scout or the Kent, decided contagious properties. In fact, to explain this extension, it is almost necessary to admit contagion; the crew are no longer under the influence of the endemic source of the disease, the hold is clean, and yet the fever spreads. From this we might infer contagion, and this inference is strengthened when we find that instances of contagion are in these cases almost universally related by the medical officers.[*]

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* Report on the Bann, p. 32.

† Op. cit. p. 44.

‡ The Seaman's Advocate. By Elliot Arthy. London, 1798. p. 194.

These and other similar cases are sufficient to prove that the doctrine of convertibility is not destitute of plausibility. Its assumption is in fact, in many cases, almost necessary, and has been admitted in the cases of typhoid and yellow fevers by men of the highest eminence. Humboldt, from personal observation, satisfied himself that yellow fever was not contagious at Vera Cruz. "It is incontestable," he writes, "that the black vomit is not contagious at Vera Cruz. On the continent of equinoctial America the yellow fever is no more contagious than the agues of Europe." But Humboldt believed the "vomito" to have been infectious in Andalusia,[†] and, in reference to this point, remarks: "It is not contrary to the analogy presented by other pathological phenomena that a disease which is not essentially contagious may, under a certain influence of climate and seasons, and by the accumulation of patients, and by individual disposition, assume a contagious character." Rochoux[‡] admits both the spontaneous development of contagions with germ, and contagions without germ, or of which the germ is easily destroyed, and which latter form the maladies called pestilential.

Dr. Tweedie, in his admirable article on Fever, in the 'Cyclopædia of Medicine,' writes thus:

> "The human body, not only when affected with disease, but under certain circumstances in a state of health, generates a poison which gives rise to fever. This principle, notwithstanding the reasoning of ingenious disputants, has been incontrovertibly established by a multitude of facts." (Cyclop. of Med. vol. ii, p. 192.)

Dr. Hennen remarks, in reference to the subject of yellow fever:

> "Surely there are few practical physicians nowadays, notwithstanding Dr. Bancroft's dogmas, who doubt that diseases, not originally capable of propagating themselves, may acquire that property by crowding many sick in filthy and ill-ventilated habitations under a tropical heat." (Medical Topography of Gibraltar, &c., by John Hennen, M.D. F.R.S.E. Lond. 1830, p. 108.)

Dr. Copland has, in spite of contradictory passages, admitted the generation of a febrile contagion in the following terms:

> "The disease produced by infectious or contagious agents may be modified or aggravated by superadded or consecutive causes; this is especially the case with those febrile maladies which arise from *endemic sources* and from animal effluvia; the emanations from the sick of these maladies, if allowed to accumulate round the patient, particularly when several are confined in ill-ventilated places, will aggravate the disease, impart to it new characters, and an infectious atmosphere may be thus generated capable of producing a modified, or even a different, but generally *a much more malignant, malady*, than that which originally existed." (Vol. ii, p. 235.)

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[*] Dr. Wilson's ingenious attempt to explain the occurrence of yellow fever on shipboard, by supposing a more or less rapid decomposition of the timbers, is not applicable to these cases, in which the fever prevails only for a limited time. We observe that this hypothesis finds little favour either from Dr. Copland or Dr. Bryson. The latter gentleman attributes more influence to thickly-stowed green firewood (Climate and Diseases of Africa, p. 220) than to any alteration of a ship's timbers.

[†] Humboldt founded his belief on the Report of the French Commissioners, but no one can doubt that, had he perused the answers and refutations by Rochoux, Chervin, and others, he would have placed less faith on so unsatisfactory a document.

[‡] Dict. de Medecine, art. Contagion, p. 504.

If, in the instance of yellow fever, it is still denied that a marsh yellow fever may assume contagious properties, we are unavoidably driven to the opinion that on board the Kent, the Scout, the Bann, and the Eclair, two diseases must have been prevalent at the same time. But this hypothesis seems to us in the highest degree improbable. How and when did the Kent or the Scout become affected with the contagious fever? Not from Port Mahon or from Port Royal, for it did not exist in those places. It is incumbent on the supporters of the hypothesis to demonstrate the channel of introduction. In the case of the Bann this was attempted, but certainly without success. Sir William Burnett has, we think, satisfactorily shown that the supposed derivation from the timber ship Caroline, was erroneous; but if this be admitted, how did it originate on board this ship? Sir William Pym has, with perfect consistency, declared that the Eclair must have had this double disease on board. "It appears evident," he says, "from the history of the Eclair, that she had both diseases on board at different and distinct periods; viz. the marsh or river, during the months of April, May, and June; and the yellow or Bulam fever, with black vomit, from the 23d of July to the time of her arrival in England." This is certainly precise enough. On the 23d of July the crew of the Eclair began to suffer from a new and more dreadful disease than any they had yet experienced. Happily of this new danger all parties seemed ignorant. It announced itself by no unusual malignancy of symptoms; on the contrary, it assumed characters which Sir William Pym will consider very extraordinary—it was distinctly remittent. (Correspondence, p. 87.) It was in no respect different in the eyes of the medical officers of the ship from the fever which had proved so fatal when they were at anchor off the coast. Even when they arrived at Boà Vista, they thought it still the common African fever. Whence then does Sir William Pym derive the contagion, and why does he select this particular date of the 23d of July? This was the day on which the Eclair left Sierra Leone, and there embarked in the Albert[*] a merchant residing in that place, who fell a victim to the fever on the 27th of July. Sir William Pym, although he does not formally state it, evidently considers that this gentleman, Mr. Dawson, introduced the true contagious fever on board. But of this there is not the slightest evidence; he is nowhere said to have been sick, nor do we find any evidence that he was the first person who died, as Sir William Pym states.[†] Captain Estcourt merely states that, before reaching Bathurst, they had lost 7 men from fever, besides a passenger from Sierra Leone, to whom he had given a passage.[‡] In the medical Report compiled by deputy-inspector Stewart, it is said that, "after leaving Sierra Leone, 3 other men who slept on shore were seized with fever in July, and died. A merchant, who embarked on board the Albert at Sierra Leone, was also taken ill and died on board of that vessel on the 27th." This importation at Sierra Leone appears to us an assumption altogether unsupported by evidence in the documents before us; and it is not satisfactory to be coolly informed by Dr. Copland, that "on this point it is impossible for Sir William Pym to have been mistaken, seeing that his experience of this distemper is greater than that of any other physician whatever."[§]

We may conclude, then, in reference to these several points now under discussion, that the spontaneous generation under certain circumstances is rendered probable, by the occurrence of similar development of other morbid poisons, by certain cases recorded by writers on yellow fever, and by the impossibility, in four well-marked instances of contagious yellow fever on board-ship, of tracing any other mode of origin than that of derivation from a malarious fever.

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[*] Which vessel was in tow of the Eclair.

[†] Letter to the Lords of the Council, p. 6.

[‡] Correspondence, p. 45.

[§] Op. cit. vol. iii, p. 173.

But we must now proceed a step farther than this, and inquire what light can be thrown on the contagious nature of yellow fever by its general history and by the study of particular epidemics. Here we find greater difficulty from the contradictory and conflicting evidence, and from the number of modifying circumstances. By selecting hitherto for examination only fevers occurring on shipboard we have avoided some of the most difficult points. On board-ship, a fever can only spread from three causes: from intercourse with the shore; from effluvia from the hold or timbers; or from contagion. The elements of the inquiry are simplified and easily appreciated. But in epidemics occurring in cities, the modifying circumstances, the effects of soils, emanations, the habits of different races, the influences of locality, &c. have to be taken into account, the discussion is complicated, and the truth proportionably obscured.

There are, however, certain preliminary points in the history of yellow fever which must be enumerated as having important bearings. 1. There is little or no doubt that epidemics of yellow fever have prevailed in the West Indies and in Spanish America from a very early date. It has even been supposed, from passages in the old Spanish historians, and particularly in the 'Historia General' of Oviedo, that the followers of Columbus were affected with the disease. Certain it is, that from 1647, when an epidemic is described by Ligon at Barbadoes, yellow fever has appeared in an epidemic form in cycles of years, from 15 to 30 in extent. Ulloa states that it was unknown at Carthagena till 1729, but this has been denied. It is curious enough how often this disease has been described by writers as a new distemper. Warren, who practised in Barbadoes in 1739, supposed it had never appeared till 1721. The epidemic of Barbadoes in 1691 was considered to be a fresh disease, and was called the "new distemper," although only 36 years before the English army in the conquest of Jamaica suffered greatly from it, and, as Echard says, "met an enemy more severe than the Spaniards in this plague." And even in the memory of the present generation, we have seen Chisholm, with an unaccountable blindness, describe the epidemic of 1793 in Grenada as a "nova pestis," a "monstrous compound which hitherto had not found a place in any nosological arrangement."*

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* The old term "maladie de Siam," applied to the yellow fever of the West Indies, arose, according to Desportes, from a fleet which arrived at Martinique from Siam towards the latter end of the seventeenth century, having lost men from the disease; long before this time, however, we have certain evidence of its prevalence in Jamaica and Barbadoes. Bancroft says (Essay, p. 320): "The Oriflamme, a French ship of war, arrived about 1686 at Martinique with some French people who had been driven from their settlements at Mergui and Bancok, in Siam, and the disease was supposed to be imported in her." Sir Wm. Pym appears to believe in this origin of the disease, as he states, in his work on the Bulam Fever, that the yellow fever of Batavia, described in Johnson's 'Tropical Climates,' might possibly have been derived from Siam. Of such an unlikely origin we need say nothing. It may not be amiss, however, to notice the kinds of fever prevalent in the English dominions nearest to Siam. The long strip of country ceded to the Company at the close of the Burmese war abuts on Siam; and Mergui, one of the reputed sources of the maladie de Siam, is now held by a small British force. The English province, part of the ancient monarchy of Pegu, has exactly the same characters as the adjacent Siamese district; immense forests and jungles cover a

In Europe, Rochoux has assigned strong reasons for believing that the yellow fever, or, as he calls it, the "typhus amaril," has been always prevalent, and was known long before the discovery of America.* He has even attempted to show, and with considerable plausibility, from monuments, inscriptions on coins, and descriptions by writers of the period, that the great plague of Barcelona of 1651, the most fatal since the "peste noire" of 1348, was a pestilence of yellow fever.

2. It is certain that between the periods of the occurrence of these epidemics sporadic cases have been seen from time to time in the places which they frequent. This is now universally admitted, and is an important point, as proving that the cause is still in existence, but, from want of proper conditions or of predisposed subjects, cannot be developed and act.

3. In all the places where yellow fever has been epidemic of late years, local causes have existed, which, in the opinion of many observers, have themselves been adequate to produce the disease. This will be admitted in the case of the West Indies; it is the case also in Spain, as is proved at once by the writings of Chervin, Rochoux, and O'Halloran. It has been said, however, that Gibraltar is an exception to this; that yellow fever never appeared before 1804, and that it has since prevailed on this barren rock, which is stated to be incapable of giving rise to anything like a febrile miasma. To those who hold such opinions, we recommend Hennen's 'Medical Topography of Gibraltar' (pp. 9-24, 44-53), where will be found details which can only excite surprise that Gibraltar has not been more visited by fevers. In 1799 also, a bilious remittent fever prevailed in Gibraltar, and is mentioned by Trotter (Medicina Nautica, vol. iii, p. 420), and for a hundred years previously the garrison had at times been severe

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mountainous and thinly peopled country; between the mountains stretch large plains, traversed by numerous rivers, the waters of which, loaded with debris from the neighbouring hills, form large shelving banks of mud and sand. The country is perfectly uncleared, except in the vicinity of the larger towns, round which a few rice-fields supply, in that luxuriant and teeming country, sufficient food for the few inhabitants they contain. A long and severe rainy season combines with a fertile soil and a burning sun to produce an almost unexampled developement of vegetation. The feeble efforts of the Burmans can hardly prevent their few scattered towns from being devoured by the encroaching forest. During certain months of the year exposure in these jungles is attended with the greatest danger. This is stated to be particularly the case at the end of the wet season, and when the ground is drying after the rains. But in the English stations, the troops, lodged in barracks, and little exposed, have enjoyed a very unexpected degree of health. The English houses, in imitation of the native, are raised from the ground to the height of from two to twelve feet. The fevers seen among the Europeans at the principal station, Moulmein, are—1, agues; 2, severe remittents, attended with very acute, rheumatic-like pains in limbs, back, and eyeballs, and accompanied with great abdominal uneasiness, constant vomiting, and perhaps diarrhœa or dysentery; 3, a congestive fever, similar, we presume, to the "peste froide" of the southern states of America, and comparatively infrequent; and 4, a very severe or malignant fever, exceedingly fatal, ushered in by great precordial oppression, accompanied almost always by a dark yellow suffusion of eyes and skin, often by a peculiar plum-coloured hue of the cheeks, and in the latter stages attended by black or lead-coloured stools, and in two cases out of eight by coffeeground-like vomiting. In some cases from the Andaman Islands, a country notorious for its insalubrity, the yellowness of the skin has been observed to be of a very deep shade, and in some instances the remarkable phenomenon has occurred of a light yellowness coming on during *convalescence*, a fact noticed by one or two of the old writers on yellow fever, but not stated, as far as we know, by recent observers. The type of these severe fevers is generally continued in the severest cases, remittent in the milder. In two cases the type has been noticed to change from continued to remittent, as the fever lessened in violence. This severe malignant-remittent fever is never attended by any unusual prevalence of agues. When it occurs in soldiers who have not been exposed in the jungles, it is generally at a time of the year when agues and mild remittents are least common. At Rangoon, in 1824-5, this fever was also seen, and black vomit occurred in several cases.

* Dissertation sur le Typhus Amaril ou Maladie de Barcelone, improprement appelée Fièvre Jaune. Par S. A. Rocheux, D.M.P. Paris, 1822. pp. 16-7.

sufferers from "dysentery and bilious fevers," diseases arguing the presence of malaria. It appears, also, that the rapid increase of the number of inhabitants and the necessary crowding of the houses had gradually augmented the sickness of the town in the first few years of the present century. "Gibraltar at all times," says Dr. Hennen, "possesses within itself many local causes of fevers, which, though not generally contagious, are capable of assuming the most malignant type; that these causes were in full and uncontrolled operation upon a densely-crowded and filthy population at the periods of epidemic fever is perfectly obvious." (Op. cit. p. 115.)

We must make another quotation from Dr. Hennen as to the state of the town during the epidemic of 1828. It is from one of his official letters:

> "In reference to my letter of this day's date," he writes, "I have minutely inspected district No. 24. It is with much regret I have to state to your Excellency that in almost every step I took in that district I had reason for surprise, not that the fever had broken out there, but that it had not extended farther; from whatever causes it may have proceeded, the pauper population is dense to a degree incredible except to those who have seen it. In sheds without ventilation, without drainage, and generally composed of the slightest materials, in tiers of beds, as close as in a crowded transport, numerous individuals sleep; they go out to their work at an early hour and return at gun-fire, locking up their miserable places of nocturnal shelter during the day, and leaving them saturated with the streams of their bedding, their food, and the overflowing receptacles of their ordure."

This is quite sufficient to prove to our readers that the opinion of the non-existence of the common causes of fever on the Gibraltar rock, however it originated, is erroneous.

These three preliminary observations are essential to the inquiry, viz.—1, that in the geographical limits of the yellow fever epidemics have been occasionally known for several centuries; 2, that in the intervals between the epidemic attacks cases constantly occur, and are called sporadic and endemic, testifying to the presence of the cause in a less active state; and, 3, that in all the places visited by epidemics there exist sources of contaminating effluvia, which may either be considered *per se* capable of generating the epidemic, or if this be doubted, which must be supposed to be necessary elements in the epidemic, by affording to an imported poison its conditions of existence.

In examining into the evidence of the contagion of epidemical yellow fever, it is necessary to admit that it will seldom be possible to trace the contagion in every instance. Some persons will be unusually predisposed, and will suffer from an exposure which is inappreciable; others will want the predisposition, and will not be able to develope the disease. But this much evidence we deem essential; the channel of introduction must be pointed out, and there must be recorded during the epidemic a sufficient number of undoubted cases of disease developed by intercourse with the sick.

The chief epidemics which have been the subjects of controversy, are the epidemic in Grenada in 1793; in Philadelphia in the same year; in Gibraltar in 1804, 1814, and 1828; and in different parts of Spain in 1814 and 1821. Now it must be confessed, that in the majority of these cases the channel of importation cannot be pointed out. In three particular cases this has, however, been attempted. The introduction into

Grenada was attributed by Chisholm to the arrival of the ship Hankey from Boullama on the coast of Africa. The examination of the evidence he adduced by Veitch, Trotter, Smith, Winterbottom, and Bancroft, showed, however, that the position was not tenable; there was not only a deficiency of proof, but Chisholm had unfortunately made several mistakes, which were unmercifully exposed by his adversaries. At the present day, with the case of the Eclair before us, we are not disposed to look with such contempt on Chisholm's opinions. He was unfortunate in his description, in his names, and in the general tone of his publication, but there can be no doubt of his sincerity. And when we simply read the case of the Hankey parting from the coast of Africa with a malignant fever on board, running down to Grenada, and there landing the remains of her diseased and wretched passengers and crew, it is impossible to forget that on board the Eclair a similar fever did really become contagious. A new and startling counterpart to the story of the Hankey has thus been given to us, which must afford more grounds for believing the truth of Chisholm's story than all the inaccurate evidence brought forward at the time. But even with this additional motive for crediting this much contested introduction, we do not think that the question is now capable of settlement, and this we presume to be Dr. Copland's opinion, for he does not even allude to the Hankey and the origin at Boullama. But whether or not the Hankey had anything to do with the introduction into Grenada, it is certain that an imported contagion had nothing to do with the disease in Dominica and Philadelphia. The epidemic in Dominica is described by Dr. J. Clarke, who is quoted by Dr. Copland as one of the best writers on "hæmagastric pestilence." Dr. Clarke says: "In June 1793 there was a prodigious influx of emigrants into the town of Roseau, in Dominica, from Martinique; the people were brought over in small vessels, exposed to the weather, and in want of all the necessaries of life. They were not sick on arrival, and this fever had not made its appearance in Martinique when they left it."* In a few days the fever broke out; it did not attack the emigrants first, but the English sailors in the ships in the harbour, then the emigrants, and then almost all new comers to the island, including soldiers, sailors, Europeans, and Negroes. "The people of colour from the other islands were also attacked, and also the new Negroes lately imported from the coast of Africa. With one exception, the Negroes who had been long on the island escaped."

It will be remarked how closely this agrees with Humboldt's observations.

About the end of November the fever had ceased altogether; "which was supposed to proceed from the comparative coolness of the weather" (ibid. p. 4); but about six weeks after we were convinced that this short respite was more owing to the want of proper subjects for the vitiated atmosphere to act upon, than to the change of its temperature;" for certain ships arriving, "all who had not been in the West Indies before were seized with it." (Ibid. p. 4.) This extraordinary cessation of the disease for six weeks, from the want of predisposed subjects, seems to us a very fair indication of some peculiar atmospheric condition, but a very unlikely circumstance to have occurred if the disease had been propagated from man to man. Speaking of contagion, Dr. Clarke says: "I have been

n the Yellow Fever, as it appeared in the Island of Dominica in the Years 1793-4-5-6. M.D., F.R.S.E. London, 1797. p. 1.

informed that it has been considered by some writers an imported and infectious disease, but in this island it did not appear to be either imported or contagious." (Ibid. p. 22.) The physicians and attendants were never attacked. "I believe the fever did never arrive at that contagious degree of accumulated impurity in this island, for when patients labouring under this fever were removed to high situations, for the sake of a cooler and purer air, and who notwithstanding fell victims to it, the people about them were never infected, nor did the disease ever prevail afterwards in such places." (Ibid. p. 64.)[*]

In July, 1793, the disease appeared at Philadelphia; the importation could not be proved, and it was, after much controversy, formally disallowed by the physicians and surgeons of that city. Dr. Rush, who at this time believed it to be contagious, admitted the domestic origin. It had not been epidemic in Philadelphia since 1762, but it had appeared at New York in 1791.

It is indeed evident that one of those mysterious cycles, the existence of which we admit, but do not understand, had commenced at this memorable time, and the prodigious influx of new comers into the harbours of the West Indian islands, from the arrival of the large squadrons and bodies of French emigrants, offered an immense number of susceptible subjects, and thereby augmented the pestilence to an incredible degree; but the proofs of contagion are, on the whole, deficient, and the opinion of the great majority of the witnesses is against this doctrine.

Leaving America and the West Indian islands, and returning to Europe, we find the epidemic in Barcelona in 1821, considered by Dr. Copland and others as one of the strongest examples of contagion on record. Dr. Copland adopts altogether the report of the French Commission, "who published, in 1823, a very detailed account of the pestilence, forming, perhaps, the best treatise extant upon it." And, when first perused, the facts as detailed by the Commission appear so plain, the conclusions so just and warranted, that one is tempted to agree with Dr. Copland. But when we read the examination of Chervin, who went over the ground after the Commission, we feel that this Report is absolutely worth nothing. Rochoux, who visited Andalusia at this time, speaks of it in terms of great contempt. To a translation of a Spanish "Manifest,"[†] to which are appended the names of one English, two French, and ten Spanish physicians, and which appeared before the Report of the Commission, Rochoux added some notes, although, as he says, the "Manifest" itself has anticipated "un assez grand nombre des assertions inexactes échappées à ces messieurs." But in the notes he proceeds farther than this, and demonstrates "évidemment la nullité médicale du travail de mes ex-collègues." Dr. Copland, however, takes not the slightest notice of these rival examinations, although we should have considered some allusion to their counter-statements almost necessary.

At first sight nothing can seem more satisfactory than Dr. Copland's condensed history of this importation. (See vol. iii, p. 162.) Ships arrived from the Havannah at Barcelona in June and July. Yellow fever was prevailing at the former place, and continued fatal on board the ships. After arriving at Barcelona, intercourse was allowed; the bedding of those who died was washed on shore, and the captain of the Grand Turk received on board his wife, children, and a servant; all these exposures were followed by disease, and the introduction into the town was traced easily from man to man. Such is the Report of the French Commission, made after the epidemic had declined. What, then, are the opposing statements of Chervin, Rochoux, &c.? They allege,—1st. That a fleet of merchantmen arrived from the Havannah, escorted by two men-of-war. 2d. That yellow fever did not prevail at the Havannah when they left, nor during the voyage. 3d. That the first cases at Barcelona occurred twenty-three days after the arrival of the fleet, and ninety after they had left the Havannah. 4th. That the two ships specially accused of importing the disease, viz. the Taille-Pierre and the Grand-Turc, had landed men at Carthagena and at Cadiz, at neither of which places did yellow fever afterwards appear. 5th. That the story of the captain's wife and family is more than doubtful. 6th. That four months before the arrival of the ships fevers had been prevalent, attended by "vomissement noir, ictère et autres symptomes alarmantes."[*] 7th. That local causes, "sufficient, with a high range of temperature, to have caused an epidemic,"[†] existed in great abundance, and that no persons suffered who were not exposed to the effluvia from the "foyers" of infection. 8th. That persons suffered who could not have held intercourse with the sick; for instance, a maniac in confinement and a lady confined to her room with chronic disease of the chest. (Rochoux, pp. 24-5.)

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[*] Dr. Chisholm asserted that yellow fever was introduced into Martinique in 1796. In the eighth volume of the Medico-Chirurgical Transactions this is completely disproved by Dr. Ferguson.

[†] Manifeste touchant l'Origine et la Propagation de la Maladie qui a regné à Barcelone en l'année 1821. Traduit de l'Espagnol par J. A. Rochoux. Paris, 1822.

Such, without descending into particulars, are a few of the prominent points discussed by the Commission and their opponents. Between the parties we cannot undertake to judge. We will only aver, that if the contagion of yellow fever rested solely on this presumed importation into Barcelona, no one could feel safe in founding his faith upon it.

The epidemic in Gibraltar in 1828, was the last which has excited much discussion. Voluminous documents attest the severity of the dispute. A French Commission, consisting of M. Trousseau, Louis, and Chervin, was acting also on this occasion, but with no certain results. A host of English army surgeons sent in Reports and answers to queries from Sir James M'Grigor, and Sir William Pym was himself there, prepared to seize every opportunity of illustrating his favourite views.

It could hardly have been supposed that some decided opinion would not have been arrived at on this occasion if the yellow fever had been violently contagious; but, on the contrary, the tide of opinion was, at the end of the dispute, certainly against the importation, and perhaps against the contagion of the disease. Dr. Gilchrest, who was in Gibraltar at the time, makes, in his admirable article in the 'Cyclopædia of Practical Medicine,' only a short allusion to this controversy, refraining, from motives of delicacy, from a detail of unpleasant circumstances. It was, however, completely proved to the satisfaction of the authorities in Gibraltar and of the Medical Board in England that the fever was not imported. Among other papers, Sir William Pym published some answers to queries from the Royal Medico-Chirurgical Society of Cadiz. These answers have been severely criticised, and we may refer our readers, among other documents, to a paper by Dr. Smith, in the 'Edinburgh Medical and Surgical Journal,' vol. xxxv, p. 12, in which is a searching examination of some of the disputed and erroneous statements.

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[*] This is, however, strenuously denied by the Commission.

[†] See also O'Halloran on this point. In the appendix to this work there is an attempt at disproving the supposed introduction of the yellow fever into the town of Zerez de la Frontera in 1820.

In the 35th volume of the 'Edinburgh Medical and Surgical Journal' is inserted a paper by Mr. Amiel; a name well known in the yellow-fever controversy. In this account, the foreign origin and contagion of the disease is altogether denied (p. 278); the first cases of the disease are said to have appeared scattered and unconnected at different points, and causes sufficient to account for the fever are stated to have existed in Gibraltar itself.

> "That Gibraltar has at all times possessed within itself many local causes of fever, capable of assuming the most malignant type, when exercising their full influence upon a densely-crowded and filthy population is perfectly obvious, and established beyond the possibility of cavil by the records of the Civil Hospital."

In the other epidemics, viz. of Cadiz and Medina Sidonia in 1801, of Malaga in 1804, of Gibraltar in 1804* and 1814, of Carthagena in 1811, the importation of the disease cannot be maintained.

The only question which arises here cannot very well receive a decisive answer; it is as to the value of this evidence as opposed to contagion. Bancroft laid great stress upon it; other non-contagionists have deemed the difficulty of proving importation of a contagion to be necessarily so great, that failure of proof constituted no valid argument against the reality of the disease possessing this property. And with this opinion we should at once agree, if the contagion of yellow fever could be proved by other ways; if it were, for instance, inoculable like smallpox; but, on the contrary, the alleged fact of importation is brought forward as one of the strong proofs of the contagion, and it is reasoning in a circle to argue the probability of this occurrence by insisting on the very quality which it is intended to prove. It is certainly remarkable how the complete and accurate proof of the introduction by the Eclair into Boà Vista contrasts with the loose and contradictory evidence of its importation into other places: but we must judge each case separately in order to draw a fair deduction from the whole.

While, however, we deny the proofs of the importation of yellow fever into these places, we are disposed to admit that it has in different instances manifested contagious properties. It seems hardly possible to deny this in the face of the evidence adduced by Sir James Fellowes, Palloni, Arejula, and others. These observers state facts which have come within the sphere of their own observations, and it is not in accordance with our rules to neglect any statement coming from a credible witness of a circumstance which has fallen within his own knowledge. But there is no doubt, from the innumerable instances to the contrary, that these cases of contagion are few and far between; that they are the exception and not the rule; and that, when manifested, the contagious power appeared more like an accidental addition to, than an inherent quality of, the morbid poison.

It is curious enough that Sir James Fellowes, who admitted the contagion of yellow fever so freely, yet observed facts which led him to assert that the conversion of an endemic into a contagious fever was necessary for their explanation. He seems to have gradually come to the conclusion that persons coming from Vera Cruz or the Havannah and, "carrying with them the seeds of disease admitted to be endemic in such places, might, during their passage in a crowded ship, undergo such a change of constitution as to produce the disorder, with the additional property of generating it in others highly predisposed."

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* We observe that Dr. Copland still repeats the story of the introduction, in 1804, into Gibraltar, by the man Santos, from Cadiz, which has long been disproved.

If this, however, were Sir James Fellowes' final conclusions, he cannot be ranked any longer among the supporters of a specific "Bulam Fever."

But we must not enlarge on the evidence of contagion in these epidemics; it presents few certain facts satisfactory to the mind, and the violence of the respective disputants has not tended to lighten the difficulty of the subject. We will pass on, then, to our conclusions—conclusions warranted, as we think, by the examination we have entered into, and by the line of argument we have adopted. At any rate, they are the honest results of an unbiassed inquiry, and of a sincere wish to arrive at truth and certainty. But we repeat an observation made at the commencement of our article, that in our opinion the time for a final decision has not yet arrived, and that we must wait for a more advanced science and a more perfect knowledge before we venture on an unqualified opinion in these difficult questions.

We draw at present the following conclusions from the whole course and tenor of the argument:

1. That yellow fever is decidedly contagious on some occasions.

2. That the proofs of the universality of this property are defective. Importation can seldom be proved; inoculation is impossible; and the disease requires a peculiar susceptibility of constitution in order to act.

3. That it is not safe to generalize from the first observation, and to conclude that yellow fever is at all times contagious because it is undoubtedly so sometimes, for these reasons:

a. Because there are fevers undistinguishable by symptoms from contagious yellow fever, which certainly are not contagious.

b. Because we are not sure how far contagion may not be an accidental property impressed on a poison by contingent circumstances, or may be only the development of a property of self-reproduction always possessed by the morbid poison, but generally in so slight a degree as to be inappreciable. If the former opinion be correct, yellow fever is both contagious and non-contagious; if the latter, it is also in the majority of cases non-contagious, in the conventional meaning of the term.

4. That contagion is only a property accidentally impressed on the yellow-fever poison appears probable.

a. Because in no other way can we explain the extraordinary discrepancies and opposing statements of men whose honesty of purpose is undoubted.

b. Because it is almost a necessary assumption, in order to explain certain facts in the history of yellow fever.

c. Because it is in accordance with analogous phenomena manifested by other morbid poisons.

d. Because there is really some direct proof of it in the apparent development of fever on board ships with clean holds, and removed from the influence of the land.

5. That if this conversion of a non-contagious into a contagious poison be denied, there is no alternative but to admit the existence of a specific contagious yellow fever.

6. That the doctrine of a specific contagious yellow fever is alone supported by the fact that it destroys the necessity of admitting the convertibility of poisons, a circumstance considered by many observers as in the highest degree unlikely and unphilosophical.

We do not think it possible to be at present more definite and decided. The whole question, indeed, turns on an undetermined point. It is necessary clearly to see the starting-ground of the argument, which is simply the possibility of the generation of a contagious poison. But, as we have already affirmed, this difficult point has been either entirely neglected in the argument, or hastily and inconsiderately settled. The errors of such an inherent and fundamental vice of premises have been sufficiently shown in the fierce controversies on the subject before us. Thus, while one party has been compelled to explain away all suspicious circumstances, to disregard undoubted occurrences, "to strain, suppress, and deny facts, and question the veracity of honourable men," because it hastily rejected the possibility of generation of contagion, the other party has been constrained to resort to false assumptions, and, by the most improbable and unlikely evidence, to detail the passage of a contagion from one part of the world to another, from one town to another, in all sorts of questionable ways.*

The extraordinary changes of opinion which very eminent men have manifested on the subject of the contagion of yellow fever, are among the most remarkable portions of its history.

The three French Commissioners, (Messrs. Bally, François, and Pariset,) changed their opinions several times. In 1804 and 1818, M. François thought it non-contagious. In 1821 he writes that, having treated this fever in the Antilles, he had strong reasons to believe it contagious, and without hesitation he admits the importation into Barcelona.

M. Bally was doubtful if contagion could be suspected in the terrible epidemic of St. Domingo in 1802; in 1804 he denied contagion altogether. In 1821 he admitted its presence without reservation.

Before 1820 M. Pariset doubted whether yellow fever was contagious in America: "It is not contagious in America, whether it has ever been, or whether it has ceased to be." (Bulletin des Sciences Médicales, tom. xii, p. 126.) A very short time afterwards he writes, "Yes, the disease which devastates Barcelona is the yellow fever of America; yes, it has been imported; yes, a thousand times yes, it is contagious." (Moniteur de 15 Nov. 1821.)*

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* This view of yellow fever has been adopted by many writers, Sir G. Blane among the number. Perhaps one of the most definite opinions is that given by Ceresa, who seems to have come to conclusions nearly identical with our own. "Il y a dix ans que j'annonçai la possibilité que la fièvre jaune fut de double nature." (Aperçu adressé à l'Académie de Médecine à Paris, sur la question si la Fiévre jaune est contagieuse ou non-contagieuse, par Ceresa. Vienne, 1829. p. 8.) He afterwards says that the words miasma and contagious matter "ont été trop souvent confondus par les médecins; mon sentiment est que l'on ne saurait exister sans l'autre, et que s'il existe entre eux simplement et dans leur nature double quelque différence, il y a toujours identité d'action." (p. 17.) He insists much on this identity of action (that is, the sum of the phenomena being the same), declaring that, without keeping this in mind, we can never arrive at a satisfactory conclusion: "Je serai parvenu à prouver que la fièvre jaune, de quelque nature qu'elle soit, pourra être contagieuse et non-contagieuse, et consequèmment, que l'on ne doit jamais négliger isolement, et qu'il serait très dangereux de supprimer les quarantaines." (p. 28.) Dr. M'William adopts also the opinion that the contagious fever of the Eclair arose from a conversion of an endemic marsh fever.

In America the contagion of the disease was, in 1793, generally admitted. In 1796 the importation was also admitted; gradually, after some sharp discussion, in which there were considerable modifications of opinion,† the general opinion seems to have completely changed, and in New York, in 1823, there were only three or four practitioners in the city who admitted the contagion of the disease. In 1825 Dr. Gilchrest states, on the authority of an American almanac, that "twenty-eight persons only throughout the whole country were in favour of the doctrine of contagion." (Cyclopædia of Practical Medicine, p. 289.) At the present time the contagion of the disease seems to be again generally admitted, as we shall have occasion presently to see.

The most interesting instance of change of opinion with which we are acquainted is that of Dr. Rush. This truly great and philosophical physician was, in 1793, inclined to believe in the contagion, though not in the importation of yellow fever; in fact, he derived the disease from malaria, and even in some degree adopted the opinion of Fordyce, that agues were, in the proper sense of the word, contagious. Afterwards he completely altered his opinions.

> "You will perceive," he says, in a letter to Dr. Miller (Med. Reposit. vol. vi, p. 147,) "from the facts and reasonings contained in this letter, that I have relinquished the opinion published in my account of the yellow fever in the years 1793, 94, 97, respecting its contagious nature. I am aware of the influence which such changes in medical opinions as I have acknowledged have upon a physician's reputation; but small indeed should I consider the total sacrifice of mine, could it avert the evils which are connected with the importation of pestilential diseases, and insure the benefits to the world which would necessarily flow from the establishment of the principles contained in this letter."

In the 'Medical and Physical Journal,' p. 85, he writes thus:

> "In the fourth volume the reader will find a retraction of the author's former opinions of the yellow fever spreading by contagion. He begs forgiveness of the friends of science and humanity, if the publication of that opinion has had any influence in increasing the mortality and misery attendant upon that disease. Indeed such is the pain he feels in recollecting that he ever entertained or propagated it, that it will long, and perhaps always, deprive him of the pleasure he might otherwise have derived from a review of his attempts to fulfil the public duties of his situation."‡

It appears to us that in these very changes and variations of opinion, we see an argument for supposing the yellow fever to be also of variable nature. If it be sometimes contagious and sometimes not, or if it be at all times contagious, but only very slightly so, unless it meets with concentrated effluvia to develope in, then we may explain the inconsistencies before referred to. It may be supposed that Rush was correct in both opinions he at different times professed, and that he need not have so bitterly repented of his early bias and his first-formed creed.

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* See also Examen Critique des prétendues preuves de contagion de la fièvre jaune observée en Espagne, par N. Chervin (Paris, 1829), where will be found a merciless exposé of the changes of opinion of several French writers.

† As, for example, in the case of Hosack, who started a decided contagionist, but who afterwards adopted opinions approaching to those we have advocated above; or of Beck, who, from a staunch contagionist, became an ultra non-contagionist.

‡ The noble abandonment of an error is, in our eyes, as creditable to humanity as the establishment of a truth; and the man who can sacrifice to the voice of conscience opinions long cherished and contended for, and can bear without shrinking the inuendoes and sarcasms of opponents, is worthy of the highest admiration. The truth and reality of Dr. Rush's conversion is proved by the whole tenour of his life and character, and by the internal evidence derived from his works. We therefore reject as utterly false and calumnious the atrocious declaration of M. Moreau de St. Mery (Dict. des Sciences Méd. tom. xv, p. 351), that Rush, on his death-bed, confessed that he had made the above declaration from interested motives, and that he had always continued to be really satisfied of the contagion of yellow fever.

We must now leave this portion of our subject, and proceed to a consideration of the necessity or expediency of quarantines as applied to this disease, and of the principles which should direct their practical application.

Quarantines. Are measures of seclusion and isolation useful in the case of Yellow Fever?

It is natural to suppose that in all ages, when men have witnessed a pestilential disease spreading by contagion, they have adopted some mode of arresting intercourse between the infected and the sound. But it is only since the doctrine of a specific virus being the cause of each contagious disease was promulgated by Fracastore, that the practice has been based upon a recognised and philosophical principle. This principle is, as we understand it, that every germ of morbid poison emitted from a diseased person has its term of development, its point of highest vigour, and its period of decline; its life and its extension are alike limited, and although capable of great modification from the nature of the surrounding media, yet the one can under no circumstances be prolonged beyond a certain time, nor the other be extended beyond a certain space. The modern quarantine laws, therefore, fix a certain arbitrary time as a period of purification, within which the vitality of the poison must be exhausted, and a certain arbitrary space, beyond which the germs, weakened by diffusion, do not spread, or do so harmlessly.

The great objection which has been pleaded against this practice is, as we have said, that its rules are *arbitrary;* they are not founded on observation, which has not yet fixed the length of life of a poisonous germ, nor the distance of its spread; one poison may live only a few hours, and spread only a few feet; another, more tenacious, may live many days, and spread for hundreds of yards; the quarantine laws regard each alike, and, with manifest injustice and detriment to all concerned, submit the ephemeral and the durable poison to a like treatment.

Another and more important objection is that the principle of these laws is partial; it regards only the poison; it forgets that the poison requires conditions, without which it dies, and becomes innocuous; it leaves to time and natural decay the extinction of a disease which science can, or at any rate soon will, effect at once.

Slowly, and step by step, we are acquiring some knowledge of these mysterious poisons; we recognise that each requires a peculiar condition; one demands a certain heat, another a certain degree of moisture; the typhus poison dies in the tropics, the yellow-fever poison in the arctic regions; the variolous poison will not live in a dry air, the choleraic virus in an atmosphere loaded with moisture. And if these distinctions are as yet too doubtful to be applied to practice, and too liable to fallacy to be quoted as immutable truths, we are yet certain of the grand fact that emanations and effluvia, both from animals and vegetables, are, as it were, the matrix or nidus of these poisons, and that the absence of one necessitates the death of the other.

Although with our present knowledge we regard quarantines as necessary safeguards, particularly as applied to poisons which are multiplied rapidly by the human body, or which develope themselves with difficulty apart from this organism, as smallpox or oriental plague, we conceive that the time must soon arrive when their use will be in great measure abandoned, except in certain countries, and during certain seasons.

Leaving, however, this general view of the subject, and keeping to the question as to the use of quarantines in the case of yellow fever only, we are to consider whether in our West Indian colonies or in America the entrance of this disease can by such means be arrested. Quarantines profess to keep out of a place a contagious disease. In the case of yellow fever, what is it they profess to keep out? Those who believe in a specific contagious yellow fever, will find no difficulty in answering the inquiry; we, who consider that an accidental combination of influences can generate this disease, find it no such easy matter. Yellow fever is not produced by a single and unique cause; its contagious form we have concluded to be only a variety. Are we to extend a rule applicable to a variety to the fever in all its forms? If the contagious variety is to be excluded by quarantines, can we always tell when the prevalent fever is of this type, or are all vessels which have lost men from yellow fever to be submitted to the same rules? The question is a simple one for this country; we need entertain no dread of yellow fever, unless, as in the case of the Eclair, the disease is actually prevalent at the date of arrival here; but in the instance of the West Indies and America the case is not so simple.

Even admitting that the contagious yellow fever could be always diagnosed, it remains to be seen whether the virus can be excluded by preventive measures; whether it be not too diffusible for the allotted space, or too durable for the allotted time. These questions can only be answered by experience, and it so happens that opinions as to the efficacy of quarantines are as much divided as those on the subject of the contagion of the disease. The Report presented by a select committee* to the House of Assembly of the State of New York is intended to prove the efficacy and the necessity of quarantines. The committee arrive at very decided conclusions. (p. 43.) They state that the yellow fever has been brought to the city of New York either by foul vessels, or by sick persons, or by damaged cargoes, or by the clothes of persons who had died of the yellow fever. Frost destroys the fever, and violent atmospheric commotions check it. The fever is not infectious, contagious, or epidemic in a perfectly pure atmosphere, unless it has been pent up in clothes.† The code of regulations founded upon these conclusions is sufficiently stringent. All vessels direct from any place where "yellow, bilious, malignant, or other pestilential or infectious fever existed at the time of their departure, and arriving between the last day of May and the first of October," are to remain in quarantine for at least thirty days after arrival. No vessel infected with yellow fever is to approach within 300 yards of the city of New York. Passengers arriving in vessels subject to quarantine are not permitted to leave quarantine, "until 15 days after the sailing of their vessel from her port of departure, 15 days after the last case of pestilential or infectious fever which shall have occurred on board, and 10 days after her arrival at quarantine." The committee have therefore fixed 30 days as the maximum period during which a ship or cargo can retain the germs of the yellow-fever virus, 10 or 15 days as the period in which those germs become manifested in the system, and 300 yards as the distance beyond which the virus becomes innocuous.

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* Messrs. Wheeler, Comstock, and Hine; these gentlemen do not appear to be medical men, but to have taken considerable pains to make themselves acquainted with the subject.

† We do not quite understand this. Is it meant that the virus, *because* it has been pent up in es, acquires the power of becoming epidemic in a "perfectly pure atmosphere."

Yet, strange to say, the facts brought forward in the Report do not warrant these conclusions, and the period of 30 days in particular appears, from the statements of several believers in contagion, to be very doubtful, and we may remark in this place that the evidence for contagion is not so unequivocal as Dr. Copland's remark at the conclusion of his article on " hæmagastric pestilence " would have led us to suspect.

The Report consists of three parts. The facts and conclusions arrived at by the committee; the act founded upon these, and various letters and affidavits from medical men and others, in answer to certain queries addressed to them by the committee.

The several epidemics which appeared in New York previous to the year 1795 are, the committee informs us, barely reported; and the little history we have of them is very unsatisfactory. Since 1795, yellow fever has occurred epidemically five times, viz. in 1798, 1803, 1805, 1819, and 1822. Since the latter date the city has been free from epidemic attacks, but sporadic cases are supposed to occur every year. (Report, p. 14.) In all these epidemics the origin of the disease was a matter of doubt and controversy; as far as we can judge from the statements of the committee, their facts are strongly opposed to the importation and even to the contagion of the disease. For proofs of this statement we refer to the letters of Dr. Bayley (p. 7), the Health Commissioners of 1798 (pp. 8, 9), Dr. Rogers (p. 10), &c. The committee have indeed arrived at a contrary opinion, but on what grounds we cannot understand.

The most valuable portion of the Report consists in the answers to interrogations by ten medical men. Of these ten, five give unequivocal opinions against contagion, viz. Messrs. Harris (p. 112), Manley (p. 165), Hort (p. 173), Reese (p. 211), and Roschenberger (pp. 219-23). One, Dr. Sweetser, considers it to be very slightly contagious, but " by no means like smallpox," and of domestic origin (pp. 155-8); and another, Dr. Wallace, considers it to be imported, " but not communicable by contact." Three decide the disease to be contagious; of these three, one, Dr. Vaché, actually furnishes evidence against the time which the committee considers sufficient for purification (see from page 79 to 94); another, Dr. Francis, states his opinion without a single confirmatory fact; and the third, Dr. Townsend, a well-known name in the yellow-fever controversy, is a follower of Chisholm and Pym.

If the facts detailed in the Report do not bear out the positive conclusions professed to be founded upon them, still less does the history of quarantines in New York indicate that any benefit has been derived from them.

> " The state of New York, as early as 1784, enacted quarantine laws, and the examination of them will show that they are scarcely behind those now existing in their practical operation; and yet in 1798, when the city contained only 58,000 inhabitants, between 2400 and 2500 died with the disease the law was passed to

> prevent. At subsequent periods laws have been enacted and precautionary measures taken; still, in the years 1805 and 1822, the city found itself in the midst of pestilence, and in some other years cases have occurred, but the number has been so small, that the disease could hardly be regarded as epidemic." (p. 41.)

It appears that the cleanliness of the city of New York has greatly increased during the last twenty years; yet it seems that there are still abundant sources of effluvia and nuisances of different kinds, and that although the authorities have taken "incipient measures" to establish sanitary regulations, "yet what has been actually done has been far behind the absolute wants, the absolute demands of humanity." (p. 43.)

But we must now quit this Report, and state our general conclusions as to the efficacy of quarantines in the case of yellow fever.

1. As we deem contagion to be only an accidental property impressed on a fever by peculiar circumstances, and as of course this property can only be maintained by a continuance of the same causes which produced it, quarantines may be abandoned as useless in all cases where we feel certain that the poison will not meet with its conditions of existence. These conditions are a certain heat, a certain moisture, and certain effluvia from animals or vegetables. Thus we consider that in this climate, the yellow-fever poison would be incapable of development during the greater part of the year, in consequence of the cold, and perhaps through the whole year, in consequence of our habits of cleanliness and ventilation, which, if not yet so perfect as they will be, have yet so greatly improved as to have kept at bay all pestilences during the last century. We consider that the proposition of receiving the sick of the Eclair into Haslar Hospital was perfectly justifiable, and we entertain no fear but that, from the ventilation and cleanliness of the hospital, the disease would have lost its contagious property, or, in other words, that its self-reproductive property would have become inappreciable.

2. But in the case of the yellow-fever districts, where the conditions of existence cannot be so readily removed, the question has a different aspect.[*] It is true that quarantines have very often failed. They were rigidly enforced at New York in 1822, and yet the disease appeared; in 1797, they were in full vigour at Philadelphia, and yet the disease was not arrested. Dr. Ferguson states that at Martinique, where they established a strict quarantine, particularly against Guadaloupe, "they have been consumed with yellow fevers; but at Dominique, Tobago, St. Vincent's, where they established none at all, they had not had, as far as I could learn, a single case."

In 1810, on Sir W. Pym's arrival at Gibraltar, measures of seclusion and separation were adopted, and soon afterwards the disease declined. By one party this is attributed to the influence of the quarantine measures; by the other to a sudden change of weather, accompanied by a cold north wind, which checked the disease. (See Cyclop. of Pract. Med., vol. ii., p. 291.) In 1813, at Gibraltar, the dockyard labourers were strictly separated from the rest of the garrison, and were stated by two officers of the quarantine department to have escaped altogether the disease. But Dr.

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[*] Dr. Hort, in the New York Report, surmises that the gradual improvements in New York and Charleston, and the adjacent countries, have removed these places from the yellow-fever region. The northern limit of the yellow-fever district is stated by a committee of physicians of New Orleans to be twelve degrees south of what it was a hundred years ago: "and this great result has been accomplished, not by quarantine laws, but by judicious police regulations." (Report, p. 202.)

O'Halloran, who was in Gibraltar at the time, proved satisfactorily that many cases occurred among these men. Dr. Gilchrest remarks on this point:

> "During a residence at Gibraltar we had ample means, by referring to the declarations of the official authorities at the dockyard, of confirming the assertion of Dr. O'Halloran as to several cases in 1813 having occurred there as well as some deaths, indeed the names of twenty-three could be given, were it necessary; so that, regarding the original statements, no impressions favourable to the accuracy or candour of the quarantine officers, who made them, can be entertained; and in the justly severe remarks of Dr. O'Halloran on the subject, to which no reply has been made, future observers of circumstances connected with the public interest have received a salutary warning." (Op. cit. p. 292.)

Nevertheless, however strong the evidence may be against quarantines, there is no doubt that, as in the case of the Eclair and Boà Vista, the Bann and Ascension, the yellow fever, when it becomes contagious, and is imported into a place in the yellow-fever region where it meets with its conditions of existence,—conditions which cannot at present be removed,—can only be combated by such rude contrivances as measures of isolation and seclusion. It remains therefore to be determined by actual observation how these measures may be best and practically applied; that is, we require some certain knowledge of the power of diffusion of the virus, of its durability, and of its rapidity of propagation.

3. On all these points, however, there is great uncertainty. It is essential for the interests of commerce that the quarantine period should be only the time required for the natural death of the poison; but is this 10 days, as some assert, or will it extend occasionally to 50 or 60 days, as Dr. Vaché makes it appear? We have little doubt that the virus will occasionally develope itself gradually in a fit atmosphere, even without acting on human systems; thus at Boà Vista, a soldier of the 5th guard at the Fort on the island was attacked in the Fort, and without having had intercourse with sick people certainly more than 15 days after the departure of the Eclair. (Report, case of Jose Sancha, p. 25.) The dates are not very clear, but it must have been as much as 15 days, as the 4th guard, who came on duty when the Eclair left, were 12 or 13 days there, and Jose Sancha, who relieved them, was 3 days on duty before he was taken ill. But we do not put much faith in Dr. Vaché's statements, and we have been able to find little additional information on this point from authors.

The incubative period is generally allowed to be from 2 to 12 days; it is possible, however, that it may occasionally be much more than this. Marsh fevers have become developed weeks and even months after exposure to the exciting cause. The incubative period of the African fever appears to have been in many cases 12, 14, 15, even 22 days. (Climate and Diseases of Africa, pp. 151-2-3 and 233.) It appears that the disease did not seem to be of milder type after this long incubative period, and therefore the mere severity of the "hæmagastric pestilence" does not argue a short period of latency.

The quarantine space also has yet to be decided. Five hundred yards has been stated by some writers as the maximum diffusion distance of the poison. The American Commissioners fix 300 yards. A much shorter distance is, however, assigned generally to the febrile poison of this country; and we doubt whether any accurate rule can be yet laid down for the yellow-fever virus when it becomes contagious. From the ready way in which it confessedly loses its contagious property, we should believe the distance to be very small. The degree of adherence of the poison to clothes, blankets, and other articles in daily use, is another point requiring more data for elucidation. We are disposed to give little credence to the usually quoted instances of this occurrence.

It is not our part, however, to go into details on this subject, we have to deal only with principles, and although we believe that quarantines cannot in all cases of yellow fever be abolished,* yet we think they should be considered only as temporary expedients which must some time become obsolete.

The attention of government as well as of individuals should be fixed on the means of acquiring that knowledge which alone can warrant the abolition of quarantines. The poison or poisons of tropical fevers must be more carefully studied. We have supposed that the same virus evolved from certain soils, or productions of the soil undergoing chemical decomposition, produces both the common remittents, and the severe remittents, or yellow fever, the highest or epidemic degree of which is sometimes, though not always, contagious. But it must be remembered that this opinion does not exclude, or rather that it includes, the supposition that there must be many modifications in point of chemical composition produced in this virus by external circumstances of soil, temperature, humidity, occult conditions of the atmosphere, &c. We have seen Humboldt referring to such differences as the only mode of explaining the insusceptibility of the inhabitant of a yellow-fever country to the poison of his own district, and his susceptibility to the poison of another district. It is probable that infinite modifications may exist in this poison even in epidemics of yellow fever; for how else are we to reconcile the varying effects ascribed to it, and the difference in symptoms in different years? And although it is not in this generation, nor in the present state of science, that we can look for a solution of these abstruse and profound questions, it is not less true that, without a greater knowledge than we now possess, the practical details of quarantines must remain subjects of doubt and controversy.

This increased knowledge is to be obtained, not by a search after these poisons as they exist in the atmosphere, but by an inquiry into the conditions antecedent to their production, and when they are thrown off into the states necessary to their existence, the means whereby they live, and the sources whence they gain increase and development. In this way only will the question of quarantines receive a complete and satisfactory elucidation. Then only can we expect to reconcile the interests of commerce and the facilities of intercourse with the duties which a protecting government owes to its citizens, and the care which a state professes for the health of its people. It would be well if the medical profession, whose opinion guides the state, would reconsider the grounds of their approval or dislike of quarantines. It would be wise if all parties would agree that in the present state of our knowledge extreme opinions either way are inadmissible;—if they would allow that quarantines must sometimes be admitted to be necessary, but that they must be considered hurtful only when so exclusively considered as to draw off the attention from those internal measures of prevention, which are still more powerful than quarantines in annihilating the pestilential poisons, and in thus arresting the stealthy inroads and the deadly march of these destroyers of mankind.

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* For example, it must be acknowledged that they would have been useful at Boa Vista or at Ascension.

Since the above article has been in type, we have received the 2d edition of Dr. Bartlett's 'History of the Fevers of the United States.'* This work is divided into four parts; 1. Typhoid fever. 2. Typhus fever. 3. Periodical fever. 4. Yellow Fever. The last part is the only one which concerns us at this time, and its consideration will not detain us long, as it is not written from personal observation. Dr. Bartlett informs us, that he "has never met yellow fever at the bedside, and has had no opportunities of direct and clinical study of the disease."

He considers, however, that yellow fever differs altogether from periodical fever. His chief ground for this opinion seems to be that remissions do not occur in yellow fever. "Yellow fever does not belong to the class or family of periodical diseases; it is not properly remittent nor intermittent in its type; *it is not marked by any obvious nor regular series of recurrent phenomena.*" (p. 486.)

The occurrence of remissions is to be attributed to the "periodical element" being engrafted on the true yellow fever. In our former article we referred to this point, and have necessarily anticipated the arguments which refute this statement. This will save us from the necessity of further considering it at this time.

With regard to contagion, Dr. Bartlett does not seem to have arrived at any certain conclusions. He details the three chief opinions, viz. that the yellow fever is always contagious, that it is never contagious, and that it is sometimes or contingently contagious, but he expresses no personal opinion on the subject. We cannot avoid remarking that this chapter bears evidence, as does indeed the whole section on yellow fever, of a defective acquaintance with the English writers on this subject. Thus Dr. Bartlett quotes the case of the Hussar from Blane's 'Diseases of Seamen,' and criticises the loose and inaccurate account given in this work by Sir Gilbert Blane; but he does not seem to be aware that in 'Select Dissertations,' the account has been amplified and rendered infinitely more conclusive and stringent than it was before. The cases of the Bann, the Kent, the Scout, and others are left unnoticed; and the only remark made respecting the fever on board the Eclair is, that "it was yellow fever arising during the passage from the coast of Africa, and it occurred at Boa Vista for the *first time*, nearly a month after the departure of the Eclair." (p. 404.) The fever of the Hankey Dr. Bartlett considers to have been "remittent and not fellow fever." (p. 526.)

Dr. Bartlett devotes only two pages to the subject of exemption from subsequent attacks; he details the inquiries made at Gibraltar, to which we have already referred,† and then continues,

> "Dr. Lewis has investigated this question with some care, and the result of his inquiries differed somewhat from that which I have just given. Five respectable citizens of Mobile, he says, have had the disease as many as three times, according

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* The History, Diagnosis, and Treatment of the Fevers of the United States. By Elisha Bartlett, M.D. Philadelphia, 1847. pp. 534.

† See our 1st Number, p. 70.

to the testimony of competent judges. As many as 20 of his own patients who were mildly attacked in 1843, stated, that according to their physicians they had already had yellow fever during the epidemics of 1817 or 1839. Dr. Lewis concludes that in 1843, about one fifth of the patients who had mild yellow fever, had been subjects of the disease during previous epidemics." (p. 464.)

The origin of yellow fever from marsh miasmata, is denied on two grounds: 1st, That in countries devastated by periodical fevers, true yellow fever is not seen; and 2dly, that in some places subject to yellow fever there are no possible sources from which malaria could be evolved. (p. 466.) The first proposition *per se* is imperfect, as it is not stated whether the malarious districts referred to are in temperate or in tropical climates; a very essential point, as the firmest supporters of the miasmatic origin of yellow fever have always contended, that a high temperature and perhaps occult atmospheric conditions are necessary to produce that modification of miasma, which gives rise to yellow fever rather than to any of the numerous and variable forms of periodical fever. The second proposition also is not yet sufficiently proved. Dr. Bartlett refers to Gibraltar as not containing any of the usual sources of malaria, but to this objection we have already replied. The other examples to which Dr. Bartlett refers, viz. Woodville, Barbadoes, Brimstone Hill, and Stoney Hill in Jamaica, are doubtful cases. It must be remembered, that any place, if it be situated in certain geographical limits, and possess certain other conditions necessary for the propagation of the poison, may be visited by epidemics of yellow fever during the prevalence of those mysterious influences which give so singular a law of periodicity to the origin and recurrence of this peculiar poison, although such place may not present any of the more obvious and universally recognised sources of marsh miasmata.

We observe also, that Dr. Bartlett believes the yellow-fever poison to be of "terrestrial origin."

> "In regard to the essential poison," he writes, "the application of which to the system gives rise to yellow fever, I can do but little more than to repeat the remarks which have already been made in connexion with the essential etiological poisons of other fevers; the nature and composition of the former, like those of the latter, are entirely unknown to us. It would seem to be clearly enough of *terrestrial origin.*" (p. 480.)

This opinion, coupled with his uncertainty respecting the contagion of the disease, narrows the question, as far as Dr. Bartlett is concerned, to this point. He has certainly not proved that yellow and periodical fevers are distinguishable by symptoms; he expressly declares the post-mortem appearances insufficient as diagnostic marks, as he doubts whether the "yellow colour" of the liver described by Louis, is a constant appearance after death, "there is good reason to believe that the change in the colour of the liver is not a constant occurrence." (p. 501.) Therefore we would ask him whether it is not *most probable* that the yellow fever, traced up, as it has been by numerous transition cases, from well-marked and defined periodical fevers, may not be simply a modification of these latter arising from unusual epidemic influences not yet recognised by our imperfect science; or, that if the terrestrial poison be really specific, and give rise to a peculiar disease which will hereafter be recognised by pathognomonic symptoms or by distinctive post-mortem appearances, is it not *most likely* that the source of this poison is to be sought in those same fruitful generative "foyers" from which issue the several allied poisons which, according to inherent differences in themselves, or in the bodies on which they act, produce the multiform varieties of malarious fevers? The symptoms and post-mortem appearances are detailed with clearness and conciseness by Dr. Bartlett. There are several points, however, which would bear discussion did our space permit. We are sorry to find Dr. Bartlett speaking in a disparaging tone of the great mass of English writings, with which, however, he appears imperfectly acquainted, and a deeper study of which would have led to a more complete history of yellow fever. We observe that Dr. Bartlett confounds Dr. John Hunter, the author of the 'Diseases of Jamaica,' with the great John Hunter. (p. 520.) It is an unimportant mistake; we merely point it out as worthy of correction in a future edition.

Art. VI.

Lectures on Nutrition, Hypertrophy, and Atrophy, delivered in the Theatre of the Royal College of Surgeons, May, 1847. By James Paget, Professor of Anatomy and Surgery to the College. (From the London Medical Gazette.)—*London,* 1847. 8vo, pp. 50.

We are desirous of bringing this short course of Lectures under the notice of our readers, as a most felicitous example of the new and superior method of conjoint physiological and pathological investigation, which is now being carried out by several of our most earnest and enlightened inquirers. We have spoken of it as *new;* and yet it is really *old*, having been clearly indicated by John Hunter. And what branch of biological inquiry, we may ask, did his comprehensive and penetrating view fail to discern? But so complete an isolation has subsequently existed between the physiologist and the pathologist, between the man of science and the medical practitioner, as to have kept out of sight the great truths which it was the labour of his life to develope; and now that they are again brought forwards, with all the aids and illustrations derived from modern science, they come to us with the value and attractiveness of new revelations.

Nor should we omit to point out, how completely the choice which the College has made, in the appointment of its new Professor, has been justified by the result of this, his first trial; for a more philosophical exposition of any one of the subjects that fall under our peculiar cognizance, more original and suggestive in its character, and more lucid and elegant in its diction, it has never been our good fortune to meet with. To say that it may fairly challenge comparison with the most successful efforts of the distinguished anatomist and physiologist, who so worthily occupies the Hunterian chair in the same College, will be admitted to be very high praise; yet we are confident that those who will thoughtfully peruse Mr. Paget's Lectures with minds prepared by previous cultivation for the full appreciation of their value, will agree with us that it is by no means undeserved. His nomination to the professorship is due to the ability which he has shown in the arrangement and description of the pathological portion of the Hunterian Museum; his descriptive Catalogue of which, now in course of publication, we shall take an opportunity of noticing hereafter. In selecting a subject for the present course, being embarrassed what best to choose in the multitude of rare and instructive things at his disposal, he resolved, he tells his auditors, "to make no choice at all, and take what first presents itself to the student of the museum," namely, the first two series of preparations illustrating the simple excess, and the simple deficiency of nutrition in parts, or hypertrophy and atrophy. In order to attain a right comprehension, however, of these processes which are accounted morbid, it was necessary to examine carefully into the nature and conditions of the healthy formative process; or, in other words, to study the physiology of nutrition. The subject is thus introduced:

> "The formative process manifests itself in three modes, which, though they bear different names, and are sometimes described as if they were wholly different things, yet probably are only three expressions of one law operating in different conditions. The three, enumerating them in the order of their time, are development, growth, and assimilation.
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> "By *development*, we mean the process by which a tissue or organ is first formed, or by which one, being already imperfectly formed, is so changed in shape, as to be fitted for a higher function; or, finally, is advanced to the state in which it exists in the most perfect condition of the species." (p. 4.)

Thus when, in the child, cartilage is converted into bone, there is not necessarily an increase of size; or, if there be that, there is something more, namely, a change of structure adapting the fabric to higher conditions of existence; or, going back to the earlier phases of embryonic life, when from the mass of simple cells which constitutes the early germ, the rudiments of the vertebral column, the nervous system, the digestive and circulating apparatus, &c., are evolved, there is far more in the process than mere increase in bulk; there is a change both in the form and in the intimate structure of the whole organism, a morphological and histological transformation. As an interesting example of the distinction between development and increase or growth, Mr. Paget alludes to cases in which they usually concur, but in which occasionally one proceeds without the other. Thus he adverts to two adult idiotic brains nearly of the same size, both being equally diminutive; in one of which there was a due proportion of all the parts, the development having proceeded although the growth was checked; whilst in the other there was a deficiency of the posterior lobes of the cerebrum, its development having been apparently checked at about the beginning of the fifth month, although its growth must have continued for some time longer. He adverts also to malformations of the heart as examples of arrest of development, even where the processes of growth have passed their ordinary limits:

> "It is, then, by a change to a higher state of form or composition, that development differs from *growth*, the second mode of the formative process. For in mere growth, no change of form or composition occurs; parts only increase in weight, and, usually, in size. In growth there is an addition of quantity, but no improvement in the quality of a part; the power of the growing part increases with the growth, but is, after all, only more of the same power; so, in the attainment of manhood, the heart of a boy, having all its necessary parts, and all well formed, acquires perfection by acquiring greater bulk, and therewith greater power.
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> "Lastly, in the formative process, as it is normally manifested in the adult, i. e. in ordinary *assimilation*, parts only maintain their status. No perceptible change of size or weight ensues; no change of form or composition; there is exact stability. But this stability is maintained through continual changing of the

particles; the change consisting in the regular formation of new parts in the place of those which are impaired, or die, in the course of life." (p. 5.)

We cannot think that Mr. Paget is happy in his choice of a term to designate the continuance of the formative process in the adult, after development and growth have ceased; since the etymology of the word, and we believe the ordinary sense in which it is employed, convey a very different idea of the part of the process to which it should be applied. By *assimilation*, is commonly understood the operation by which the nutrient materials absorbed into the system, are brought to a resemblance, as regards their composition and character, to the tissues they are destined to nourish, whether in the way of development, growth, or reparation. Thus the conversion of albumen, the raw or unorganizable material, into the plastic or organizable fibrine, is commonly treated as one portion, indeed as the most important part of the assimilating process. We cannot see that there is any propriety in departing from the ordinary method of employing the term; and we shall consequently speak of the third form of the nutritive operation distinguished by Mr. Paget, as the *maintenance* of tissues and organs in their normal condition.

In elucidating the mutation of parts, in the perfecting of which the formative process is continually occupied in the healthy body, Mr. Paget speaks:

1st. Of the sources of that impairment, or of that wear and tear to which every part of the body appears to be subject.

2d. Of the conditions necessary for the healthy state of the formative process, by which the wear and tear is repaired.

3d. Of the formative process itself.

Each of these subjects is systematically discussed; and we shall briefly follow in the track which Mr. Paget has marked out.

I. *Sources of Impairment.* The continual deterioration of the body is traceable to two principal sources; namely, the wearing out of the parts by exercise, and the natural deterioration or death of every part or organ, independent of the decay or death of the whole body, after a certain period of existence. The first of these is especially manifested in that disintegration of the muscular and nervous tissues, which is now generally admitted to be a necessary result of their functional activity; and in that wear and tear of the passive organs of locomotion, which must proceed from the friction, pressure, stretching, &c., to which they are subjected, notwithstanding the various beautiful contrivances adopted to diminish these as much as possible. In regard to the changes in the nervous system produced by its exercise, we find the following very interesting and suggestive remark; which furnishes a complete answer to those who charge the physiologist with materialism, for maintaining that no operation of the human mind can take place, in man's present sphere of existence, without a physical change in the nervous system:

> "To this conclusion also, that mental exercise, whether perceptive or active, impairs the structure of the brain, we are led by our knowledge of the nature of the mind; for to the principle, the immaterial thing, we cannot ascribe a weariness; it cannot be obnoxious to waste or to decay: mental fatigue, as we vaguely call it, is only what the mind feels of an impaired state of the brain; and the recovery from what we call a weary mind is the restoration, not of the mind itself, but of the organs which connect it with the external world, and in which, during tranquil sleep, the reparative nutrition goes on undisturbed." (p. 5.)

The merit of having first maintained as more than a mere hypothesis, that each part of the organism has a limit to its term of existence, in virtue of its own individual and independent life, is assigned by Mr. Paget to Dr. Carpenter. We look upon this as a most important principle; and we are peculiarly struck with the felicitous manner in which it has been adopted, and its application extended by Mr. Paget. It is perhaps, however, best elucidated by a reference to the vegetable economy, where its results are displayed in a most obvious manner; the leaves having a very short and limited term of life, whilst the woody stem has a long and almost indefinite duration. The "fall of the leaf" is *not* the *cause* of the death and decay of the organ, but its *result;* for the decomposition of its tissues is already far advanced when its detachment occurs. Its functions have been fulfilled, its term of life is expired, and it is cast off, to be replaced by a new development of the same kind; which, again, is destined in its turn to be replaced by another. The successive generations of transitory and deciduous cells which constitute the parenchyma of the leaf, have the important vital operation of preparing the materials which are to afford the pabulum for the growth of the more permanent parts; and we find that the duration of their existence bears an inverse relation to the energy with which their functions have been performed,—those which work the least actively being the longest livers, and *vice versâ.* This kind of exuviation is much more constantly going on in the animal body, than is generally supposed; the researches of the microscopist having shown that every act of secretion involves the casting-off from the general structure, and the consequent death, of the cells which are the instruments of the operation; so that a constant loss and renewal thus goes on, which may be likened to that which we observe in an evergreen tree. Mr. Paget alludes to the hair and the milk-teeth as affording conspicuous examples of this exuviation in the animal body; and we shall quote his account of the process as it relates to the former, since it contains several details which we believe to be novel as well as interesting:

> "An eyelash which naturally falls, or which can be drawn out without pain, is one that has lived its natural time, and has died and been separated from the living parts. In its bulb such an one will be found very different from those that are still living in any period of their age. In the early period of the growth of a dark eyelash, we find its outer end almost uniformly dark, marked only with darker, short linear streaks, and exhibiting no distinction of cortical and medullary substance. Not far from its end, however, this distinction is plainly marked; dark as the cortical part may be, the medullary appears like an interior cylinder, of much darker, granular substance; and in young hair this condition is continued down to its deepest part, where it enlarges to form the bulb. Now this enlargement, which is of nearly cup-like form, appears to depend on the accumulation of nucleated cells, whose nuclei, according to their position, either by narrowing and elongation, are to form the fibrous substance of the outer part of the growing and further protruding shaft, or are to be transformed into the granular matter of the medullary portion. At this time of most active growth, all the cells and nuclei contain abundant pigment-matter, and the whole bulb looks nearly black. The sources of the material out of which the cells form themselves are at least two; the inner surface of the sheath, or capsule, which dips into the skin enveloping the hair, and the surface of a vascular pulp which fits in a conical cavity in the bottom of the hair-bulb.
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> "Such is the state of parts, so long as the growing hair is all dark. But as it approaches the end of its existence, it seems to give token of advancing age, by

becoming gray. Instead of the almost sudden enlargement at its bulb, the hair only swells a little, and then tapers nearly to a point; the conical cavity in its base is contracted, and hardly demonstrable, and the cells produced on the inner surface of the capsule contain no particle of pigment. Still, for some time, it continues thus to live and grow, and we find that the vigour of the pulp lasts rather longer than that of the sheath or capsule, for it continues to produce pigment-matter for the medullary substance of the hair, for some time after the cortical substance has been entirely white. Thus, we can trace the column of dark medullary substance, growing paler and more slender, and perhaps interrupted, down to the point of the conical pulp, which, though smaller, is still distinct, because of the pigment-cells covering its surface.

"At length, the pulp can be no longer discerned, and uncoloured cells alone are produced, and maintain the latest growth of the hair. With these it appears to grow yet some further distance, for we see traces of the elongation of their nuclei into fibres, in lines running from the inner surface of the hair, and we can always observe that the column of dark medullary substance ceases at some distance above the lower end of the contracted hair-bulb.

"The end of all this, is the complete closure of the conical cavity in which the hair-pulp was lodged; the cessation of the production of new cells from the inner surface of the capsule, and the consequent detachment of the hair as a dead part, which now falls by the first accident,—falls, sometimes, quite bare and smooth on the surface of its white bulb, but sometimes brings with it a layer of cells detached from the inner surface of the capsule.

"Such is the life of a hair, and such its death; which death, you see, is natural, spontaneous, independent of exercise, or of any mechanical external force,—the natural termination of a certain period of life. Yet before it dies, it makes provision for its successor, for when its growth is failing, you often find, just below the base of the old hair, a dark spot, the germ or young pulp of the new one, covered with cells containing pigment, and often connected by a series of pigment-cells with the old pulp or capsule. And this appears to be the product, as it were an offshoot, from some portion of the capsule of the old hair; for though it may sometimes appear only in the form of a conical pulp, yet more often, I think, it shows signs of connexion with the capsule, and the cone is only more evident than the rest because of its covering of dark cells." (p. 6.)

In this process, we seem, as Mr. Paget justly observes, to have an image on which are plainly marked—though, as it were, in rough outline—all the great features of the process of nutrition; namely, the continuance of growth for a limited period, the degeneration and death of the part, and, in the very act (as it were) of its own decay, the communication of life to its successor. We have here, in fact, the repetition, on a larger and more complex scale, of the history of the simplest cellular plant; which, having arrived at its term of life, bursts or dissolves away,—thus ceasing to exist as an individual,—but at the same time sets free the germs of a new generation. But, as it might be affirmed that what takes place with regard to a superficial structure like the hair is no fair illustration of the process of nutrition as it occurs in internal parts, Mr. Paget refers to the case of the deciduous teeth; in which we have to observe not merely the death and the casting-off of the crown, but the degeneration and absorption of the fang with its bony sheathing and vascular and nervous pulp. "This degeneration is accompanied by some spontaneous decomposition of the fang, for it could not be absorbed unless it were so changed as to be soluble. And it is degeneration, not death, which precedes its removal; for when a tooth-fang really dies, as that of the second tooth does in old age, then it is not absorbed, but is cast out entire as a dead part." The moulting or shedding of the antlers, the entire desquamation of serpents, the change of plumage in birds and of the hair in mammals, are all examples of the same great fact, that the several organs have their respectively-appointed periods of life, and times for degeneration, death, and removal; being destined to be replaced by others, which in their turn are to be developed to perfection, to live their life in the mature state, and in their turn to be cast off. As another example of the determinate life of individual parts, Mr. Paget notices the gland-cells; and as the connecting link between their office and the nutrition of the most highly-organized parts, as well as a manifest instance of determinate length of life and natural death, he adduces the ova.

> "These attain their maturity in fixed successive periods of days; they are separated (as some of the materials of several other secretions are) while yet living, and with a marvellous capacity of development, if only they be impregnated during the few days of life that remain to them after separation; but, if these days pass, and impregnation is not effected, they die, and are cast out, as impotent as the merest epithelium-cell." (p. 7.)

It is to repair the effects of these natural deteriorations, that the process of nutrition in the adult is chiefly directed; and the occasional errors of that process, and the consequent production of disease, seem to be more frequently attributable to the disturbance or the want of certain conditions essential to its due performance, than (if we may thus employ the term) to an *idiopathic* suspension or perversion of itself.

II. Among the *conditions necessary to healthy nutrition*, Mr. Paget enumerates the four following as the chief:

1. A right state and composition of the blood or other nutritive material.
2. A regular and not far distant supply of such blood.
3. (At least in most cases) a certain influence of the nervous system.
4. A natural state of the part to be nourished.

These we shall now successively consider, under his guidance.

1. *Right state of the nutritive fluid.* This is dependent upon the due performance of the blood-making and blood-purifying functions,—healthy digestion, healthy respiration, healthy excretion; and also upon the proper assimilating action of the blood itself.

> "Notwithstanding the diversity of materials put into the blood, and the diversity of conditions in which the functions ministering to its formation are discharged, yet the blood throughout life retains in each person certain characters as peculiar as those outer features of the man, for the continual renewal of which it provides appropriate materials. And by this assimilative power of the blood it is, that the tissues are continually guarded; for by it many noxious substances introduced into the blood are changed and made harmless before they come to the tissues; nor can any substance introduced from without produce disease in an organ, unless it be such an one as can escape the assimilative and excretory power of the blood itself." (p. 8.)

Although the more considerable alterations which the blood presents in its morbid states may be detected by our present chemical and microscopical resources, yet it must be confessed that by far the greater number of these are of such a nature, as altogether to elude our existing means of research, and can be judged of only by their results. The living tissues are often much better indicators of changes in the character of the influences to which they are subjected, than are any of the instruments by which the chemist attempts to determine their existence or their nature. Thus Matteucci states that his galvanoscopic frog will indicate the existence of electric disturbance of such feeble intensity as not to produce any effect upon the most delicate galvanometer. We know that an infant may be poisoned by milk imbibed from its mother's breast, if the secretion have been formed during or soon after violent mental agitation; although no alteration can be detected in the physical and chemical characters of the secretion. And, as Mr. Paget justly remarks, "the healthy process of nutrition depends on so nice a refinement of affinities,—such an exact and constant adjustment of the adaptation between the blood and the tissues,—as we can only discern when we see the consequences of its loss." In illustration of this position, Mr. Paget refers to Dr. Budd's and his own essays on Symmetrical Diseases;* the true value of which is only now becoming generally appreciated. Excluding the cases of congenital symmetrical defects, and a few which seem to depend on morbid influence of the nervous system, it may be stated generally that all symmetrical diseases depend on the presence of some morbid material in the blood, which usually enters into combination with the tissue that is diseased, or with the organized product of the morbid process. Such a substance fastens upon certain spots or islands on one side of the body, leaving the surrounding parts unaffected; and precisely similar spots or islands are affected in like manner on the other side. The conclusion is unavoidable, that, however closely one portion of skin or bone may seem to resemble another portion of skin or bone, the only parts that are *exactly* alike are those which repeat each other on the opposite sides of the body. Thus although no power of artificial chemistry can determine the difference, the chemistry of the living body makes it evident; for the morbid material tests out the parts to which it has the greatest affinity, unites with these, and passes by the rest. The following remarks are highly ingenious and suggestive:

> "In what these differences consist, I do not pretend to explain. Some of them may not even be permanent, but may depend on the several parts of a bone, or of the skin, of a limb (for example), being in different stages of development or degeneration. The symmetrical parts of the tissue being in this respect exactly alike, may be simultaneously and equally affected by the disease; while other parts of the same remain unaffected, till in the course of time they attain, by development or degeneration, the very same condition as the parts first affected. Then, if the morbid material still exist in the blood, these parts also become diseased; and so in succession may nearly the whole of a tissue. This view agrees very well with the fact, that these symmetrical diseases spread, and so give evidence that a part which in one week or month is not susceptible of the influence of the morbid material, may in the next become as susceptible as that which was first affected." (p. 9.)

A further illustration of the same general principle is afforded by the fact that the "seats of election" of certain morbid products bear a close conformity, not merely on the two sides of the same individual, but also in different individuals. Thus we find the syphilitic poison attacking certain parts of the tibiæ and of the skull, with great uniformity: and Mr. Paget refers to two lions' pelves, in which new osseous matter has been deposited, as the product of some disease resembling human rheumatism, in a most complex and irregular pattern; and in which almost every spot and line in one is represented in the other, with an exactness only inferior to the symmetrical correspondence between the two sides of each.

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* Medico-Chirurgical Transactions, vol. xxv.

> "Such a fact proves on the one hand, as the cases of symmetrical diseases do, that the composition of the several portions of what we call the same tissue is not absolutely identical; if it were so, these diseases would as often affect one part of a bone or other tissue as another part, or would affect all parts alike. And it proves on the other hand, a constant similarity, even an identity, of the morbid material on which each of these diseases depends, though it be produced in different individuals; so that we may venture to predict that, whenever chemistry shall discover the composition of these substances, it will be found as constant and as definite as the composition of those inorganic substances which the science has most successfully scrutinized." (p. 10.)

The argument is still further strengthened by the fact pointed out by Dr. W. Budd, that these diseases often show that, next to the parts which are symmetrically placed, none are so nearly identical in composition as those which are analogous,—as the corresponding parts of the superior and inferior extremities. All these facts agree in indicating the perfect and most minute exactness of the adaptation which exists in health between the blood and all the tissues; as well as the almost inconceivable minuteness of the alteration in this adaptation, which may become a source of disease. And having laid this foundation in a broad and secure induction, Mr. Paget proceeds to build upon it some speculative views of great interest, relative to a source of change in the constitution of the blood, which may be in one case for the better, and in another for the worse, but which either way may be hereafter made of much importance in both physiology and pathology. The germ of his hypothesis is furnished by the remark of Treviranus,—that "each single part of the body, in respect of its nutrition, stands to the whole body in the relation of an excreted substance;" in other words, every part of the body, by taking from the blood the peculiar substances which it needs for its own nutrition, does thereby act as an excretory organ, inasmuch as it removes from the blood that which, if retained in it, would be injurious to the nutrition of the rest of the body. Thus the phosphates which are deposited in our bones are as effectually excreted from the blood and the other tissues, as those which are discharged with the urine. But Treviranus did not, in Mr. Paget's opinion, appreciate the full importance of the principle which he thus clearly though briefly stated; and the following is given by Mr. Paget as an example of its capability of extensive and interesting application:

> "The influence of this principle may be considered in a large class of outward-growing tissues. The hair, in its constant growth, serves, over and above its local purposes, for the advantage of the whole body, in that, as it grows, it removes from the blood the bisulphide of proteine and other constituents of its substance, which are thus excreted from the body. Now this excretive office appears, in some instances, to be the only one by which the hair serves the purpose of the individual; as, for example, in the fœtus. Thus, in the fœtus of the seal, and I believe of most other mammals, removed as they are from all those conditions against which hair protects, a perfect coat of hair is formed within the uterus, and very shortly after birth is shed, and replaced by another coat of wholly different colour, the growth of which had begun within the uterus. Surely, in these cases, it is only as an excretion, or chiefly as such, that this first growth of hair serves

to the advantage of the individual. The *lanugo* of the human fœtus is an homologous production, and must, I think, similarly serve in its economy by removing from the blood, as so much excreted matter, the materials of which it is composed.

"Now, if this be reasonable, we may carry this principle to the apprehension of the true import of the hair which exists in a kind of rudimental state on the general surface of our bodies, and to that of many other permanently rudimental organs, such as the mammary glands of the male and others. For these rudimental organs certainly do not serve, in a lower degree, the same purposes as are served by the homologous parts which are completely developed in other species, or in the other sex. To say they are useless, is contrary to all we know of the absolute perfection and all-pervading purpose of creation; to say they exist merely for the sake of conformity with a general type of structure, is surely unphilosophical, for the law of the unity of organic types is, in larger instances, not observed, except when its observance contributes to the advantage of the individual. No: all these rudimental organs must, as they grow, be as excretions serving a definite purpose in the economy by removing their appropriate materials from the blood, and leaving it fitter for the nutrition of other parts, or adjusting the balance which might else be disturbed by the formation of some other part. Thus they minister to the self-interest of the individual, while, as if for the sake of wonder, beauty, and perfect order, they are conformed with the great law of the unity of organic types, and concur with the universal plan observed in the construction of organic beings." (p. 11.)

It is obvious that no tissue can be produced, whose materials do not pre-exist in the blood; and it would seem to be the general rule that the presence of the material is the immediate occasion of the development of the tissue. Thus, when one kidney is destroyed by disease or is purposely removed, the other usually becomes larger after a time; a considerable amount of new tissue being developed in it, as a result of the imperfect separation of the elements of the urinary excretion (which form part of the nutritive materials of that tissue), and of their consequent accumulation in the blood. "What then happens? The kidney grows; more renal cells develope, and discharge, and renew themselves; in short, the existence of the constituents of the urine in the blood induces the formation of renal substance." So it is also with the deposition of fat; so also with the production of various morbid organisms in many of the inoculable and other blood-diseases. The influence said to be exerted by various kinds of diet, in promoting the growth of special tissues,—as the muscles, the bones, the hair, or the wool,—would be referable, if real, to the same cause.

"Now, if we combine these two principles,—*firstly*, that the blood is definitely altered by the abstraction of every material necessary for the nutrition of a part; and *secondly*, that the existence of certain materials in the blood induces, or, at the least, favours, the formation of corresponding tissues,—it will follow, at any rate as a reasonable hypothesis, that the order in which the several organs of the body appear in the course of development, while it is conformable to the law of imitation of the parent, and with the law of progressive ascent towards the higher grade of being, is yet (at least in part, and in this part more directly) the result of necessary and successive consequences, the formation of one organ or series of organs inducing or supplying a necessary condition for the formation of others, by the changes successively produced in the composition of the nutritive material from which they all take their nutriment. In other words, the development of each organ or system, co-operating with the self-development of the blood, prepares it for the formation of some other organ or system, till, by the successive changes thus produced, and by its own development and increase, the blood is fitted for the maintenance and nutrition of the completed organism." (p. 13.)

The same principle may be applied to numerous individual instances, on the assumption (in which we seem perfectly justified) that certain organs stand, in their nutrition, in a complemental relation to each other; so that neither of them can be duly formed, or maintained in a healthy structure, unless the right condition of the blood be induced and preserved by the formation of the other. Although it may be difficult to produce direct evidence in support of this theory, there are many facts which can be explained by it so well, that they become evidence for it,—which facts are the fairer subjects for theoretical explanation, since they must be admitted to be hitherto wholly unexplained.

"Among these is the general fact, that a great change in nutrition rarely takes place in one organ at a time, but usually affects simultaneously two or more parts, between whose nutrition there is a manifest and constant connexion, although there is little or no relation between their external functions. Such, to take an instance from a large class, is the connexion between the growth of various appendages of the integument, and the development or maintenance of the genital organs. This appears to be a general rule. The growth of the beard at the period of puberty in man, with which we are so familiar, is more instructively represented in many animals; especially in birds. In these, as you know, at the approach of every breeding time, the genital organs begin to develope themselves for the season, as in man they do for the whole time of vigorous life. And, commensurately with this development, the plumage (especially in the male bird) becomes brighter and more deeply coloured, both by the growth of new feathers, and by the addition of colour to old ones. The height and perfection of the plumage are coincident with the full development and activity of the reproductive organs; but, as in man, when the development of the reproductive organs is prevented, the development of the beard, and of all the other external sexual characters, is, as a consequence, hindered; so, in the birds, when the breeding season ends, and the sexual organs pass gradually into their periodic atrophy, at once the plumage begins to assume the paler and more sober colours which characterize the barrenness of winter." (p. 14.)

After adverting to the influence of castration in checking or modifying the growth of the horns of the deer, and in preventing their replacement after exuviation, as ascertained by the experiments of Sir Philip Egerton, Mr. Paget continues:

"It appears, then, as a general fact, that the development and activity of the reproductive organs have as a consequence, or as a necessary coincidence, a peculiar development and active growth or nutrition of certain other structures, which, therefore, form the external sexual characters, but whose external functions stand in no apparent, often in no conceivable, connexion with the generation of the species. The fact, of the certainty and extent of which there can be no doubt, has not been hitherto explained; it is explicable on the theory of complemental nutrition,—by believing that the materials which in the formation of these organs of external sexual character are removed from the blood, leave and maintain the blood in the state necessary for the further development, growth, and active function of the proper sexual or reproductive organs. In other words, I would say, that where two organs are thus manifestly connected in nutrition, and not connected in the exercise of any external office, their connexion is because one is partly formed of materials left in the blood by the formation of the other, so that each, at the same time that it discharges its own proper and external office, maintains the blood in the condition most favourable to the formation of the other.

"Now, if the theory be admissible, we may find through it the meaning of the commensurate development and nutrition of other organs, which in their external functions appear unconnected. Such are the concurrent development and activity of the thymus gland and the air-breathing organs, during the body's growth,—

of the thyroid gland and the brain (instances of commensurate development cited by Mr. Simon),—of the spleen and pancreas (as pointed out by Professor Owen), —and, I would add, of the embryo and the mammary gland; for the same theory may hold true concerning the formation of certain organs which are finally connected in their external functions." (p. 14.)

2. The second of the conditions enumerated as essential to healthy nutrition is *a regular supply of appropriate blood*, in or near the part to be nourished. Although the importance of this condition is made apparent by the frequent occurrences which involve a violation of it, yet there is a very general misapprehension respecting the precise manner in which it is fulfilled in the healthy system, and a consequent misinterpretation of several of the changes which present themselves in disease. It cannot be too strongly enforced upon all who would rightly study the physiology of nutrition, that the ultimate elements of *all* the tissues of the animal body are alike extravascular. Fat-cells, muscular fibre, ganglionic vesicles, nerve-tubes, &c. are all as completely distinct from the system of capillary vessels, as are hair, nails, epidermic and epithelial cells, &c. All growing parts imbibe their nutrient materials from the nearest source of supply; and if that supply be not furnished, their development must cease. And we find that the elementary particles of the so-called vascular tissues differ from those of the non-vascular, as to their relation to blood-vessels, only in this,—that the former are in nearer proximity to them than the latter. Thus in the substance of a muscle, which needs continual and active regeneration, the capillaries are so minutely distributed as to form a reticulation in the immediate neighbourhood of each fibre; yet still the fibrillæ, that occupy the centre of the bundle of which each fibre is composed, must imbibe their nutriment, not directly from the blood-vessels, but through the fibrillæ that form the outer part of the bundle. Compare this with cartilage, in which we find the blood-vessels confined to the membrane that invests the entire mass, so that the internal portions of the tissue can only derive their nutriment by absorption through the external; and it is at once seen that the difference is only one of degree, the fibrillæ of a single fibre of muscle repeating on a minute scale that which goes on in the entire mass of cartilage. And the cells of the non-vascular epidermis are continually growing at the expense of nourishment which they have imbibed from the vessels of the subjacent cutis; so that they differ from the cells of cartilage merely in having a vascular membrane on one side of them, instead of being altogether inclosed within it. The osseous tissue, as Mr. Paget justly remarks, is vascular or non-vascular according to the thickness of its layer; for when it forms only a thin lamina, such as that of the lachrymal and turbinated bones in man, or of the scapula in a mouse, the tissue can draw sufficient nutriment from the vascular membrane in contact with its surface, and no vessels pass into its interior; but when the mass is thicker, so that its component parts are further removed from the investing vascular membrane, then prolongations of this are transmitted through canals excavated in its substance, so as to bring the blood into nearer proximity with the interior parts of the mass. But the islets of solid bone encircled by the Haversian canals, are really as non-vascular as are the thin laminæ which are not penetrated by those channels.—It is only by acquiring clear ideas on this point, that we can understand the true relation between the circulation of the blood and the

nutrient processes. The former has for its object simply to bring the blood within the sphere of the latter; and the blood-vessels have no more direct connection with the various operations of nutrition, than have the water-pipes which supply a large city with the diverse applications, made by its diverse inhabitants, of the liquid they convey. It is only thus, as we have ourselves frequently urged,—and we are glad to find Mr. Paget in complete accordance with us on this point,—that we can keep clear of the error and confusion which result from speaking of the "action of vessels," as if the vessels really made and unmade the parts, perverted or restored their normal actions.

3. The third condition dwelt on by Mr. Paget, is *a certain influence of the nervous system.* On this point, as our readers well know, there has been much discrepancy of opinion; and we cannot ourselves believe that we are yet in possession of the whole truth regarding the degree of control which the nervous system can exert over the operations of nutrition. By some it has been imagined that "nervous influence" is as much concerned in the acts of nutrition and secretion as the "action of vessels;" and that the two in conjunction are the essential agents in the whole process. By others it has been maintained that the formative processes are in their own nature as independent of nervous agency, as they are of the action of vessels, although capable of receiving a modifying influence from it. All extended physiological inquiry appears to us to lead to the latter conclusion; and our present knowledge of the independent vitality of the elementary parts of the several tissues seems almost certainly to forbid the idea, that their formative activity can be in any way derived from the nervous system. On the other hand, there are numerous phenomena which show that the formative processes are capable of being greatly modified by the nervous system; and the question to be discussed, therefore, is the nature and kind of influence which they receive through it. The influence of mind, acting through the nervous system, over the processes of nutrition and secretion, is universally admitted; but it has not been made the object of that attentive and systematic study which it deserves. The cure of diseases under the influence of *faith*, is surely an object as worthy of consideration as their cure by physic or by surgical appliances; and we have no question whatever, that much of the success of confident empirics is due to this cause. Mr. Paget gives us a very good illustration of this principle, in his cure of a tumour in the breast, which had sprung up under the influence of a continual apprehension of cancer; for having heard the patient's account of it, and how her mind continually dwelt in fear of cancer, he made bold to assure her, by all that was certain, that the cancer, as she supposed it, would go away; and it did become very much smaller without any help from medicine, but he unfortunately lost sight of her before the cure was complete. We have known within our own youthful experience an equally successful case, in which a large wart was "charmed" away, by confident assurances of its departure within two or three weeks, if a little aqua pura coloured with spir. lavand. comp. were applied to it with perfect regularity three times a day. We may take another opportunity of reverting to this subject; and in the mean time may leave it to our readers to consider, whether much more advantage might not be derived from this principle of operation, than "regular" practitioners are commonly in the habit of looking for; and whether we

may not, at any rate, often avail ourselves of it as an auxiliary, even when we are placing our chief trust in plans of treatment which must have a more direct—and we hope a beneficial—influence over the morbid processes we are striving to correct.

The influence of mental emotions upon the processes of secretion is still more unequivocal; and we have here, also, positive proof that it is exerted, not merely in diminishing or increasing the activity of these operations, so as to modify the *quantity* of their products, but also in changing their character, so as to alter the *quality* of these. For example, the mammary secretion in a nursing female is augmented, whenever the idea of the infant is strongly brought before the mind, whether by its actual presence, or by a vivid conception in its absence; and it is frequently diminished or altogether suspended, under the influence of strong emotions of a depressing kind. The continuance of such emotions less powerfully excited, is well known to alter the quality of the milk, so as to render it unwholesome for the child; and there are cases on record, as we have already mentioned, which indicate that this bland nutritious secretion may be converted in a few moments into a deadly poison, under the influence of violent passion. Now although it seems possible to account for changes in the activity of the secreting process, by the control which the nervous system is known to possess over the calibre of the blood-vessels, yet no such explanation will account for such a perversion of it as that to which we have just alluded; and we see no other means of accounting for phenomena of this kind, than by attributing to the nervous system some kind of participation in, or control over, the processes of vital chemistry. And there will appear to be nothing inherently improbable in such a supposition, if the correlation between the nervous and the physical forces, on which we dwelt in our last Number (pp. 231–5), be admitted.

We have alluded to this subject for the sake of supplying a deficiency in Mr. Paget's treatment of it which rather surprises us; since one of the most direct and positive sources of proof of the close relation of the nervous force to the formative processes, is afforded (in our apprehension) by the manifest influence of mental states (which can only be exerted through the nervous system) upon the secreting operations. There are numerous cases, however, in which the influence of the nervous system, quite independently of the mind, is made manifest; but the nature of this influence is rather to be inferred from the results of its withdrawal, than to be demonstrated in any more direct manner. These results are chiefly seen in the altered state of the nutrition of parts exposed to external agencies, as the integuments generally, the extremities, and other external parts; and they may be generally expressed by the statement that the withdrawal of the nervous influence from a part renders it less able to withstand the destructive influence of physical agencies. Among numerous instances of this kind, Mr. Paget cites the following on the authority of Mr. Hilton, which is one of the most remarkable that has fallen under our notice.

> "A man was at Guy's Hospital, several years ago, who, in consequence of a fracture at the lower end of the radius repaired by an excessive quantity of new bone, suffered compression of the median nerve. He had ulceration of the thumb, and fore and middle fingers, which had resisted various treatment, and was cured only by so binding the wrist, that, the parts on the palmar aspect being relaxed,

the pressure on the nerve was removed. So long as this was done, the ulcers became and remained well; but as soon as the man was allowed to use his hand, the pressure on the nerve was renewed, and the ulceration in the parts supplied by it returned." (p. 17.)

We notice the following case as confirmatory of the ordinary belief, with regard to the correctness of which some physiologists have taken upon themselves to express a doubt, that the hair may be turned gray in a few hours, under the influence of strong mental emotion.

"A lady who is subject to attacks of what are called nervous headache, always finds next morning that some patches of her hair are white, as if powdered with starch. The change is effected in a night; and in a few days after, the hairs gradually regain their dark-brownish colour." (p. 18.)

With regard to the class of nerves through which the formative processes are influenced, Mr. Paget does not think that we have grounds for arriving at a definite opinion. The atrophy of muscles whose motor nerves are paralysed, is not to be regarded as a direct result of the paralysis, since it is immediately due to the want of functional activity in the organs themselves; and as the energy of circulation in a part is in some measure dependent on its muscular action, the paralysis of its motor nerves may thus indirectly bring about a general atrophy. This seems a sufficient explanation of the results of paralysis of the motor nerves; but there are other facts which show that a more potent and direct influence is exerted through the sensory nerves. This is especially the case with regard to the trigeminus; the division of which, without any injury to the motor nerves of the face or eye, speedily exerts a very serious influence on the nutrition of the tissues, the eye of the rabbit undergoing complete disorganization in the course of three days. That these effects are rather to be attributed to the loss of the influence of the sympathetic system than to that of the cerebro-spinal, would appear from the fact noticed by Magendie and Longet, that the destructive inflammation of the eye ensues more quickly after division of the trigeminal nerve in front of the Gasserian ganglion, than when the division is made between the ganglion and the brain;—the sympathetic filaments which exist largely in this nerve being interrupted in their course to the tissues in the former case, but not in the latter. And this inference would be supported by the fact, that increased secretion of tears and mucus from the eye, and increased redness of the conjunctiva, are ordinary consequences of the extirpation of the superior cervical ganglion of the sympathetic in dogs. And it harmonizes well with this view, that the atrophy of parts supplied by the spinal nerves is much greater when the sensory as well as the motor roots are involved, than when the latter alone are paralysed; for it is through the former that the sympathetic fibres become incorporated with the cerebro-spinal nerves. But it is to be remembered on the other hand, as Mr. Paget justly remarks, that defective nutrition is a marked result of injuries of the spinal cord, whilst the sympathetic centres remain uninjured; and that general atrophy is a frequent consequence of diseases of the brain. Fresh evidence is much wanting before a definite judgment can be pronounced upon this point; and we commend the subject to the particular attention of our readers.

4. The *healthy state of the part to be nourished* constitutes the last of the conditions enumerated by Mr. Paget as requisite for the due

performance of the nutritive operations. "This is, indeed," he remarks, "involved in the very idea of assimilation, wherein the materials are supposed to be made like to the structures in which they are deposited; for unless the type be good, the antitype cannot be." The precision with which the new-formed blood and tissues take the likeness of the old ones in all their peculiarities, whether structural, chemical, or vital, whether normal or abnormal, is one of those marvels with which the world of organization is crowded, and upon the abstract cause of which we can only speculate vaguely. But as a means for advancing our knowledge on this subject, it is desirable to bring together and classify the phenomena in question; and Mr. Paget has done this in a mode so valuable and suggestive, that we shall make no apology for again following him somewhat in detail. He first notices the proofs of the exactness of those processes by which the continual regeneration of the tissues is effected, that are afforded by the constant preservation of their peculiarities of structure and action through the whole of life; and not only of those peculiarities which belong to the tissue or organ in general, but also of those which form the proper features and indicate the temperament of the individual. The same exactness is shown in the perpetuation of structures which were originally the results of disordered action, such as the cicatrices of wounds, which, when once completely formed in childhood, commonly grow at the same rate with the body, and may be preserved through the whole of life. "It is not that an unhealthy process continues; the fact is the result of the process of exact assimilation operating in a part of which the structure has been changed; the same process which once preserved the healthy state maintains now the diseased one." But this persistence, although the general and larger rule, is sometimes modified by a tendency to revert to the original condition, especially during the period of growth, "a looking back, as if with longing, after the old perfection." Hence old cicatrices may gradually disappear, and thickenings and indurations of parts may give way. To these familiar facts Mr. Paget has given a higher significance.

> "What we see in scars and thickenings of parts appears to be only an example of a very large class of cases; for the exactness by which the process of assimilation in a part maintains the change once produced by disease, offers a reasonable explanation of the fact that certain diseases usually occur only once in the same body. The poison of smallpox, or of scarlet fever, for example, being once inserted, soon, by its multiplication or otherwise, affects the whole of the blood; alters its former composition; then the disease, in a definite form and order, pursues its course; and finally the blood recovers, to all appearance, its former state. Yet it is not as it was; for now the same material, the same variolous poison, will not produce the same effect upon it; and the alteration thus made in the blood or the tissues is made once for all; for commonly, through all after life, assimilation never deviates from the altered type, but reproduces particles exactly like those altered by the disease; the new ones, therefore, like the old, are incapable of alteration by the same poison, and the individual is safe from the danger of infection.
>
> "So it must be, I think, with all diseases which, as a general rule, attack the body only once. The most remarkable instance, perhaps, is that of the vaccine virus. Inserted once in almost infinitely small quantity: yet, by multiplying itself, or otherwise affecting all the blood, it may alter it once for all. For unsearchable as the changes it effects may be; inconceivably minute as the difference must be between the blood before and the blood after vaccination; yet, in many

instances, that difference is perpetuated; in nearly all it is long retained; for, by assimilation, the altered model is precisely imitated, and all the blood thereafter formed is insusceptible of the action of the vaccine matters." (p. 20.)

And this view is not disproved by the well-known fact, that diseases which usually occur but once in the same body sometimes occur twice or more. On the contrary, the exception helps to prove the rule: for it indicates that tendency to a return to the original state, of which another manifestation has been noticed in the disappearance of a cicatrix.

But in another set of diseases we see an opposite, yet not a contradictory result. In these, as, for example, common inflammation of a part, and still more, gouty inflammation,—the occurrence of disease in a structure produces increased, instead of diminished, liability to the same morbid action; and this liability increases with every recurrence, although in the intervals between the successive attacks the part may have appeared quite healthy. Here, too, we find a tendency to a perpetuation of the new form of the nutritive process; and here, too, we occasionally observe a tendency towards a return to the original perfection, in the cessation of this liability even after it may have existed for a long period.

It is not difficult to harmonize these two classes of phenomena, which, at first sight, appear to be antipodal; for they are both results of the same general principle, "that an alteration once produced in a tissue, whether by external influence or by morbid material in the blood, is likely to be perpetuated by the exactness of assimilation, i. e., by the constant reproduction of parts in every respect precisely like their immediate predecessors." Thus, in the case of diseases usually occurring but once in life, the original character of the system is the liability to suffer them; but their occurrence produces a change that removes that liability for the future, unless it should again return as a consequence of the tendency to a gradual reversion to the pristine state. On the other hand, chronic inveterate diseases, such as syphilis, chronic lepra, eczema, gout, and many more, seem to be perpetuated by the very same law acting after a different manner.

> "In some form or other, and in ever varying quantity, whether it manifests itself externally or not, the material on which they depend is still in the blood; because by assimilation the blood constantly makes it afresh out of the materials that are added to it, let those materials be almost what they may. The tissues once affected may (and often do) in these cases recover; they may seem even to have regained their right or perfect composition; but the blood, by assimilation, still retains its taint, though it may not have in it one of the particles on which the taint first passed; and hence, after many years of seeming health, the disease may break out again from the blood, and affect a part which was never before diseased. And this appears to be the natural course of these diseases, unless the morbid material be (as we suppose) decomposed by some specific, or be excreted in the gradual tendency of the blood (like the tissues) to return to its former state; or, finally, be starved (if I may so speak) by the abstraction from the food of all such things as it can possibly be made from." (p. 20.)

The surpassing precision of the nutritive process which is exemplified in all these phenomena, as in those of symmetrical disease,—"a precision so exact, that, as we may say, a mark once made upon a particle of blood or tissue, is not for years effaced from its successors,"—is, as Mr. Paget justly remarks, a truth of very wide application; and he thus applies it to the solution of the difficulty felt by many with regard to the connexion of an immaterial mind with the brain. The idea is not new, but it is expressed with peculiar felicity.

> "When the brain is said to be essential, as the organ or instrument of the mind in its relations with the external world, not only to the perception of sensations, but to the subsequent intellectual acts, and especially to the memory of things which have been the objects of sense,—it is asked, how can you suppose the brain to be the organ of memory, when you suppose its substance to be ever changing? or, how is it that your assumed nutritive change of all the particles of the brain is not as destructive of all memory and knowledge of sensuous things, as the sudden destruction by some great injury is? The answer is, because of the exactness of assimilation: the impression once made upon the brain, whether in perception or intellectual act, is fixed and there retained; because the part, be it what it may, which has been thereby changed, is exactly represented in the part which, in the course of nutrition, succeeds to it. Thus, in the recollection of sensuous things, the mind refers to a brain, in which are retained the effects, or rather the likeness, of changes that past impressions and intellectual acts had made. As, in some way passing for our knowledge, the mind perceived, and took cognisance of, the change made by the first impression of an object acting through the senses on the brain; so afterwards it perceives and recognises the likeness of that change in the parts inserted in the process of nutrition.
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> "Yet here also the law of the tendency to revert to the former condition, or else the law of change with advancing years, may interfere; the impress may be gradually lost or superseded; and the mind—itself in its own immortal nature unchanged, and immutable by anything of earth—no longer finds in the brain the traces of the past." (p. 21.)

III. Having considered the principal conditions requisite for the perfect nutrition of a part, Mr. Paget next goes on to speak of the formative process itself; inquiring, in the first place, what becomes of the old particle—the one for the replacement of which the process of formation is required. In answer he draws a distinction to which we have ourselves always attached considerable importance, between parts that die, and those which only degenerate, when they have finished their course; the former being cast out entire; whilst the latter are disintegrated and dissolved, and then absorbed.

> "We seem to have a good example of this difference in the fangs of the two sets of teeth; those of the deciduous ones degenerate, are decomposed so as to become soluble, and are absorbed; those of what are called permanent,—more properly, those of teeth which are not to be succeeded by others deriving germs from themselves—die, and are cast out entire. And we may probably hold it is very widely true that, as Mr. Hunter was aware, living parts alone are absorbed in the tissues; dead parts, it is most probable, however small, must be separated and cast out." (p. 21.)

This distinction may probably be carried further; and will be found to exist between the simple and constant degeneration which takes place in all the tissues of the body, at rates of rapidity varying with their respective properties, and the death of the muscular and nervous tissues, which seems to be a necessary consequence of the excitement of their functional activity. Mr. Paget infers that the products formed by spontaneous decomposition in the inactive state may be different from those which result from deterioration by exercise, from the circumstance that the repair of the deterioration in the latter case is much more complete than in the former. To ourselves it has always seemed probable that, in the former case, the materials of the tissues may be reabsorbed before they undergo any destructive change; and being introduced into the current of the circulation, may be again subjected to the assimilating processes, in the same manner as nutrient matter directly introduced from without. And this reabsorption may be regarded as the main purpose of the lymphatic system; which is now almost universally admitted not to be, as Hunter supposed, the waste-pipe for carrying off the results of decay, but a part of the apparatus by which the nutrient materials are prepared for admission into the circulating current. But on the other hand, the muscular and nervous tissues are subject not merely to this slow decay, but also to peculiar chemical changes which seem to constitute a necessary condition of their functional activity. These changes, consisting chiefly in the union of oxygen with their components, necessarily involve the complete and entire death of the tissues, and the resolution of their proximate elements into new compounds, which, being totally unfit for the nourishment of the body, and injurious if retained within it, are destined for immediate excretion, and are carried off by the circulating current to the organs provided for their elimination.

With regard to the formative portion of the process,—that by which the old particle, however disposed of, is to be replaced,—it is probably always a process of development; resembling in all its essential particulars that which took place in the first production of the entire tissue in the embryo. We see this evidently in the nutrition of parts whose replacement we can watch, such as the epidermic tissues; and we seem to have a right to infer it as to other parts, from the close correspondence between activity of nutrition and abundance of nuclei. The following very important distinction has not, we believe, been formally drawn by any preceding physiologist:

> "As arising from this consideration, I would suggest, for a subject of very interesting inquiry, the difference which we may perceive between what may be called nutritive *reproduction* and nutritive *repetition*. I may illustrate my meaning by reference again to the teeth. In our own case, as the deciduous tooth is being developed, a part of its productive capsule is detached, and serves as a germ for the formation of the second tooth; in which second tooth, therefore, the first may be said to be reproduced, in the same sense as that in which we speak of the organs by which new individuals are formed, as the reproductive organs. But in the shark's jaws, in which we see row after row of teeth succeeding each other, the row behind is not formed of germs derived from the row before: the front row is simply repeated in the second one, the second in the third, and so on.
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> "So, in cuticle, the deepest layer of epidermic cells derives no germs from the layer above them; their development is not like a reproduction of the cells that have gone on towards the surface before them; it is only a repetition.
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> "Probably we shall find hereafter an analogy in this respect between tissues and whole animals; and that, as in the latter, the capacity of regeneration of lost parts is in direct proportion to the degree in which the members of the body are only repetitions one of another, so in the tissues much of the difference in the degree of repair they severally undergo after injuries or diseases, is connected with the ordinary mode of nutrition by repetition or by reproduction. When the whole cuticle of a part is removed, it may be formed again by repetition; but when a portion of muscle is removed, its germs are taken with it, and it is not reproduced." (p. 22.)

The next subject discussed is *Hypertrophy;* this term being used in the restricted sense of excessive natural growth, unattended with the formation of any unusual products. Still a distinction is to be observed between the increase of an organ by the uniform *growth* or enlargement of all its tissues, and the increase by the excessive *development* of some one. We have an example of the former in the thickening of the cuticle under pressure; and of the latter in the extraordinary development of muscular fibres in the walls of the gall-bladder or ureter, under the influence of obstruction to the exit of the fluids they contain. The conditions which give rise to hypertrophy may be reduced, in Mr. Paget's opinion, to three; namely,—1. The increased exercise of a part in its healthy functions.—2. An increased afflux of healthy blood.—3. An increased accumulation in the blood of the particular materials which any part appropriates in its nutrition or in secretion.

1. Of the hypertrophy which results from increased exercise of a part, we have no better examples than those which are furnished by the muscular system; and it is interesting to observe that this takes place to a greater extent in the muscular structures connected with organic life, than in those which are subjected to the influence of the will. We have a very striking example of this in the enormous thickening so frequently found in the muscular coat of the urinary bladder, when there has been any obstruction to the passage of the urine through the urethra. And the explanation was most appositely and exactly given by John Hunter, who attributed it to the constant striving of the involuntary muscle to overcome the resistance; whereas in the voluntary muscles there is not that continued exercise, "because the will can stoop whenever the muscles cannot follow."

2. Although the increased afflux of blood to an hypertrophied part seems to be generally the *consequence* of its increased growth, yet there appears sufficient evidence that the increased afflux of blood may occasionally be the *cause* of its enlargement. Thus increased growth of hair is occasionally noticed as a result of long-continued determination of blood to a part; and the transplantation of the spur of a cock, from the leg to the highly-vascular comb, is followed by an extraordinarily rapid growth in that organ. Mr. Paget adduces a number of curious cases which show that increased determination of blood to a bone, in consequence of necrosis of one spot of it, may give rise, especially in young persons, to hypertrophy of the entire bone. This hypertrophy will tend to produce an elongation; and thus one femur may permanently become an inch or two longer than the other. Or if the elongation be obstructed, as in the case of the tibia through its being bound down at its extremities by the fibula, the bone may be thrown into a curve. This is probably the explanation of many of those curvatures of the tibia, which have been set down to rickets, but which are to be distinguished by the elongation of the bone. In a specimen in the Museum of St. Bartholomew's Hospital, in which the fibula, and the healthy tibia of the other side, are preserved along with the diseased tibia, the latter was found to measure two inches longer than the former, measuring over the curve. Sometimes in young persons this elongation results from the increased afflux of blood, produced simply by a chronic ulcer of the superjacent integuments. Other cases of hypertrophy of bone are mentioned by Mr. Paget, which do not seem so clearly referable to either of the preceding conditions, although the purpose which the process is destined to answer is obvious enough. One of the most curious examples is the concentric hypertrophy of the skull,

which follows diminution in the bulk of the brain; the skull continuing to adapt itself to the form and size of the brain, or rather of its membranes, only without any indication in its external form and dimensions of the change going on within.

> "The thickening of the skull is effected by the gradual remodelling of the inner table and diploë of the bones of the vault; so that, although the exterior of the skull may retain its natural form and size, the inner table grows more and more inwards, as if sinking towards the retiring and shrinking brain; not itself thickening, but simply removing from the outer table, and leaving a wider space filled with healthy diploë.
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> "Again, it is a fact of singular interest, that this thickening, this hypertrophy of the skull, most commonly, if not always, takes place especially, and to a greater extent than elsewhere, in the parts of the bones at and about which ossification commenced in the fœtal state; as if some of the potency that of old brought the fœtal membrane of these parts first into the development of bone, were always afterwards concentrated in them; as if the reserve-power of growth had its seat in the same centres, where was formerly the originative power of development. We may find some further, though less sure evidence of the indwelling formative energy of these old centres, in the fact that those diseases of bone which are accompanied with excessive formation, such as morbid thickenings of the skull, and tumours, are in a large majority of cases seated in or near the centres of ossification,—you rarely find them, except at the articular ends, or round the middle of the shaft. The same does not hold of necrosis, rickets, ulceration, or other diseases indicative of depression of the formative power of the bone. . . . . . This abiding power of the centres of ossification is the more remarkable, when we remember that in many cases the thickening of the skull takes place in persons far past the middle period of life; it may happen even in very old age, and may give one more evidence of that precision of assimilation which maintains, throughout life, characteristic distinctions among portions of what we call the same tissue. Let me, however, remark that it is not peculiar to old persons: I believe that at whatever age, after the complete closure of the cranial sutures, shrinking of the brain may happen, this hypertrophy of the skull may be its consequence. I lately examined a remarkable case, showing the same conditions in a person less than 30 years old, in whom the thickening of the skull must have begun in early life. The case was that of a lady of remarkable personal attractions, but of slenderly-developed intellect, whose head did not, externally, appear below the average female size. Yet her cranial cavity was singularly contracted; the skull had adapted itself to an imperfectly-grown brain, by the hypertrophy of its diploë, which was nearly half an inch thick at and near the centres of ossification of the frontal and parietal bones." (p. 29.)

3. The third cause of hypertrophy,—namely, the accumulation in the blood of the particular materials which any part appropriates in its nutrition or in secretion,—has already been adverted to, and illustrated by the well-known fact of the increase in size of one kidney, when the other is incapable of performing its function. We have another example of it in the formation of adipose tissue, which is unquestionably promoted by the ingestion of fat-producing articles of food. Something else than oily matter, however, must be requisite for this development; for we find that some individuals, like particular breeds of cattle, "fatten" much better than others upon the same diet; and there are some who cannot be fattened in any way, the ingestion of oily matter rendering them "bilious," apparently because their adipose tissue has not the power of increasing its amount beyond very narrow limits, which causes the superfluity to

accumulate in the blood, instead of being eliminated from it by this nutritive operation.

In the last place, Mr. Paget discusses the subject of *Atrophy* in its various forms. This affection is, of course, the converse of Hypertrophy; and as the application of the latter term was limited to the cases in which there is simple increase of nutrition, or the higher development of a normal tissue, so should Atrophy be restricted to that process "by which a part either simply wastes and is reduced in size, with little or no change of texture, or else gradually and regularly degenerates into a tissue naturally existing in some other part of the body—as the fibro-cellular or the adipose." Mr. Paget argues at some length that what is commonly termed "fatty degeneration" should be ranked as a form of atrophy; and this chiefly on the following ground:—1. The frequent coincidence of fatty degeneration with emaciation or diminution of size of the part.—2. The existence of fatty degeneration under circumstances which in other instances give rise to simple wasting of the same part.—3. The frequent concurrence of fatty degeneration with senile atrophy. Almost all old people, he remarks, are either decidedly lean or decidedly fat; and the strongly-marked differences to be observed in asylums for the aged, where a large number of individuals are to be observed together under similar circumstances, are thus graphically described by him:

> "Some people, as they grow old, seem only to wither and dry up—sharp-featured, shrivelled, spinous old folks, yet withal wiry and tough, clinging to life, and letting death have them, as it were, by small instalments slowly paid. Such are the 'lean and slippered pantaloons,' and their 'shrunk shanks' declare the pervading atrophy. Others—women more than men—as old and ill-nourished as these, yet make a far different appearance. With these the first sign of old age is that they grow fat; and this abides with them till, it may be, in a last illness sharper than old age, they are robbed even of their fat. These, too, when old agt sets in, become pursy, short-winded, pot-bellied, pale, and flabby; their skin hangs, not in wrinkles, but in rolls; and their voice, instead of rising 'towards childish treble,' becomes gruff and husky.
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> "Now these classes of old persons may represent the two forms of atrophy,—of that atrophy by decrease, and that by degeneration, of tissue,—to which we shall find nearly every part of the body liable. In those of the first class you find all the tissues healthy, hardly altered from the time of vigour. I examined the muscles of such an one lately,—a woman, 76 years old, very lean, emaciated, and shrivelled. The fibres were rather soft, yet nearly as ruddy, and as strongly marked as those of a vigorous man; her skin, too, was tough and dry; her bones slender indeed, but hard and clean; her defect was a simple defect of quantity. But in those that grow fat as they grow old, you find, in all the tissues alike, bulk with imperfect texture; fat laid between, and even within, the muscular fibres; fat about the heart, the kidneys, and all the vessels; and the bones so greasy that no art can clean them: the defect of all these is the defect of quality." (p. 36.)

Fully adopting the views of those who regard the nucleus as the active agent in the changes which the cell effects, whether these be of the nature of chemical transformation, as in the operations of secretion, &c., or consist chiefly in the development of new cells in its interior, as in the production of muscular fibrillæ, Mr. Paget adduces fresh evidence in their support from his own observations on atrophied parts; for he states that "in those changes in which, by all their conditions, defective nutrition is most evidently indicated, the nucleus is absent or imperfect." That this should be especially the case with regard to fatty degeneration, affords additional evidence of its atrophous nature; and Mr. Paget has so often observed this change in fatty degenerations of the liver, kidney, and muscles, that he feels entitled to assert as a general fact that—when the accumulation of fat exceeds a very small amount, the nucleus of the cell, or other elementary structure containing it, is pale and indistinct; and when the fat is abundantly collected, it disappears completely. His account of the change in the condition of the muscular fibres of the heart is especially interesting.

> "The result of immersing the healthy fibres of the heart in acetic acid is, that they always display a succession of nuclei at nearly equal distances from each other, usually lying in the middle of the presenting surface of the fibre. Such nuclei are, so far as I know, peculiar to the heart-fibres; they are large, reddish-yellow, like blood-globules, especially when the heart is strongly developed; their form is elongated, oval or nearly parallelogram, and at each of their ends one almost always sees tapering groups of isolated, small, yellowish granules, like particles separated and departing from the nucleus. But in the degenerated fibres—when the change is least marked, and but little fatty matter has collected within the sarcolemma—the outlines of the nuclei look dim, and they lose their colour. In a further advanced stage, the nucleus of the fibre cannot be seen at all; its former place is indicated, if at all, only by some out of the narrow column of yellow granules; and in a yet later stage, or when the sarcolemma appears nearly full of fatty particles, all trace of both the nucleus and these granules is lost." (p. 36.)

Mr. Paget does not regard the fatty matter, in such cases, in the light of a new deposit, but as one of the products of the spontaneous transformation of the tissues at the end of their period of vigorous existence; so that this form of atrophy only represents the state of a tissue remaining unrepaired after it has fallen into the ordinary course of degeneration. He quotes the observation of Wurtz, respecting the formation of butyric acid during the decomposition of fibrine in the open air, in proof of the possibility of fatty matter being formed by the transformation even of proteine-compounds in the living body; to ourselves, however, the well-established fact of the conversion of nearly all the soft tissues of bodies buried under peculiar circumstances, into adipocire, is a much more satisfactory argument to the same effect. That the fat which we find in the muscles and gland-cells is really not a deposit put into them from without, but one of the products of the change in their own contents, is further made probable, as Mr. Paget justly remarks, by the frequency with which, in muscular fibres, we find the fat-particles arranged in the same manner as the proper constituents of the fibrils,—sometimes in transverse, sometimes in longitudinal rows. Indeed, he says "one is constantly tempted, in the examination of these specimens, to think that we can trace all the transitions from the 'sarcous elements' of the muscular fibre, and the 'granules' of the gland-cell, to the little oily particles, which, by clustering and then fusing with others, at length make the great oil-globules which fill the cell." This view of the phenomena presented in the fatty degeneration of the normal tissues, is fully confirmed by the corresponding changes taking place in abnormal products; of these Rokitansky has enumerated eleven classes of instances, in which proteine-compounds are replaced by fatty matter in such conditions, that it is hardly possible to assume anything but that the fat is one of the products of the spontaneous transformation of the higher compounds. Mr. Paget enters minutely into the various forms of fatty degeneration of the heart; and he describes one form not previously recognised, under the name of "granular degeneration" of its muscular tissue. In this there is neither any evident deposition of fat, nor is there that mottled appearance which indicates complete local fatty degeneration in spots or lines of the muscular tissue.

> "The whole organ has its natural shape, size, and general external appearance; but it feels soft, doughy, inelastic, unresisting, and may be moulded and doubled up like a heart beginning to decompose long after death; it seems never to have been in the state of *rigor mortis*. These appearances are more manifest when a section is made through the wall of the left ventricle. Then if the wall be only partly cut through, the rest of it may be very easily torn, as if with separation of fibres that only stick together: and the cut surface of the wall looks, as it were, lobulated and granular, almost like a piece of soft conglomerate gland,—an appearance which is yet more striking when observed with a simple lens of about half an inch focus. In colour, it has not on its surface, much less on its section, the full ruddy brown of healthy heart, a colour approaching that of the strong voluntary muscle; but is, for the most part, of a duller, dirtier, lighter brown, in some parts gradually blending with irregular marks or blotches of a paler fawn colour." (p. 47.)

When microscopically examined, the fibres are found to show a remarkable tendency to transverse cleavage; very minute oil-particles are seen in their interior; and no nuclei are found lying among them. In three cases in which Mr. Paget has observed this form of fatty degeneration, death was very sudden, and was not preceded by any symptoms indicative of imperfect action of the heart. It may proceed silently and gradually, undermining the power of the entire heart, yet not rendering itself apparent by any functional change during the ordinary events of calm and quiet life; but the individuals thus affected are wholly unable to resist the storm of a sickness, or the shock of an accident or an operation. It is very probable that many cases of sudden death, not otherwise accounted for, have been due to this cause; for a structural change of this kind may have advanced so far as to be sufficient to destroy life, without being perceptible to any but a very careful and practised examiner. "Let it not be said," observes Mr. Paget, in concluding his account of this transformation, "that one learns little in learning too late the existence of an incurable disease; for, beyond doubt, very often the death that has come from such a disease as this has been ascribed to a wrong cause, and has spoiled confidence in good men and their good measures."

Pursuing his inquiries with regard to fatty degeneration into other channels, Mr. Paget has satisfied himself that the disease which most English writers have described as *mollities ossium*, is in reality a fatty degeneration of the bones. In no case has he been able to obtain any evidence of the correctness of the general notion of the nature of the disease, as consisting in the removal of the earthy matter of bone, and the reduction of the skeleton, wholly or in part, to its cartilaginous basis. And in all which have been particularly described in this country, or which have fallen under Mr. Paget's own notice, softness and brittleness of the bones, with the presence of a large quantity of adipose matter apparently resulting from the conversion of the cartilaginous basis into fat, have been the prominent features of the structural change, rather than that flexibility and tenacity which would exhibit themselves, if the calcareous matter were deficient and the cartilaginous basis retained its normal state. Some of the cases described by Rokitansky, however, and others lately put on record by Dr. Bence Jones and Mr. Dalrymple, appear to coincide in their characters with the ordinary notion of the disease.

In the last place, Mr. Paget refers to the fatty degeneration of the liver, the peculiar elementary characters of which were first detected by Mr. Bowman; and he gives the following reasons for not assenting to the ordinary idea, that the fatty liver is one actively discharging its office as vicarious to the lungs, secreting the hydrogen and carbon which the lungs, by reason of some defect in their structure, cannot discharge.—1. The connexion between fatty liver and disease of the lungs is not general. —2. There is no evidence derived from examination of the fæces. that the fatty liver does secrete an unusual quantity of carbon and hydrogen. —3. If the carbon and hydrogen, supposed to be formed in extra quantity in the liver, be not in the fæces, then the lungs would only be damaged, by the excessive formation of those elements in the liver; the function of the liver, in warm-blooded animals after birth, being chiefly preparatory to that of the lungs.—And 4. All the conditions of the fatty liver show that it is an inactive organ, one which is discharging less than its ordinary function, and the less, the more general the fatty degeneration of its cells. This is indicated by the analogy of all fatty degenerations, the absence of nuclei in the fatty cells, the absence of all appearance of the colouring matter of the bile in them, the large size of the liver (indicating a tardy or obstructed removal of its cells), the paleness and defective supply of blood, and the frequent coincidence of other morbid changes, such as would naturally hinder the proper activity of the organ. The occurrence of a corresponding structural change in the kidney, too, without any excretion of fatty matter, and with a diminished excretion of the proper materials of the urine, is to our minds an additional argument, whose analogical importance is not trifling. Altogether we feel no doubt that Mr. Paget's view is the correct one; and that the idea against which he argues, however plausible at first sight, will not bear a close examination.

Having purposely brought prominently forwards those portions of Mr. Paget's Lectures, which seem to us to exhibit a decided advance in physiological and pathological science, or to contain the germs of future progress, we have left ourselves no room to speak of a variety of other topics which he has handled in a scarcely less original or interesting manner; and the exhaustion of our space requires that we should pass over all his observations on the ordinary forms of atrophy. We shall conclude with a valuable practical suggestion offered by him as arising out of the fact, ascertained by the experiments of Dr. John Reid, that the atrophy of paralysed muscles is due to their disuse, and that if they can be artificially stimulated to action their nutrition is sustained.

> "When muscles are paralysed through affection of the nervous system, we ought to give them artificial exercise; they should be often put in action by electricity or otherwise; their action, though thus artificial, will ensure their nutrition; and then, when the nervous system recovers, they may be in a condition to act with it. In one case in which I could act on this suggestion, the result was encouraging. A little girl, about eight years old, had angular curvature
> lete loss of voluntary movement in the lower extremities. This had
> weeks; but as I found she had reflex movements, the legs twitching

in a very disorderly way as often as the soles were touched, I advised that the limbs should be put in active exercise for about an hour, two or three times a day, by tickling the feet, or in some similar way. The result was, that when, several weeks afterwards, the spinal cord recovered, and she could again direct the effort of her will to the lower limbs, the recovery of the strength was speedy and complete; more so, I think, than if, in the paralysed condition, the muscles had been left to the progress of the atrophy." (p. 44.)

We think this suggestion peculiarly worthy of attention in cases of hysterical paralysis, in which the condition of the nervous system suspends for a time its functional influence over the muscular parts, and in which there is every reason to hope that this influence will, some time or other, be restored. Where such paralysis has long existed, the muscles commonly become so atrophied, that the recovery of their power, when the disorder of the nervous system has been removed, is frequently the most tedious part of the curative process. In a case of hysteric paraplegia, which fell under our notice some time since, one of the legs was frequently affected by violent cramps, from which the other remained free; and the former measured fully an inch more in circumference than the latter. But with the recovery of voluntary power, the cessation of the cramps, and the consequent equalization of the muscular exercise of the two limbs, that which was previously the smaller gradually attained the size of the other. Thus, whether naturally or artificially induced, the exercise of paralysed muscles sustains their nutrition.

We must now take leave of Mr. Paget, cordially thanking him for the instruction which he has imparted to us, and earnestly expressing the hope that it will not be long before he gives us another opportunity of profiting by the rich stores of scientific information in his possession, and by his philosophic sagacity in the use and disposition of them.

Art. VII.

1. *A System of Surgery.* By J. M. Chelius, Doctor in Medicine and Surgery, Public Professor of General and Ophthalmic Surgery, Director of the Chirurgical and Ophthalmic Clinic in the University of Heidelberg, &c. &c. Translated from the German by John F. South, Professor of Surgery to the Royal College of Surgeons of England, and one of the Surgeons to St. Thomas's Hospital.—*London,* 1847. 2 vols. 8vo, pp. 814, 1009.
2. *A System of Practical Surgery, including all the recent Discoveries and Operations; with Forty-nine Illustrative Plates, from Drawings made expressly for the Work.* By John Lizars, late Professor of Surgery to the Royal College of Surgeons, and Senior Operating Surgeon to the Royal Infirmary of Edinburgh. Second Edition.—*Edinburgh,* 1847. 1 vol. 8vo, pp. 503.

We hailed with pleasure the commencement of Mr. South's translation of our German contemporary's Handbook, and we have followed him with much interest through the different steps of his laborious undertaking. We now cordially congratulate him on the completion of his task, and, as a whole, on the mode in which he has fulfilled it: and we feel assured that he will not quarrel with us for stating our conviction that, with a little more self-confidence, he might have safely ventured on the broad ocean of surgical science and literature on his own account, and without the aid of a German swimming-jacket to float him. That his own resources were equal to this, we think the result has satisfactorily proved.

Mr. South's ambition, however, was not limited to the production of a mere translation of his author, with the addition of such occasional notes as might render the 'Handbook' more acceptable to the English student; but he aimed at the higher object of supplying what he truly observes has been a desideratum for some time past in English surgical literature, a complete 'System of Surgery,' suited to the wants of the practitioner and worthy of the country and language in which it is written. In achieving this end, the character of the work as it first appeared in German, and the purpose for which it was published, have been in a great degree sacrificed; and we scarcely anticipate that the present 'System' will ever become the manual of the student, who, for the most part, prefers more succinct descriptions of disease, and a more dogmatic style. This, however, is not a subject of regret, for there was no lack of 'Outlines' and 'Compendiums,' which fully answer the purpose for which they were written; and we are therefore better pleased to welcome the work in its existing form.

The title-page of Mr. Lizars' volume sets forth the pretensions of the work to be considered a 'System of Practical Surgery,' brought up to the present day; and as such we are bound to accept and criticise it. We regret that its contents fall far short of our anticipations, and that the general character of the work is such as by no means to justify its claim to be regarded as a representation of the existing state of British surgery. Thus much we have thought it our duty to say in the outset, as an apology for not contrasting or collating the two works which stand at the head of the present article: their stamp and general qualities are, in almost every respect, so essentially different, that the task would be both difficult and invidious. Moreover, it will be impracticable to give anything like a satisfactory analysis of the whole of Mr. South's copious work: we shall, therefore, content ourselves with laying before our readers an outline of the contents and arrangement of the volumes before us, selecting, in passing, certain divisions or chapters, as subject-matter for especial comment.

To those who are familiar with the Manual of Chelius in the original, the comparative bulk of Mr. South's translation will at once convey a just impression of the copious introduction of new matter into the English version. The plan adopted is this. The text of the original is printed in a larger type, with numerical headings to the paragraphs. The introduced matter consists of quotations, all of which have their appropriate references, and of the results of the translator's own experience. This is printed in a smaller type and included between brackets, Mr. South's own opinions being further distinguished by the insertion of his initials at the close of the paragraphs containing them. There are, further, other occasional paragraphs, likewise printed in a smaller type, which constitute part of the original work, where they also appear under the same distinguishing form. These, when they exist, immediately follow the principal text, and are *not* contained between brackets.

The arrangement of the work is based on scientific principles, and its contents are comprised under the following eight divisions:—1, Inflammation; 2, Diseases which result from the disturbance of physical continuity; 3, Diseases dependent on unnatural coherence; 4, Diseases dependent on the presence of foreign bodies; 5, Diseases which consist in the degeneration of organic parts, or in the production of new structures; 6, Loss of organic parts; 7, Superfluity of organic parts; 8, The elementary proceedings of surgical operations. These are preceded by a brief introduction, a historical sketch of surgery, and a copious table of its general literature.

The order of subjects treated of by Mr. Lizars is different from the above; this author preferring to discuss successively the diseases of various organs. Thus, the diseases of the blood-vessels, of the bones, of the joints, of the glands, of the nervous system, &c., constitute so many different sections of the work; and other chapters are devoted to wounds in general, and to the injuries and diseases of certain regions. This classification possesses some advantages, and is, perhaps, more convenient for students, though all pretensions to scientific arrangement must thereby be sacrificed.

The comparatively short introductory chapter of Chelius on Inflammation has been much expanded by his translator, who very laudably stands forth as the champion of our immortal Hunter. We say laudably, because we feel and believe, as it is clear Mr. South does, that justice has not been done to the labours of our countryman, who was unquestionably far before his age in the preciseness and originality of his views on inflammation; much that has been put forth as new of late years having been really anticipated, or at least the right road to its discovery pointed out by him. Much laborious research and talent are evinced in the compilation of this very interesting division of the work. It presents us with an excellent digest of what has been done, and all that is now known in this fundamental branch of the *science* of surgery; and each contributor has his due meed of praise ascribed to him for what he has done, including Andral, Müller, Henle, Travers, John Thomson, Wharton Jones, Wilson Philip, &c. It would be beyond the scope of the present article to give an analysis of this subject, though we cannot pass it by without giving it our unqualified approbation.

The same remarks apply to the terminations of inflammation, which are likewise treated of generally, before the variations of inflammation come under notice. In this latter division of the present section, the author divides his subject into *idiopathic*, *symptomatic*, *specific*, and *sympathetic*. The following definition of these various forms of inflammation may serve as a specimen of our German author's succinct style:

> "*Idiopathic inflammation* is the consequence of external violence; it exists as a local disease, and its severity is regulated by the degree of the injury and the condition of the subject. *Symptomatic inflammation*, at least the definite form under which it first appears, depends on internal causes, and the inflammation itself is to be considered only as a reflection of the general disease. If this be of a specific nature, as syphilis, and so on, the inflammation is said to be *specific*. *Sympathetic inflammation* is the consequence of a consensual change in the mutual relations which one part holds to another, by which their diseased affections become shared by both. The *metastatic inflammation*, which passes from one organ to another, is in close connexion with the sympathetic." (p. 71.)

Inflammation of various tissues is then discussed, and Mr. South finds ample opportunitiy of doing justice to other authors, besides introducing some valuable practical hints from his own experience. He objects, and we think correctly, to the subordinate place assigned to that important affection of the cellular tissue, usually terminating in diffused suppuration or gangrene. We further entirely concur in the opinion of its essentially idiopathic character in a large majority of cases; and, though allied to erysipelas, it possesses distinguishing characteristics, which entitle it to an independent notice. When on the subject of arteritis, Mr. South quotes an interesting case, under his own care in St. Thomas's Hospital, the history of which disproves the opinion of Dupuytren and others, that the coagulation of blood in the inflamed vessel necessarily causes mortification of the limb. The patient, who was the subject of this affection, was a young man, and the inflammatory attack seemed to be independent of any local injury; he died within three months, and it was found that "the brachial artery, high up on the arm, over a space of from half an inch to an inch, was of a red colour, and its coats thickened, containing at this point a plug of coagulable lymph, adherent on one side to the lining of the vessel. Below this part the vessel was much contracted, to the extent of three or four inches, beyond which it again resumed its natural calibre, and there the orifices of three or four minute vessels were perceived."

When speaking of the treatment of abscess, Mr. South has some judicious observations, which we recommend to the notice of students and heavy-handed practitioners, especially the caution not to handle roughly, nor press and squeeze a newly-opened abscess. This is rarely admissible; for, in addition to what our translator has pointed out as prejudicial in adopting such a plan, the emptied sac cannot at once contract, and usually will not collapse, so that air rushes in to fill the vacuum, producing decomposition of the remaining contents, and creating very great disturbance and serious irritation. Mr. South here criticises the treatment of his author, in recommending that abscesses in the neighbourhood of important parts should be opened late, "because in a large collection of pus the elevation of the skin ensures against any injury to deep-seated parts." This, says the translator, is not good practice; "abscesses just beneath the skin should always be punctured early, as otherwise there is a great risk of sloughing of the integument, and the formation of an ugly scar." The now obsolete employment of escharotics and seton, in the opening of abscesses, is deprecated by both.

We trust to be excused for thus adverting to what may appear of comparatively trifling importance in a 'System of Surgery;' but we do not hold with those who measure the importance of an operation or of a disease by its comparative infrequency; and we regard the features we have attempted to illustrate above, as a valuable and an interesting characteristic of the work before us. It is time, however, that we turn to the volume of the Edinburgh professor, not, as we have already said, with a view to institute any comparison between the works, but to give our readers a just estimate of how each has executed his task. This we must unshrinkingly do, however anxious we may feel to deal gently and kindly with all; for it is a grave responsibility we take upon ourselves, under any circumstances, to review a book impartially; but especially is this the case when our opinion is intended to assist the student in the selection of a suitable guide for his studies, and a "vade mecum" to fall back upon during his early practice. From the tenor of this prologue, our readers will naturally conclude that we have found more to criticise than to praise in Mr. Lizars' volume, and such truly is the case; but we are anxious to make no random remarks nor unfounded strictures, but to let our author speak for himself, whilst we point out what, in our opinion, are the errors and omissions, not forgetting the more meritorious parts, of his composition.

When a work has passed through one edition, and a second is called for, the critic requires some firmness, if not boldness, to assail what received the sanction of the public; but the duty is the more imperative where some error and much evidence of negligence characterize a work, as in the case with that before us. One marked feature in Mr. Lizars' volume is the introduction of numerous graphic illustrations, an aid to instruction which has now become very general,—in our opinion, even to a prejudicial extent. Many of the plates before us are very good, especially such as are designed to illustrate that which (if we may use the expression in a somewhat restricted sense) is purely mechanical; such as the mode of reducing dislocations, of applying fracture apparatus, &c.; but many are deceptive and useless, and some even absurd. We shall have to refer to these again as we proceed.

The volume commences, of course, with a chapter on "Inflammation," which Mr. Lizars defines as "an increased action of the extreme nerves and capillary blood-vessels of the part affected, in which the lymphatics are often involved. The nerves are first thrown into action, which instantly excite the contiguous arteries and veins, and, as inflammation increases, the excitement extends to the brain and heart." This is rather a summary, not to say obsolete, introduction to this all-important groundwork of surgical science; but we proceed. Our author then affirms that the pain and heat attending inflammation are "more dependent on nervous than arterial action," the proofs being, that, in trifling inflammation these conditions are slightly developed, and the converse. Moreover, that the sensation of *heat* is intolerable when a severe blow has been received, i. e. "at the moment of the injury; and not until the inflammation has lasted for some time, does the increased quantity of blood contribute to form the increased heat." Surely Mr. Lizars must have strangely neglected to keep pace with the precise physiology and pathology of the present day, in thus adhering to the vague and illogical language of bygone times. What does "nervous action" mean, and how does the sensation of heat under the infliction of an injury prove that the temperature of the injured part is really raised? No one doubts that all sensations are communicated to the brain through the medium of the nerves, and this is all that our author's dogmas prove, whilst he leaves the question of how increased temperature (which is a very deceptive condition, by the way) and pain are produced, quite untouched. But we will go on a little further, and allow Mr. Lizars to speak for himself:

> "The redness . . . . is caused by the arteries becoming enlarged in their calibre, and conveying more red blood than usual; and also by those vessels, which previously conducted pale blood or lymph, becoming enlarged and transmitting coloured blood. The capillaries and communicating vessels, and even

the commencements of the veins themselves, are similarly dilated. The blood flows with increased velocity, owing to that part of the artery which is immediately contiguous, or leading to the inflamed or dilated portion, being morbidly excited.

"The artery of an inflamed part, therefore, is at first smaller than during health, in consequence of the spasmodic action of the nerves and the distension of the vasa vasorum diminishing the calibre of the vessel; but it very soon becomes enlarged and over-distended with blood, from the nerves being partially exhausted, while the portion leading to the inflamed part is contracted in diameter; and if the inflammatory action has been of any continuance, the coats of the artery become inflamed, and the tunica intima and arterial tissue studded with vasa vasorum, approach to ramollissement. The blood circulating in these inflamed capillaries is of a deeper colour, and contains small, irregular flocculi." (p. 18.)

What are the capillaries and what the communicating vessels according to our author? Is Mr. Lizars sure that the blood flows with increased velocity through arteries immediately contiguous to inflamed parts? Is he satisfied that they are ever diminished in calibre? and what does he mean by the "spasmodic action of nerves," and their subsequent "partial exhaustion?" But we will not pursue this inquiry further, though the reader may be surprised that it is barely the first page and a half of the text that have afforded us the material for this criticism. The author's division of inflammation, and his treatment of its causes, terminations, &c., present much that is faulty and negligent; but we will give him the opportunity of speaking for himself anon as to subjects of a more exclusively surgical nature; and we will dismiss this, his first chapter, viz., that on inflammation, by noticing that it includes, besides what we have alluded to, the operations of arteriotomy and phlebotomy, inflammation of certain textures, as fascia, veins, lymphatics; the various forms of ulcer, tinea, porrigo, anthrax, whitlow, lumbar abscess, &c., &c.

In the chapter on Burns, which succeeds that on Erysipelas, and wherein the English translation of our German author is much enriched by additional and valuable information, the remedies recommended at different periods, with their advantages and disadvantages, are enumerated. Mr. South himself gives a preference, in simple scald or burn unattended by destruction of true-skin, to the employment of cloths dipped in lime-water and oil for the first forty-eight hours, and the subsequent application of bread-and-*milk* poultices. He speaks favourably also of the use of cotton. In the account of the treatment of scars which follows, he describes a long and severe operation performed by himself for the relief of permanent flexion of the forearm. Mr. South does not seem here to have calculated on the permanent contraction of the muscles, which had thus adapted themselves to the existing condition of the limb; for he had to divide one flexor after the other, and still the result, as regarded immediate relief, was unsatisfactory. A weight was subsequently worn in the hand to keep up extension of the forearm, and the case was ultimately benefited, though we scarcely think that the advantage gained was commensurate with the severity and risk of the operation. In referring to Mr. Lizars' treatment of burns, we find that he recommends that the parts should be swaddled in cotton wadding, even where there is destruction of the true skin involving suppuration and sloughing. In this severer form of burn, the wadding is not to be allowed to remain on more than eight days! The same author advocates the practice of plastic operations for the removal of scars occasioned by burns; this has been very successful in the hands of Dr. Mütter, of Philadelphia, as quoted by Mr. South, and of others.

We must, however, deny ourselves the satisfaction of pursuing a regular analysis of the volumes before us, and adopt the plan we proposed of selecting a subject here and there, by which alone it will be practicable to restrain the present article within even moderate limits; and we will now take a glance at the chapters on the "Diseases and injuries of the blood-vessels."

In the treatise of Chelius, the subject of wounded arteries is discussed under the head of wounds generally; and here again, Hunter, Travers, Lawrence, and others, are laid under contribution, so as to afford an excellent digest of all that the most recent investigations have supplied in elucidation of the modes in which recent wounds are repaired. In speaking of the various methods to be resorted to, in stanching hemorrhage from divided and bleeding arteries, Professor Chelius thus states his opinion respecting direct compression. We give this passage as an illustration of our German author's trite and simple style, and of the manner in which Mr. South has executed his task as a translator:

> "*Immediate or direct compression* consists in laying some charpie, rolled together, or pieces of agaric, sprinkled or moistened with styptic remedies, upon the mouth of the bleeding vessel, and fixing it tightly by a suitable bandage. This kind of blood-stanching is less certain, and, as regards the healing of the wound, very injurious. It must, therefore, be employed only in those cases where tying the bleeding artery is not possible; for instance, if the blood wells up from the whole surface of a wound, in wounds of the meningeal artery, in severe bleeding from the nose, after the operation of cutting for the stone, and so on. This compression most certainly stops the bleeding, when the artery can be compressed against a bone.
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> "*The tying or ligature* (*ligatura*) of a bleeding artery is the most simple, certain, and, in most cases, practicable method of stanching the blood. The operation of the ligature consists in its preventing the current of blood, and, by its irritation, producing inflammation, exudation of plastic lymph, and union of the coats of the artery. This takes place as well when the arterial coats are kept merely in contact by the ligature, as when the inner and middle coats are torn through by means of the tightening, and the small size of the ligature, the outer coat alone remaining undivided." (pp. 301–3.)

In his comments on these passages, Mr. South remarks that it is best not to use styptics as an adjunct to pressure, i. e., as a general rule; and we agree with him, that it is better to trust, where such assistance can be obtained, and is otherwise available, to the finger only; the indiscriminate employment of sponge tents and the like is much to be deprecated. The thorough cleansing of a bleeding surface from all clots, and exposure to the air, constitutes also excellent practice; in addition to which, however, we have found that cold water, squeezed *guttatim* from a sponge upon the oozing surface, has given important aid in procuring that exudation of fibrine which is essential to the closure of the open mouths of the vessels. Of course these remarks apply only to the lesion of very small arteries; and position must be in all cases attended to, so as to facilitate the return of blood to the heart. Further on, Mr. South denounces, and very justly, the mischievous practice of enlarging wounds in the palm of the hand or the sole of the foot, in searching for bleeding vessels,—a proceeding which necessarily enhances the existing difficulty, by laying open fresh vessels, to say nothing of the ulterior evil of leaving a larger space to heal. In these cases we would always recommend the employment of pressure by small compresses of cork or similar material upon the arterial trunks immediately supplying the seat of lesion, as on the radial and ulnar at the wrist, or even on the brachial itself, in bleeding wounds of the palm, before the operation of placing a ligature on either of these vessels is resorted to. Mr. South agrees with Professor Chelius in denying the necessity of dividing the inner and middle coats of an artery, in order to bring about the adhesive process by which obliteration at the seat of ligature is obtained. This we believe is generally admitted, though the experiments of Travers, and even those of Scarpa and Crampton, prove, likewise, the uncertainty of simple compression, as compared with the tight application of a small ligature so as to divide the serous and elastic coats of an artery. Mr. South quotes an interesting experiment of his own, in which the *loose* application of a ligature round the carotid of a dog was sufficient to induce obliteration of the vessel to the extent of two or three inches; this, as he remarks, is no guide for similar practice on the human subject. These questions obtain a more extended notice under the head of Aneurism.

We now turn to the corresponding section in Mr. Lizars' volume, and shall find that his practice does not altogether tally with that which we have commended as judicious in our German author. The chapter on "Diseases of arteries" commences with a brief account of "arteritis;" and the author then proceeds to discuss the subject of wounds of these vessels, and aneurism. In his definition of the latter disease, he refers to the divisions of Scarpa and Guthrie; and his description of this affection is concise, and on the whole good. A large section of the human race is, according to Mr. Lizars, prone to aneurism, as may be inferred from the following categorical arrangement of its victims:

> "It attacks the irritable, the passionate, the gluttonous, the drunken, the debauched, the syphilitic, the mercurial (lively?), the rheumatic. Coachmen, postboys, postilions, soldiers (particularly dragoons), sailors, porters, labourers, and miners are said to be most subject to it." (p. 84.)

The first form of aneurism which Mr. Lizars treats of is that affecting the popliteal artery, and this is accompanied by a description of the operation required for its relief. The directions for finding the femoral artery are tediously minute, commencing with—"the space between the anterior superior spinous process of the os ilium and the spine of the os pubis, is to be divided into ten proportional parts, &c." On this subject (and similar directions occur in reference to the carotid) we have only to remark, that if an operator has not a sufficiently accurate knowledge of anatomy to supersede the necessity for such adventitious aid, he has no business with the knife; arteries are not to be tied as yards of riband are measured. We should also apprehend that it was unnecessary to guard the surgeon, albeit a tyro, against mistaking the saphena vein for the artery he is in search of. The observations which relate to the necessary caution about passing the aneurism needle are judicious; but we dissent from the recommendation to cut off both ends of the ligature close to the knot.

The author then proceeds to the subject of wounded popliteal artery; and before describing the operation for securing the vessel at the seat of injury, recommends that "compression, combined with throwing a ligature round the superficial femoral artery," should be employed as the most appropriate treatment. He then adds, "if the wound be large enough to enable the operator to secure the artery with facility, the patient should be placed on his face," &c., concluding with directions for taking up the vessel at the seat of injury. Now we are aware that some surgeons advocate this practice of securing the femoral artery in wound of the popliteal, though we think the practice decidedly heterodox; but, that the size of the inflicted wound is to determine the adoption of one or other method, and thus decide what must be considered as involving an important principle in practice, is quite new to us. The same directions, involving the same condition and alternative, are given in reference to the posterior tibial artery; we will quote this and the following paragraph, as an illustration of the author's style (not at all an unfavorable one), and the practice he inculcates:

> "When the posterior tibial artery is wounded in the upper or proximal third of the leg, if the wound be small, compression should be applied, and the superficial femoral secured with a ligature; but if the wound be so large as to permit the surgeon to reach the artery easily, let him keep in view the following directions: A very long and deep incision must be made in the central line, between the inner angle of the tibia and the mesial line of the popliteal aspect of the leg, through the integuments, fasciæ, and bellies of the gastrocnemius and soleus muscles, or an incision made between the said central line and the inner angle of the tibia, to avoid injuring the gastrocnemius muscle, which is to be held aside. The deep fascia should be carefully incised, the venæ comites are large, and have numerous inosculations, which obscure the artery; and the nerve lies rather superficial and internal.
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> "The plantar arteries in the foot, like the anterior tibial, are subject chiefly to aneurism by anastomosis. When wounded, they must be cut down upon and secured, however deep, and however entangled with nerves, tendons, and fascia." (p. 93.)

We refrain from any further comment on the former of these paragraphs; the latter enjoins an heroic style of practice which will scarcely bear the test of bedside experience, as we have already had occasion to notice.

In wound of the brachial artery in venesection, Mr. Lizars prefers securing the injured vessel at once at the seat of puncture, of course employing two ligatures; and in this practice we are disposed to concur, for the operation is a simple one, and much more certain as regards the object in view, than the bandaging method, i. e., if the patient is willing to submit. Immediately after giving this advice, the following short and pointed paragraph stands alone: "All wounded arteries ought to be secured at once, and in the same manner." How is this to be reconciled with the preceding advice regarding the popliteal and tibial arteries?

Aneurism and wounds of the axillary, brachial, and carotid arteries, &c., are successively brought under consideration, together with the several appropriate operations for their relief or cure; but the whole occupies a very small space, and then the treatment of nævi is discussed more at length. This latter section includes an enumeration of the methods employed and advocated by different surgeons; but the absence of any very specific directions as to which measures are to be preferred in the different forms of this troublesome defect, must leave the student at a loss when seeking for such information in time of need. This section of Mr. Lizars' work closes with a few remarks on transfusion; in which the author remarks that "it is only necessary to inject a few drops of blood to stimulate the heart;" we have seen many drachms unsuccessful. He further thinks the saline solution (used in cholera) preferable to human blood. Before dismissing the present subject, we observe that there are plates introduced illustrative of the operations on the arteries and some of their diseases. One of the former affords a ludicrous example of want of proportion in the drawing, viz., that representing the ligature of the iliac artery, in which that vessel is drawn nearly twice the size of the penis hard by.

We now turn to the more pleasing task of examining cursorily Mr. South's chapter on Aneurism. This disease is treated of in the second volume, and under the head of "Solution of continuity from unnatural extension." The first part contains an interesting and sufficiently copious account (much detail being added by the translator) of the symptoms, distinguishing characters, causes, origin, spontaneous cure, and treatment of the disease. Many authors who have contributed to this branch of surgical pathology are aptly quoted, and some apposite cases are given, including much that is curious and interesting as regards the statistics of the disease. As we have not yet allowed the author to speak for any length through his translator, we shall take the present opportunity, by quoting the paragraph on the symptoms of aneurism:

> "The symptoms which characterize aneurism are the following: A little elastic, pulsating swelling, which diminishes on pressure, and soon returns on its withdrawal, arises at some one spot corresponding to the course of the artery. The pulsation ceases when the artery is compressed between the swelling and the heart, and the former becomes generally less tense. If the artery be compressed below the swelling, the pulsation becomes more bounding and distinct. The tumour is usually free from pain, the skin over it unchanged, and it quickly enlarges to a considerable size. In proportion as the swelling increases, the blood contained in it becomes more solid by coagulating, and can no longer be got rid of by pressure; the pulsation is weaker and often entirely lost. When the tumour has acquired considerable size, it acts injuriously by its pressure and expansion upon the neighbouring parts; the circulation in the diseased artery, in the other blood and lymphatic vessels, is interfered with; the nerves are compressed; the nourishment, warmth, and sensation of the part are diminished; the limb becomes œdematously swelled; the tumour is covered with varicose vessels, and becomes bluish; and the surrounding muscles, and bones even, may be destroyed by the constant pressure and absorption. In consequence of the expansion of the parts covering the swelling, they at last inflame; at the most prominent part an abscess, or commonly a slough, is formed, after the separation of which the coagulated blood is discharged, and a dangerous or fatal bleeding ensues. The coverings of the tumour may be also torn by gradual distension. Its size may even become so great, that by the pressure and destruction of the surrounding parts, the nourishment of the limb may be entirely prevented, and its death caused." (p. 198.)

In the ensuing paragraph we find our German author and his translator at issue as to the several conditions which characterize true and false aneurism, and constitute their distinguishing features; and indeed we think with the latter, that the statement of the professor to the effect that "true aneurism quickly diminishes on the application of pressure, though it reappears almost as soon as the pressure is removed;" whereas "false aneurism only disappears gradually, and returns slowly after pressure has been taken off, because the blood can only gradually pass from the sac into the artery, and from it again into the sac," requires considerable qualification, and may mislead those who depend on this difference or contrast as the means of distinguishing the two forms of disease. The diminution of the bulk of a true aneurism, as Mr. South rightly observes, "depends materially on the stage at which it has arrived," or, in other words, on the accumulation of clot, and consequent density of the sac. It is but justice, however, to Professor Chelius, to add that he does not limit the distinction to this point, but indicates other concurrent signs, which, taken together, are sufficiently diagnostic of the two diseases.

When speaking of the period of life at which aneurism is of most frequent occurrence, Mr. South quotes Sir A. Cooper's experience, who tied the femoral artery successfully for popliteal aneurism in a man of eighty, and mentions a case of aneurism of the anterior tibial artery in a boy of eleven years of age, who was a patient in St. Thomas's Hospital.

Further, Mr. South's experience coincides with that of the surgeon we have just quoted, as well as of Hodgson and Lisfranc, and, in fact, of all surgeons who have had extended opportunities of observing disease, as regards the comparative infrequency in females of aneurism affecting external arteries. A case, cited a little further on, proves the importance of ascertaining the condition of the heart and internal vessels before proceeding to operate for aneurism. The patient was on the operating table, and about to have his femoral artery tied by Sir A. Cooper, when he stretched himself out, gasped, and died. An aneurism of the aorta had burst into the pericardium.

The employment of astringent remedies, and of cold, in the cure of aneurism, are casually mentioned, but not with much confidence in their efficacy; they are, in fact, now nearly abandoned, save as palliatives under certain circumstances. An extended historical notice of the employment of compression then follows, in which want of space will not permit us to follow our authors; we must, however, find room for Professor Chelius' concluding and very judicious advice upon the subject:

> "It (compression) may be employed if the aneurism be still recent, if it be not large, especially when the consequence of an external injury; if there be no circumstances which render a speedy cure necessary; if the patient be not very stout, the limb not very much swollen, and the artery so situated that its walls can be properly brought together by compression. Where beneficial, it soon shows; the experiment of compression is therefore never to be persisted in too long, and it should be left off as soon as circumstances occur which render it dangerous. It is always proper to accompany the pressure with rest, bloodletting, cold applications to the swelling, and the internal use of digitalis, and so on." (p. 213, vol. ii.)

The use of the ligature next comes under consideration, and there is an interesting historical notice accompanying this branch of the subject likewise. This, however, we must pass rapidly by, merely repeating Mr. South's opinion, that a *moderate* degree of tightness, short, indeed, of cutting through the internal coat of artery, is all that is absolutely required to procure obliteration of the calibre of the vessel. As regards the extreme caution which should be exercised so as to avoid disturbing the relations of an artery, we entirely concur with Mr. South; but we think it safer to divide the inner and middle coats of the vessel by the ligature, and this may be done without at all endangering the integrity of the cellular coat, which, when healthy, is capable, from its toughness, of resisting any amount of compression with a good sized ligature. We think it is an error to employ too small a ligature. When speaking of the effects of a ligature on the aneurismal swelling, Professor Chelius guards the surgeon against concluding, because the pulsation in the sac returns, not infrequently very soon, that the obliteration of the artery is incomplete. On the contrary, he esteems this sign (the previous interruption of the circulation having, of course, been complete) as satisfactory evidence that the ligature has done its duty, or, in other words, that the circulation has been re-established by the blood being forced through collateral channels. The pulsation spoken of, may result from regurgitation, as well as from the more direct communication with the sac itself, or with the main arterial trunk above it. Regarding this condition, our own experience accords with that of Mr. South, that the physical sign just alluded to, is not of frequent occurrence; he mentions an instance under his own care, where pulsation in a popliteal sac returned on the third day; and another case under the care of his colleague, Mr. Green, in which that surgeon tied the common carotid, but the pulsation did not cease immediately on the application of the ligature, and recurred on the following day, and was never lost afterwards, the sac and throbbing concurrently increasing after the seventh week. An interesting circumstance connected with this case is, that the jugular vein of the same side pulsated at intervals after the operation was performed. The sequel is not given; but we think that the explanation suggested by the operator is most probably correct, viz. that the sac was situated at the bifurcation of the common carotid, and was fed by blood regurgitating from the internal carotid. As regards the exclusive use of the ligature at a distance from the sac, Professor Chelius makes the following remark:

> "Important as is the superiority of Hunter's over the old operation, so that by many surgeons it is considered unconditionally as the best mode of operating, yet, after what has been already said about the accidents after the operation, the opening of the sac must be conceded, if the aneurism be diffused and accompanied with much extravasation of blood, and in aneurism at the bend of the arm, on the back and front of the hand and foot." (vol. ii, p. 227.)

We have not space to devote to the other and less frequent or accepted modes of operating, with which this section on "aneurisms in general" is concluded. For the same reason we must also deny ourselves the satisfaction of analysing the chapter on "aneurisms in particular," which contains an excellent digest of all that the surgeon requires to know on the subject, together with plain and sufficiently copious directions for the treatment of aneurismal enlargement in different arteries, from the posterior aural to the innominata; this is enriched by much additional matter, supplied from the translator's own reading and experience. We will, however, detain our readers with a few passing remarks on the succeeding sub-sections which are allied with that we have just passed in review; and first of aneurismal varix and varicose aneurism. We commence with quoting the author's opinion of the appropriate treatment for the former of these diseases.

> "The *cure* of the aneurismal *varix* may be in many cases effected by continued compression, which either effects obliteration of the artery, or brings the walls of

the vein so into contact that the aperture of the artery is closed. But as this mode of treatment, if the walls of both vessels be not connected, exposes the patient to the danger of a complication with aneurism, so may it be employed only in recent cases, and in young or thin persons, where the walls of the vessel can be sufficiently compressed, and the patient recommended abstinence from all exertion of the part, when from that evil no further symptoms are caused. But if the above-mentioned inconveniences of diminished nutrition, sensation, motion, and so on, occur, the operation is indicated, and not indeed, as by many advised, by tying the affected artery above the aneurismal part, but according to the old method, by cutting into the sac, and applying a ligature round the artery above and below the wounded part." (vol. ii, p. 271.)

In the allied disease of varicose aneurism, the author recommends a similar course, in preference to the practice, advocated by Scarpa and Hodgson, of tying the artery above the sac.

In treating of nævus, the author prefers the name "teleangiectasy" to designate this class of vascular swellings, which he considers to have their seat essentially in the capillary system, but involving more or less, according to circumstances, the extreme ramifications of the arteries or veins. In the cure of these troublesome affections, Professor Chelius evidently gives the preference to the ligature over the knife, where the tumour projects, and especially if it have a narrow base. But where the swelling is broad and superficial, he prefers caustic potash, "applied as a paste in an aperture of sticking-plaster, put on around the teleangiectasy," so as to ensure the separation of a sufficient slough. Mr. South has not so much faith in this plan of treatment, but the case he cites in point is scarcely a fair one, as the disease was very extensive. We think very highly of caustics in the cure of superficial or purely cutaneous nævus, but generally employ the nitric acid, unless the subcutaneous textures are involved, when caustic potash is certainly to be preferred. Astringent injection also has often proved successful in our hands, as well as the use of needles in cellular nævi; but large ones require the ligature.

The subject of varix is next discussed, and the various modes of treatment recommended for its cure successively noticed. The usual caution of the author is evinced in his summing up; for he remarks that, "in reviewing these different modes of treatment for the radical cure of *varix*, it must be remembered that in all those accompanied with wound of the vein, there is danger of venous inflammation arising, which often spreads widely and causes death." He, therefore, recommends that the radical cure of varix should never be undertaken "without careful review of the patient's general condition," (whether gouty, rheumatic, or presenting other abnormal distension of the venous system, &c.,") and never without important reason, and pressing demand." Puncture of the vein he regards as least dangerous, but uncertain in its effects; next to this, extirpation and the introduction of threads; tying and incision he dreads as frequently setting up dangerous inflammation; but thinks Sanson's plan of simply compressing the vein, deserving of especial notice and further trial. We think the Professor a little exaggerates the risk of tying a varicose vein, provided, of course, proper regard be had to the constitutional tendency of the patient, as well as to his previous preparation and subsequent treatment. The best method of effecting this, is simply to pass a needle beneath the vein, so that it shall perforate both sides of the skin, and then to wind a strong silk ligature around the needle in a figure-of-eight form, as in hare-lip; of course including the skin covering the vein. Should the inflammation which ensues become excessive (which is very rarely the case), the needle can be removed, otherwise it may be allowed to remain in two or three days, or even till it cuts its way through. Varicose veins do not seem so prone as healthy vessels, to assume that active form of phlebitis, which is so formidable and frequently fatal; probably this is in part due to the alteration in their mechanical arrangement, by which the blood flows tardily through them, and partly referable to their changed and thickened texture.

The last subjects to which we shall devote a brief notice in this section, are "varicocele" and "hemorrhoids." The former of these troublesome affections Professor Chelius considers most amenable to Breschet's plan of treatment, of which we are tempted to insert a short account. The patient is first to prepare himself by exercise or a warm bed, so as to ensure repletion of the diseased veins. The scrotum is then raised, and the vas deferens felt for and excluded. The compressing forceps of the inventor (supplied with a screw) is then to be applied as high up as possible, and so as to include *all* the varicose veins, and in this way nearly the whole of the affected side of the scrotum is brought within the grasp of the forceps, which are forthwith tightened by means of the screw; a further arrangement in this piece of mechanism allows of a concealed plate on the upper arm of the forceps being pushed forwards next the screw on the septum, and thus the compression is rendered more uniform and complete. Suppuration is established about the fifth or sixth day, and the apparatus is then to be removed, and the part treated simply. This plan has been before the public many years (since 1834), but has never, we believe, been much used in England, though many modifications of it have been recommended.

In the treatment of hemorrhoids, our German author gives a decided preference to excision with *scissors* over the employment of the ligature, on account of the severe pain, inflammation, and constitutional disturbance which so frequently follow the latter practice. The pile, he says, should be grasped with the forceps, drawn forwards, and cut off in such way that the wound shall be partially covered by the skin; and in external hemorrhoids, this cut is to be made in the parts below the sphincter, so that the wound may retract and the bleeding be commanded by the contraction of this muscle. When the ligature is used, Copeland's recommendation is a safe one, viz. to include only one of the tumours at a time, and to let the patient recover from this before the operation is repeated.

We think it unnecessary to apologise for the lengthened notice we have taken of this section. It has afforded us, from its importance and the care with which the entire subject is treated, an opportunity of discharging our duty as reviewers, more satisfactorily than by cursory glances at many chapters. We shall, however, be obliged, in consequence, to be more brief in our analysis of such remaining parts of the work as we think it desirable to bring before our readers. But Mr. Lizars will think we have quite taken leave of him, and we must again renew our (we fear not very friendly) acquaintance before we dismiss him.

The third chapter of the system of the ci-devant Professor of Surgery to the Royal College of Surgeons of Edinburgh, is occupied with the diseases of bones; which subject, including fractures general and special, is dismissed in twenty-four pages. We have selected this for our next examination, as it is one of so purely surgical and practical a character, that it will afford us more ready means of ascertaining the author's capabilities and acquirements in a branch of his profession, which must of all others be deemed a fair test of his qualifications to guide his younger brethren in the commencement of their surgical career. We fear that he will here also be found wanting; but we will not anticipate the verdict, which we must leave, when the case is stated, for our readers to pronounce.

Of the twenty-four pages aforesaid, eight are appropriated to a very scanty notice of the diseases of bones and their treatment. We pass over these without comment, and turn to the treatment of fracture generally, where some introductory remarks are given, the case supposed, by way of illustration, being simple fracture of the tibia. As the paragraph to which we allude struck us, during perusal, as being one of the best the work affords, both as to matter and style, (we do not say it is faultless,) we willingly quote it entire and without comment. After speaking of the form of splint to be used, and the proper mode of bandaging the limb, the author thus proceeds:

> "The foot of the splint should be raised as high as agreeable to the patient, to favour the venous and lymphatic circulation, and to prevent inflammation. The region of the fracture should be fomented if necessary; the patient put on low diet, rest enjoined, and gentle laxatives administered, until every tendency to inflammation has subsided. Then generous diet should be allowed, and the limb bandaged firmer and firmer, but never examined at this stage of the cure, unless the bandages become slackened, or the fractured ends of the bones displaced. If no union has taken place, at the end of six weeks or two months, pasteboard splints cushioned with tow, ought to be applied on each side, very firmly bandaged, and the limb again laid on its iron splint, and retained for two or three months longer. If there be still no union by this time, viz. five months, it is to be feared that a false joint has formed. If the process of reunion has gone on mildly, the patient may be allowed to sit up with the splint at the end of six weeks, or with Dupuytren's splint, which is much lighter. This latter closely resembles Desault's long splint for the thigh-bone, (represented in plate ix, fig. 4,) being merely shorter, extending from the end of the fibula or tibia, to a little beyond the sole of the foot. But if there has been any inflammation, or the fracture be near the ancle-joint, upwards of eight weeks should be allowed to elapse before sitting up. The patient should walk on crutches with his foot suspended by a broad belt over his shoulders for the first month or so; and not attempt to put it to the ground for four months from the reception of the injury, even in the most favourable case, because any weight pressing on the newly-formed bone, is liable to excite absorption, and thus reproduce the fracture. During the treatment, there is often considerable inflammatory action, which is not unfrequently caused by tight bandaging; hence, for the first eight or ten days, the site of the fracture should be examined every second day, but the ends of the bone never disturbed." (p. 124.)

The fractures to which different bones are obnoxious, are next brought successively under consideration. We shall make a passing comment here and there, as we turn over the pages. In fracture of the patella, Mr. Lizars is satisfied with raising the trunk and keeping the limb extended on a splint, and rolling it throughout, exclusive of the parts between the separated fragments. The union, he adds, is never ossific, but *fibro-ligamentous* or *cartilaginous*. This result is by no means necessary, as Dupuytren and other surgeons have shown; ossific union would be facilitated, we apprehend, if some further measure than that recommended by our author were employed for approximating the separated fragments. Fracture of the neck of the humerus is dismissed with this brief remark:

> "It is frequent in advanced life, and requires careful examination, as it is liable to be mistaken for luxation of its head into the axilla. In the fracture, the rotundity of the joint is preserved. It should be treated as the preceding (fracture of the shaft), with the addition of a pad in the axilla." (p. 129.)

We guess (as our transatlantic brethren would say) that the young practitioner, consulting this 'System of Surgery,' in his first posing case of this nature, would find himself but little enlightened by this diagnostic exposition, to say nothing of the puzzling information that the injury in question is liable to be mistaken for dislocation into the axilla, and yet the rotundity of the joint is preserved!

Nothing is said about the employment of convex splints or pads in fracture of the bones of the forearm, nor of adduction or flexion of the hand in fracture of the carpal extremity of the radius. The author questions whether the coracoid process of the scapula is ever broken off. And, in speaking of fracture of the spine at the next page (133), he says, "cases have occurred wherein the second, third, and fourth cervical vertebræ were fractured, and yet respiration continued unimpaired, seemingly through the medium of the great sympathetic nerves." Does Mr. Lizars believe that, in such rare cases, the spinal cord really was crushed at the seat of injury, and that the sympathetic can, subsequently to such a lesion, sustain the respiratory movement? Can our readers comprehend the precise meaning of the following observation on the operation of trephining and raising a depressed fragment of bone in fractured vertebræ?

> "The analogy between trephining the cranium and the vertebræ, is fallacious. When we trephine the cranium, we do not expose any great extent of the dura mater; but when we remove a portion of a vertebra, we expose a great extent of the theca vertebralis. Besides, when the dura mater is incised, the arachnoid and pia mater fill up the wound from the pressure made by the circulation of the vessels of the brain; but, if a wound be made in the theca vertebralis, a great extent of the arachnoid membrane is exposed, and if this last be wounded, the pia mater still more extensively." (pp. 133–4.)

As we presume these remarks are designed to apply to the ill success which has attended the few attempts which have been made to relieve the symptoms produced by pressure on the spinal cord, we may venture to remind the author that it is the irremediable mischief done at the time of the accident to the true spinal centre and accumulated conducting nervous matter at the seat of lesion, and the disorganization consequent thereon, that constitute the real cause of failure where the accident is sufficiently serious to threaten life, and to lead, without such operative interference, to a fatal termination. We have witnessed the operation, and have no faith in it. Again, fracture of the pelvis is spoken of as necessarily accompanied by "paralysis of the urinary bladder and rectum," at least there is no qualifying word or phrase; and emphysema is mentioned as being produced by the fractured rib piercing the lungs, without wounding the pleura costalis;—rather a delicate operation, we trow. And here we come to the close of this chapter, but shall detain our readers with one or two remarks on the allied subject of "Diseases of joints," before we proceed further.

We pass by, as before, the scanty section which is devoted to the disease affecting the various constituent parts of articulations, and shall confine ourselves to their injuries, which are included in the same chapter. After a few lines of general introductory matter, the author commences with "luxation of the shoulder-joint;" we will see what he says on this simple, but important and practical subject, and first of the signs of axillary dislocation.

> "When the head of the bone of the arm is forced into the axilla, there is a marked depression under the acromion scapulæ, the arm is semi-bent, the patient supports it with the sound one, and rests it on his thigh or pelvis, and generally complains of a good deal of pain. The surgeon cannot raise it to a right angle with the body, and the attempt aggravates the sufferings. In a day or two, sometimes considerable œdematous swelling supervenes.
>
> "Dissection exhibits the fibrous capsule torn, the short muscles around the joint lacerated, and even some of the longer, as the coraco-brachialis, &c." (pp. 151–2.)

Further on (the reduction being effected), "the arm should remain unused for some days for fear of inflammation . . . .; and it should never afterwards be raised above a right angle with the body, otherwise the dislocation will be reproduced." Now, as we have no alternative but to infer that the mischief described as being displayed by dissection, is such as is ordinarily met with in this dislocation (according to our author's experience), we may naturally ask with some surprise (and incredulity, if answered in the affirmative) whether "some days" are to be considered sufficient time for the reparation of such a serious lesion? This may not be,—nay, we must believe, is not,—the author's meaning; but is not such a loose mode of expressing himself most culpable, when he takes upon himself the responsible duty of instructing the tyro in surgery? And does he really consider as sufficient the few lines he has devoted to the diagnosis of this common dislocation, the mistaking of which has ruined the prospects of, we believe, many a practitioner? Why are not the distinctive characteristics between this and other luxations of the shoulder given, and especially between dislocation and fracture near the joint? Want of room cannot be pleaded, for many comparatively unimportant subjects have much more space assigned to them.—But we proceed. Luxation of the wrist-joint is spoken of as a frequent accident; this is not in accordance with the experience of most practical men; Dupuytren even believes that the cases recorded as such were fractures of the radius near the joint. In discussing the dislocations of the hip, a distinctive characteristic between luxation on to the dorsum and into the ischiatic notch, is incorrectly ascribed to each, viz., the amount of inversion as marked by the position of the toes of the injured limb; "the toes," (of the affected limb), says our author, in speaking of the dorsal dislocation, "rest on the great toe of the sound limb;" and again, in describing the signs of dislocation into the ischiatic notch, "the great toe rests on the tarsus of the sound foot." Further he remarks, "the symptoms (of the latter dislocation) are so nearly the same with the former, that I am convinced we are often deceived between them." This last (though unfounded) assertion enhances the gravity of the error in diagnosis we have just pointed out. We must beg, however, to differ from Mr. Lizars in the opinion that these dislocations may be so easily confounded; the amount of shortening and inversion are alone important distinctions; but further, is there no difference in the position of the head of the femur, which is, in most instances of dorsal luxation, sufficiently perceptible to the touch? That dislocation into the ischiatic notch may be quite overlooked, we quite believe; for the deformity, with the exception of the altered position of the great trochanter, is not very striking to a superficial observer. Mr. Lizars informs us that he has seen two cases of dislocation of the ancle *without fracture;* that outwards in an old lady, and inwards in a young lady. This has, we venture to say, occurred to but few surgeons to witness.

Dislocation of the vertebræ, without fracture, would seem to be no such rare accident as others have found it, for our author says, "the cervical are those most frequently luxated," &c. The closing subject of this chapter is club-foot, to which nearly the same space is devoted as to all the diseases of bones. The author recommends that the operation for talipes should not be attempted before the patient is three years old; and he ascribes these deformities to "the slow and progressive luxation of some of the tarsal bones, and the consecutive derangement of their ligaments and muscular tendons." We cannot agree with the advice here given, believing that, as soon as the child can balance itself on its legs, it is time to operate; and, with regard to the opinion we have quoted, it appears to us very like putting the cart before the horse and mistaking cause for effect; the root of the evil we believe to be in the nervous centres (most probably the excito-motor), by which tonic or permanent spasm of certain muscles is produced, and thus the foot is distorted; but there is no luxation of any of the tarsal bones, such as Mr. Lizars speaks of; indeed, in long-standing cases, we have been surprised, on examination of the tarsus, to find how little its component parts have lost their normal relations compared with what might have been anticipated. We are bound to say that the plates accompanying this and the preceding chapter, illustrating the reduction of dislocations, apparatus employed in fractures, and different deformities of the feet and legs, are, for the most part, good.

We select for notice one more, and the concluding chapter, viz., that on the "Diseases of the genito-urinary organs," before we finally dismiss the volume of our Edinburgh contemporary; and though we make this choice, it is not with a view to further strictures (if we may be excused a bad pun), for, on the whole, we find less to condemn, and more to commend, in this section, than in any other in the work.

A fearful announcement is made when the use of the catheter is described, which albeit, we fear, is not entirely without foundation. "Catheterism," says our author, "is, generally speaking, one of the most difficult operations in surgery; it has consigned more men to the grave than all the weapons wielded by the bands of Sylla and Marius!" (p. 379.) Flexible gum catheters, he advises, should never be used in retention of urine in the first instance; and we know that many, if not most, surgeons acquiesce in this opinion. We think, however, that there is no instrument like that made of flexible gum, inclosing a small sound with a broad handle; the operator has more command over it than even the silver catheter, and the sound can be withdrawn as easily as a wire, leaving what, if so required, is certainly the best instrument to remain in the bladder. The general directions which are given for passing the catheter, are good; but we cannot agree with the author in thinking that puncturing the bladder from the rectum, is preferable to opening the urethra from the perineum, where the source of obstruction is permanent stricture; though we acquiesce in his condemnation of the performance of the operation above the pubes. In the operation of lithotomy, Mr. Lizars uses the knife, and a curved staff grooved at the side: we think both the side groove and curve (except near to the extremity) are less safe than the central groove and straight staff, when the knife is employed; this, however, is matter of opinion and habit, for nearly every lithotomist has some fancy of his own, of which it is better that he should be left in undisturbed possession. Our author's remarks on lithotrity prove that it is not a favourite operation with him. His treatment of diseases and injuries of the male urethra is, on the whole, judicious, and evinces a practical acquaintance with the subject; but there is one remark, occurring in the chapter on the "Diseases of the female organs," which a little startled us, as being at variance with our own experience, and, we fear, that of most of our chirurgical brethren; it is this: "This aperture (vesico-vaginal fistula), which I have seen so large as to admit three fingers, is easily cured by touching its margin with the actual cautery at a black heat, but not oftener than once in three months." (p. 456.)

The remaining part of Mr. South's work which we select for notice is, the fifth division, which includes those "Diseases which consist in the degeneration of organic parts, or in the production of new structures." Our space and the largeness of the subject will not admit, however, of much more than a cursory survey of the important pathological changes, and their corresponding surgical treatment, which it embraces. The chapter is introduced by a brief but instructive review of the various classifications of the large body of diseases to which it has reference.

> "All degenerations of organic parts," says Professor Chelius, "appear to have their origin in a local increased vascular activity, on which depends the deposit of a substance more or less resembling the elementary parts of the organ, or of a peculiar substance in the interstices of the part, in which, by the shooting forth of the vessels and their very extensive ramifications, the unnatural formative disposition is sustained, and the growth of the tumour increased. We find, therefore, also, in most cases that, although there be no characteristic signs of inflammation, yet that increase of temperature, peculiar sensibility on pressure, tension, and the like, accompany the beginning of degeneration............... If these conversions depend only on quantitatively-changed nourishment, they can increase to an enormous extent, without otherwise than mechanically operating destructively.............But if there be at the same time a qualitative change of the nutritive matter, if they depend on general *diathesis*, and alteration of the formative disposition, they have a specific character, are accompanied with reaction of the whole organism, and draw all the tissues without distinction of their organization into the same diseased change...........Hence arises the division into *benignant* and *malignant* tumours."

After this clear and simple exposition of the great distinetion with which the above paragraph concludes, the author proceeds to speak of the "after-products," which must be considered as new formations, and which, he remarks, are either "repetitions of natural formations," or they are formed from "substances foreign to the natural composition of organic parts," or simple "vegetative growths," or, lastly, they possess the specific characters above described; adipose tumours, medullary fungus, polypi, and scirrhus constitute, severally, familiar examples of the above varieties. The *cellular* and *corpuscular* theories, springing from microscopic observations, are next alluded to; and, subsequently, the classifications of Abernethy, Meckel, Laennec, and Müller. At the risk of being thought tedious, we cannot resist the temptation to quote again from our author, the simplicity of whose style, and whose practical and sensible views, mingled with a sufficient share of learning and philosophical discrimination, shine in such passages as the following; and let us not exclude Mr. South from his share of praise, for entering into the spirit of his prototype (for such he is evidently gratified to make him), and so well rendering his meaning:

> "These various opinions serve to prove how difficult it is to make an accurate and sufficient division of tumours dependent on their unnatural structure. The ground of this difficulty rests, without doubt, on the manifold changes to which the diseased matter is subject in the various periods of the development of the tumour; further, on the undistinguishable influence which the natural structure of an organ has upon the after-formation developed in it, and on the variety of causes upon which it depends. There are, therefore, tumours in which are present, at the same time, several of these products, either lying near to, or intimately connected with, each other. But if pathological anatomy be of direct use and immediate application to practical medicine, it must not rest on the mere examination of the changes of organic parts and of the substances forming the tumour, and assume these alone as the ground of division; but it must at the same time consider the symptoms, course, and effect upon the immediate neighbourhood, and the whole body, if it would not be seduced, by incorrect particulars, into subdividing similar diseased conditions. Under this supposition, the number of tumours founded upon the above-mentioned variety of diseased tissue, may be conveniently referred to three, namely, *medullary fungus*, *scirrhus*, and *tubercle*, in which case the pancreatic and mammary tissues are to be considered merely as accidental modifications of the medullary." (Vol. ii, p. 648.)

The author adds that these unnatural structures may agree in general with each other, consisting "probably, or for the most part, of *Albumen*, and their tissue being more or less distinctly cellular."

The first specific affection which is treated of under the present division of Professor Chelius' work is "enlargement of the tongue," consisting of a simple hypertrophy of the organ. In the milder and more tractable form of the complaint, the author recommends the use of astringent applications, scarifications, leeches, and moderate pressure. But when the enlargement is considerable, the ligature or excision is called for; of these, the author seems to prefer the latter, paring the tongue in a semilunar form, or removing a triangular portion, with its point directed backwards. The next subject is "bronchocele," which is divided into the simple, the vascular, the lymphatic, and the scirrhous. Cretinism and bronchocele the author does not regard as standing in any necessary casual relation; but he remarks that "experience shows in most cases in cretins there is a peculiar misformation of the skull, by which the circulation in the carotids is more or less disturbed, and the enlargement of the thyroid gland produced by the greater inflow of blood." In the vascular form of this disease the author states that he has employed cold applications, conjoined with the internal use of digitalis and avoidance of every exertion, in two cases, and with success. But, if it resist this treatment, he considers the application of a ligature on the superior thyroid artery the most appropriate treatment, preferring it to attempted extirpation of the morbid growth or the employment of seton for its reduction.

Enlargement of the clitoris and labia pudendi, warts, callosities, and corns and horny growths, have each a short section devoted to them. Under the head of bony growths, exostosis is first treated of. Osteo-steatoma, osteosarcoma, and spina ventosa are classed together as allied diseased conditions. Mr. South gives, in an extract from his works, the description of the last-named disease by our countryman Wiseman; and in speaking of the succeeding affection, tubercle in the bone, Professor Chelius pays his French translator the compliment of transcribing the abstract which he (M. Piqué) has given of M. Nelaton's treatise on this subject. But we must pass by these and other allied subjects, and limit our remarks, in the brief space that remains to us, to medullary fungus and cancer.

The former of these diseases, which is classed with the latter by Carswell, Travers, and Walshe, has been variously named medullary cancer, encephaloid disease, fungoid disease, fungus hæmatodes, &c., by different authors. It is defined by our author as a "soft, elastic swelling, which, both during life and after death, yields a delusive fluctuation to the touch." The skin subsequently yields at one point, and therefrom springs a *fungus*, "loose, but little painful when touched, and bleeding readily," &c. He adds, that it is difficult to describe the diseased substance contained in the tumour, as it varies so much at different periods of the disease, even (in point of colour) between the extremes of white and black. Thus, Professor Chelius holds with Meckel, that melanosis is, in one point of view only, to be regarded as identical with medullary fungus; he observes, melanosis may occur in other ways, and is to be considered merely as a diseased secretion of a colouring matter, without trace of organization, presenting itself either alone or in tumours of different kinds. In contrasting the above disease with scirrhus, the author points out the following distinguishing characters of the latter.

> "At its onset it forms a hard, firm, incompressible substance, which, when cut into thin layers, is semi-transparent, has the consistence of cartilage and fibro-cartilage, even to that of lard with which it agrees in appearance, and is composed of two different substances: the one hard and fibrous, the other soft, and of an inorganic appearance. The fibrous part forms various partitions and cavities without arrangement, in which is contained the softer substance, ordinarily of a pale brownish, sometimes bluish, greenish, whitish, or reddish colour, similar to hardened *albumen*; the fibrous part has sometimes a cartilaginous hardness. But especially are the proportions of these substances very different," &c. (Vol. ii, p. 724.)

Our author's experience confirms that of most practical men, that art can do little against medullary fungus, and even that its entire and early removal is, in rare cases only, crowned with success.

In the section on Cancer, Professor Chelius remarks that two distinctions are to be established in reference to its origin; "it is either developed from a previously formed *scirrhus*, or it arises from some other swelling, or some other ulcer in which the scirrhous degeneration has been set up." The course of the disease is then graphically described, and its microscopic appearance enumerated. Müller's division of carcinoma into four kinds is subsequently set forth by the author; and this is followed by the account given of its development and progress by Dr. Hodgkin, for which we are indebted to the translator. The difficulty attending the diagnosis of scirrhous tumours is admitted by our author, who considers that "when the skin covering the swelling is puckered, has a dark leaden colour, a knotty and irregular surface, when sometimes there is lancinating pain in the tumour, and it is firmly attached to the neighbouring parts," there can be no doubt of the scirrhous nature of the swelling. These are, however, not to be regarded as the invariable or necessary indications of scirrhus; for some, and even many, or most, of these signs may be absent; the weight of the tumour, together with the general limitation of its size may serve to assist in the diagnosis, as may also the effects of treatment, adapted to scrofulous syphilitic affections, which, as our author observes, cannot avail in the malignant disease, soothing remedies alone diminishing the pain.

Under the head of Treatment, the various measures recommended are discussed. With respect to the first, pressure, we regret that we are forced to coincide with Mr. South, that it is of little real service, even when employed according to Dr. Arnott's improved method; and we suspect that many of the cases reported as cured by this ingenious method, must have been simple tumours of a benignant character, simulating scirrhus. Caustic, our author thinks applicable only where the disease is superficial. The consideration of removal of scirrhous tumours by the knife finds its appropriate place under the subdivisions which include the several localities or organs obnoxious to the disease. These we shall be unable to analyse, although we recommend them (both original and added matter) to the attentive perusal of our readers. We cannot but remark, however, on the coincidence of opinion, elicited by a careful examination of their writings, which exists amongst the most experienced surgeons, as to the questionable advisability of removing scirrhous tumours, especially of the breast; and it is a disheartening, but important lesson to the young and sanguine practitioner, to weigh carefully all that can be brought to bear upon the merits of each case presented to his notice before he takes the knife; and never rashly to promise future impunity from so dire a disease, which it is too often entirely beyond the reach of operative surgery to cure.

And now for a few concluding remarks, such as are usually and reasonably expected of critical reviewers; partly that they may give a sort of "*quod erat demonstrandum*" finish to the problem of relative merit which they proposed to unravel, in bringing their authors to the tribunal of standard worth and merit; and partly to satisfy such of their readers as are too indolent or busy to accompany them in their analyses, or who have sufficient bad taste to think them prosy and prolix. Not that we can think the first of these remarks is necessary for those who have honoured us by a perusal of the foregoing pages, in which our opinions stand recorded in, we apprehend, unmistakeable clearness. But we must not forget the sedateness pertaining to our judicial character.

First, then, respecting the work of Mr. Lizars. We candidly acknowledge that, although it has passed one edition, and is now presented to the public for the second time, we were entirely unacquainted, until the present volume was placed in our hands, with its literary or professional merits, and had, indeed, never even seen it before. Our opinions, therefore, were not, even unconsciously, biassed one way or the other; and we admit our disappointment and chagrin that this volume should have reached a second edition, and have been put forth in its existing condition from the British press, as a 'System,' representing the present state of surgery in this country. Neither should we have thought it worth while to devote so much space to the analysis of its contents, had we not felt imperatively bound to do our duty in pointing out errors and omissions which might mislead the student, for whom, we presume, it is especially intended. We believe that our extracts and remarks will have fully proved that these observations are not unmerited or are too severe. The literary character of the volume is, generally speaking, below mediocrity; and there is a looseness and carelessness of diction in many parts, nay, in most pages, which is offensive, because we can scarcely believe that Mr. Lizars could not have written better if he had pleased. This will have been noticed in some of our extracts: but what do our readers think of *gunshot* wounds having "long been proved to have no poisonous character, as supposed by the *ancients*," (p. 173;) or of the fact that "a bee-hive attacking an unfortunate individual may prove fatal," (p. 181.) But we could multiply these instances indefinitely, were we so disposed. There is also a great inconsistency or disproportion in the amount of space devoted to different subjects. Thus, fracture of the neck of the humerus and of the elbow-joint are unitedly comprised, treatment and all, in *nine* lines (p. 129); whereas three-quarters of a page are devoted to the description of a clumsy mode of plugging the nostrils in epistaxis. The engravings, as we have already remarked, are very unequal in character, and might have been considerably reduced in number, with advantage to the general character of the work.

We turn from this ungracious task to the more grateful one of taking leave of our German author and his worthy English coadjutor. Of food for criticism there is but little. We certainly could have wished that Mr. South had rendered the German into more pure idiomatic English, substituting corresponding vernacular phrases for the Germanisms which are of frequent occurrence, and which mar the uniformity of style that is so desirable for continuous reading. We suspect, however, that Mr. South would rather pique himself upon this, as it savours of the same smack of pedantry that his pure Saxon English possesses. We should have been better pleased, and we think it would have been in better taste, as well as more euphonious, if the Latin terminology had not been so neglected,—we had almost said, scrupulously excluded. "The joint-ends of tubular bones" would have read more pleasantly, had it been, "the articular ends of cylindrical bones," &c.;—and the introduction of such a word as "rump" grates a little harshly upon the ear, when other and more comely synonymes might have been substituted. These, however, are but trifles after all, and matters of taste and embellishment; for we have no complaint to make of want of sound grammatical construction in Mr. South's composition; but we think that authors should recollect that style holds the same relation to the matter of their works as the frame does to a picture; and no artist, we take it, would be willing to forego the acknowledged advantage pertaining to a tasteful and well-selected finish of this sort, even for his masterpiece.

Of the arrangement of the work we cannot but approve, and think it judicious of Mr. South not to have interfered with the plan selected by his author. The annotations and additions evince infinite research and great judgment. In a future edition they may, perhaps, bear a little more compression. The translator enters entirely into the spirit of his author;

and we should say that there is much that is congenial in their cast of mind; leaning, in surgical practice, rather to the cautious than to the heroic. The tabular views of operations, especially of stone cases, are valuable and interesting in a statistical point of view. A *very* copious index is appended to the work, which of course greatly enhances the value of so voluminous a treatise; though we think Mr. South might improve even this in another edition. On the whole, we feel gratified with, and proud of, the work in its English garb; and we do not hesitate to pronounce it the best and most comprehensive system of modern surgery with which we are acquainted, and as such we earnestly recommend it to the student and practitioner.

ART. VIII.

Die Krankheiten der Arbeiter in den Phosphorzündholzfabriken, insbesondere das Leiden der Kiefercknochen durch Phosphordämpfe. Vom chemischphysiologischen, medicinischchirurgischen und medicinisch-polizeilichen Standpunkte. Bearbeitet von Dr. FREIHERRN ERNST VON BIBRA und Dr. LORENZ GEIST. Mit neun gemalten Kupfertafeln.—*Erlangen*, 1847.

The Diseases of the Workmen employed in Lucifer Manufactories, and especially the Affection of the Maxillæ, produced by the Vapours of Phosphorus, considered in their Chemico-physiological, Medico-Chirurgical, and Forensic Relations. By F. ERNST VON BIBRA, P.H.D., and LORENZ GEIST, M.D. With nine coloured Engravings.—*Erlangen*, 1847. pp. 347.

IN the Surgical Reports of Guy's Hospital, for the year April 1846 to March 1847, we find it stated (p. 163), that of the diseases of the lower jaw, one occurred in a lucifer-match maker, with suppuration and exfoliation of bone; and that the disease had been previously noticed not to be uncommon in those working in phosphorus. Dr. Letheby has also drawn attention to the fact of the inhalation of phosphorus producing a peculiar effect on the system, in a paper on the detection of poisons in the urine, read before the Medico-Chirurgical Society at the commencement of the year 1847. These notices embrace, with the exception of an extract from German writers given by Dr. Balfour in the Northern Journal (vide Lancet, Aug. 29, 1846), all that has been said on the subject of the phosphorus-disease in this country. A work like the one before us, containing a summary of all that continental authors have written on the subject, as well as an original and satisfactory investigation of the diease, is therefore very acceptable; and we must feel the more indebted to Doctors Von Bibra and Geist for applying their combined talents to the inquiry, producing a result worthy in every way of the present state of natural and strictly medical science. Although the subject may not appear to be one of very general interest to our readers, nor the treatise likely to fall into the hands of many British practitioners, we would wish to hold up the work before us as an example of that spirit of liberality and fellowship which is essential to the cultivation and development of genuine knowledge, and which will become more necessary the more the field of intellect is widened. It is impossible, even for the present generation of medical men, to acquire or keep up an intimate knowledge of all the branches of science which bear upon the practice of our profession; and a division of labour will be rendered more and more imperative as we advance. All that can be fairly expected is, that he who devotes himself to the cultivation of a portion of the field, should be possessed of sufficient discernment to appreciate the labours of others in the same field, and be ready to cooperate with them for the ultimate attainment of the same object.

The chemist and the physician have, in the present instance, conjointly applied themselves to the investigation of a disease, which necessarily is of recent origin, since the manufacture of lucifers does not itself date far back. The old-fashioned tinderbox is scarcely yet forgotten amongst ourselves; and abroad we still find the *boletus igniarius* in common use as a means of obtaining light and fire. The manufacture of the common sulphur match, which is now exploded, might occasionally have produced bronchial affections by the disengagement of sulphurous-acid fumes; but the quantity consumed was so small, compared to the present consumption of lucifer matches, and the manufacture itself is so much simpler than that of the latter, that for both reasons the number of workpeople employed in the two instances differed very much. The processes necessary for the preparation of lucifers include those for the manufacture of sulphur matches, as the first stages; and the account we shall give applies as well to the method adopted in Germany as to that pursued in England, which we have been at considerable pains to investigate, in order, if possible, to satisfy our readers as to the reality both of the causes and of the effects. It appears that the invention was originally made in Germany, and thence imported into England. Dr. Geist states that his countrymen commenced the manufacture between sixteen and twenty years ago; whereas the proprietor of an extensive manufactory in the city, who assumed for himself the priority of the invention, or of its introduction into England, stated that he had only been engaged in the business for about ten years. However, we have received conflicting accounts with regard to this point, and would not therefore insist upon the absolute correctness of the information.

The first stage in the manufacture of lucifers is the cutting the wood, which is done, according to the extent of the manufactory, either by hand or by machinery. This, as well as the subsequent process of counting and placing the matches in frames, is in itself necessarily free from any inconvenience or evil consequences; nor does it appear that the third stage, which consists in melting the sulphur and dipping the heads of the matches in it, produces any inconvenience. The fourth, fifth, sixth, and seventh stages comprise the grinding, mullering, and mixing of the explosive compound, the process of dipping the matches in it, the counting and boxing. The dipping, counting, and packing appear to be, according to Dr. Geist, the only departments in which the workpeople are in any way affected with peculiar complaints; we would even limit the appearance of the jaw-disease to those engaged in dipping, at least all that we have examined on the subject were unanimous as to the fact that dippers only were attacked. There is a certain degree of secrecy observed relative to the proportions of the composition; and the mixture of the materials is generally performed by the proprietor of the manufactory or by a confidential workman. Chlorate of potash is considered an essential ingredient in England; but in the manufactories at Nürnberg it has not been employed for a number of years, as its explosive properties much endangered the safety of the buildings and the limbs of the workmen.

> "The composition used in Nürnberg consists of one third of phosphorus, of gum arabic (which is eschewed by English manufacturers on account of its hygrometric property), of water, and of colouring matter, for which either minium or Prussian blue is employed. If ignition be required without a flame, the quantity of phosphorus is diminished, or nitrate of lead is added. The mixing is conducted in a water-bath, and during this process, and, as long as the phosphorus is being ground or 'mullered,' copious fumes are evolved. The dipping is performed in the following manner: the melted composition is spread upon a board, covered with cloth or leather, and the workman dips the two ends of the matches alternately that are fixed in the frame; and as this is done with great rapidity, the disengagement of fumes is very considerable, and the more liable to be injurious as they are evolved in a very concentrated form close to the face of the workman. This department is generally left to a single workman, and the average number that he can dip in an hour, supposing each frame to hold 3000 matches, would be one million." (p. 233.)

After the matches have been dipped, they require to be dried. This is generally done in the room in which the former process is carried on; and as a temperature of from 80° to 90° F. is necessary, the greatest quantity of fumes is evolved at this stage. When the matches are dried, the frames are removed from the drying-room, and the lucifers are now ready to be counted out into boxes. As this is done with great rapidity, they frequently take fire, and, although instantly extinguished, in the saw-dust or the water which is at hand, the occurrence gives rise to an additional and frequent evolution of fumes.

The first, and probably the most natural, inference that any medical man would draw from the foregoing account, would be, that the workpeople engaged in lucifer manufactories were liable to great inconvenience or disease from the direct contact of the phosphorus fumes with the mucous membranes of the eyes, of the respiratory and digestive organs, and (in females) of the genital system. But it would not seem that the effect is proportioned to the apparent strength of the cause, or that, in fact, any constant effect of this kind is observed. Only the French writers[*] dwell upon bronchitic affections, as a frequent and serious consequence of the influence of the phosphorus fumes; and although Dr. Geist gives one case of severe bronchitis, which he was unable to refer to anything else than to the direct influence of the vapour, he distinctly states it as his experience that bronchitic affections are the exception, and that the people engaged in this occupation are not more liable to general indisposition than the workpeople in other manufactories. Our own inquiries, so far as they go, fully corroborate Dr Geist's statement; in fact, we are able to advance a step further, for several of our informants have assured us that their general health has improved since they have been engaged in this occupation. We shall have occasion hereafter to revert to the London cases more particularly; but we may here mention a fact which is assumed by the workpeople themselves as a matter of confirmed experience, that when the local affection occurs, it acts as a counter-irritant, and thus serves to improve the constitutional condition, if this had been previously debilitated by other causes.

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[*] Gendrin, in Roussel's Mémoire présenté à l'Académie des Sciences, le 16 Fevr. 1846; Strohl, Gazette Méd. de Strasbourg, Nov. 1845.

It is to this local affection that Drs. von Bibra and Geist have more particularly devoted their attention; and, in accordance with the title of the work, they have divided the subject into three sections. The first, or chemico-physiological part, is treated by Dr. von Bibra; the second and third, which discuss the question in a medico-chirurgical and forensic point of view, and which constitutes the bulk of the volume, are the contribution of Dr. Geist. A short appendix is added by both gentlemen, in which the most recent cases are adverted to, which had occurred while the work was in the press.

So little material difference presents itself in the symptoms of the numerous cases given, that we shall consult the convenience of our readers by detailing but one at length. This will at once serve as the most suitable pivot or starting-point for the further remarks that we shall be induced to make, and will afford a more distinct picture of the affection than a general summary.

"Barbara Keim, æt. 22, a well-made, under-sized brunette; of healthy constitution; menses regular, no morbid predisposition, not scrofulous; had been engaged for four years in a lucifer manufactory in counting the matches, which at that time was done in the drying room.

"During the first 3½ years she remained perfectly healthy; during the last half year she had occasionally suffered from toothache on the right side of the lower jaw, but to this she paid little attention, as she had formerly been subject to toothache, and had lost several teeth by caries. As the pain only occurred periodically, she was not induced to quit her occupation. At the beginning of February, 1843, the toothache on the right side of the lower jaw became more severe, it ceased to be limited to the carious teeth, and extended through the whole jaw, over the cheek, and even to the temporal region and the neck. At the same time considerable febrile disturbance, with occasional rigors, supervened, accompanied by swelling of gums and cheek, with erysipelatous redness of the latter. February 4th, she was admitted into the hospital. The examination showed the right cheek much swollen, and very tense towards the eye, the mouth, the chin, and neck; the tension most considerable in the vicinity of the lower jaw. Pain deep-seated, throbbing, piercing, concentrated at the angle of the jaw, and radiating thence over the adjacent soft parts. Extreme tenderness on pressure. The gums of the diseased side of the jaw much swollen, tense, darkened, and tender; the mucous membrane of the cheek equally so; between the angle of the jaw and the first molar, thick, fetid pus of a phlegmonous character, oozed out on pressure.

"The first and fourth molars on the right side of the lower jaw were carious, the other molars deficient, incisors and canine sound. Tongue furred, bitter taste, pain in forehead, oppression at the epigastrium, ructus, nausea, constipation, inflammatory fever.

"Ordered, an emetic, 15 leeches at angle of jaw, a laxative to follow, and a gargle of oxymel and aq. flor. sambuci.

"Evacuation of a large quantity of acid and acrid saburra. Relief of general symptoms; gradual progress of local affection. The swelling of gums increased, became more tense, extended to the soft palate; dysphagia and salivation supervened; and the jaw became almost immoveable. Suppuration increased, pus laudable, but fetid. Ordered, 8 leeches to angle of jaw. Ung. hydr. with ol. hyosc., to be rubbed in, nitre mixture, iodine externally and internally. Poultices.

"During the second week, the four incisors and one molar became loosened, suppuration ichorous and rusty, gums softened and livid, formation of sinuses on the external and internal surface, through which the probe reached the bone, which appeared in part rough, in part smooth. Pain lessened, extreme exhaustion. Commencement of cough and hectic symptoms. In the progress of the disease, increased retraction of gums from the alveolar surface, fresh abscesses, increased discoloration, and burrowing sinuses, so that the entire side of the jaw was exposed to the probe, both externally and internally. The bone appeared almost entirely detached from the soft parts, floating in an excessively fetid sanious fluid. The first and fourth molar and the four incisors either fell out or were taken away by the fingers. The swelling of the cheek, over which there was a frequent livid flush, remained undiminished, but it yielded more to pressure; apparent fluctuation at one or two points, but no pointing of abscess externally. The lips and eyelids became œdematous; and during the latter weeks of the patient's existence, the left cheek also became painful and swollen. The affection had thus reached the stage of gangrenous destruction of the soft parts; the pain ceased almost entirely; but there was a gradual increase of the hectic symptoms; and in spite of tonic and antiseptic treatment, the patient died on the 73d day of the illness, worn out by hectic.

"*Post-mortem*, 8 hours after death.

"Extreme emaciation, right cheek swollen, soft, livid. On being detached from the edge of the lower jaw, a rusty, grumous, highly offensive, and greasy fluid exuded. Neither gums, periosteum, nor muscles of the cheek, were distinguishable. All those soft parts appeared to be dissolved in the grumous fluid, which was inclosed in the integument of the cheek as in a pouch. The right half of the inferior maxilla perfectly denuded, and void of all connexion with the soft parts in this grumous mass, so that after division of the capsular ligament, it was extracted without the least difficulty. In the lungs, tubercular deposition; but neither softening nor suppuration, which accorded with the previous symptoms, as nothing had indicated the second and third stages of pulmonary phthisis. No tubercles in the mesenteric glands. Anæmic and flabby state of the chief organs, the heart, liver, spleen, and kidneys; the blood in the large veins, very thin and blackish.

"*Description of inferior maxilla.* On the external and internal surface of the ramus of the right side, there is a deposition of new osseous matter, partly accumulated in large masses, partly in small isolated portions round the neck of the articulating process, without affecting the glenoid surface, extending along the external and internal surfaces of the condyle and the coronoid process to the angle of the jaw. The deposit is extremely delicate at the neck, and where it forms the detached portions, increasing in compactness and size as it descends, so as to present a thickness of from a line to a line and a half at the angle of the jaw, where the deposits of the two sides join, without at all points being in actual contact with the bone of the jaw. On the inner surfaces of the maxilla the deposit extends almost to the chin. At the alveolar processes of the incisors, there are three larger insular deposits. On the external surface this new formation reaches to the fourth molar, and there are smaller deposits on the alveolar process of the right carious tooth. There are also two delicate deposits on the left side in the region of the first and second molar.

"The new deposit is everywhere but loosely attached to the subjacent bone, has no organic connexion with it; the thinner portions may be easily removed by the nail, and the surface of the exposed bone appears perfectly smooth. Some parts of the former decay, and separate spontaneously, which, however, is not the case in the more compact mass attached to the angle of the jaw. The more delicate portions of the deposit present a porous structure, resembling a fine sponge, an appearance due to the innumerable vascular orifices. In proportion as the deposit descends, and becomes more compact, the sponginess diminishes, and at the angle of the jaw, the surface is much smoother, and the vascular orifices less numerous. The new deposit is partly of a dingy gray colour, partly having a yellowish, or a brownish, or reddish tinge. The alveolar process, at the right ˈd left last molar, is perforated, and looks corroded. The alveoli, which are

open, present nothing abnormal. The bone of the maxilla itself presents throughout a smooth surface, even under the new formation, and must be considered, so far as external signs serve as an indication, as a perfectly sound bone." (p. 148.)

The above case is one in which the disease ran a more rapid course than is usually observed; but otherwise it presents the symptoms which appear essential and common to all, in which the affection of the jawbone, either the upper or lower, constitutes the main point. The liability of the inferior maxilla would seem greater than that of the superior maxilla; for of the 22 cases extracted from Dr. Lorinser's account, we find 9 in which the upper, 12 in which the lower jaw, and 1 in which both were affected; of the Nürnberg 15 cases, 5 in which the upper, 9 in which the lower, and 1 in which both were affected; of Dr. Neumann's 8 cases, 3 were affected in the superior, 4 in the inferior maxilla, and one in both.

Of the remaining seven cases recorded by German writers, the disease was limited to the upper jaw in 4, in 2 it attacked both jaws, and in one the subject is not adverted to. If we arrange these statements in a tabular form, we obtain the following numbers:

No. of Cases.	Maxilla sup.	Maxilla inf.	Max. sup. and inf.	Uncertain.
52	21	25	5	1

We lay no further stress upon this result, than so far as it shows that the preponderance on the side of the lower jaw is not so great as the cases of Drs. Lorinser and Geist alone would indicate; and for that reason gives further weight to the other arguments adduced in favour of the purely local, and therefore to a certain extent accidental, origin of the complaint.

The exact commencement of the disease is not easily ascertained; for, as in the above case, we find that toothache invariably precedes the more severe affection, and that the patient attributes to his carious teeth, symptoms, which are often already a sign of deeper malady. We shall subsequently inquire how the carious state of the teeth may have predisposed to the development of the disease of the bone; but it appears, at present, to be an incontrovertible fact that this was a *sine quâ non;* since not only had all those patients who laboured under the disease, caries of one or more teeth, previously to their being attacked; but as long as their teeth remained sound, the affection did not show itself. The case of Rosine Meier is instanced by Dr. Geist, as a marked proof in point; she was distinguished by the beauty of her teeth when she entered the Nürnberg manufactory, and the disease did not appear until after her *dentes sapientiæ* had become carious. Roussel, the French writer above quoted, likewise states that those workmen remained in good health whose teeth were sound, although they were exposed to the noxious vapours for many years; whereas, all patients whom he was able to examine carefully, had one or more carious teeth, previously to entering the manufactory, or at least a considerable period before the appearance of the disease; and he, like the German writers, considers an unsound state of the teeth as an indispensable condition to the production of the jaw-disease. The toothache may intermit, and recur for a considerable period; the work is continued; and the person remaining exposed to the exciting cause, finds the pain gradually become more constant; it spreads over the whole affected side of the face; the cervical glands swell, and the cheek becomes tumid, red, and tense. The gums of the corresponding alveolar processes inflame, an abscess forms, which soon discharges fetid pus into the cavity of the mouth; and a sinus being once established, the livid gums retract more and more from the jaw-bone, the remaining teeth become loose and fall out, and more sinuses and abscesses form, through which the probe reaches the bone. In the same manner, abscesses may also form externally, and the diseased portion of the bone will now soon appear to have become totally detached from the other portions, and will force its way into the mouth. The disease may terminate in this exfoliation; but if the strength of the patient does not suffice to carry him through, the soft parts become still further involved, and as in the case we quoted at full, the ravages extend, until death terminates the scene.

The constitutional disturbance bears a direct ratio to the extent and stage of the local malady. At first there is but little fever; but there are loss of appetite, thirst, and derangement of the alvine functions, constipation being the most prevailing symptom. The gastric derangement may increase, but this, as well as the febrile symptoms, may disappear, so that we are unable to fix upon any constant type or expression of the general sympathy of the system, peculiar to this disease. Urinoscopy either is not in vogue in Nürnberg, or the patients presented no evident traces of an implication of the renal organs. Neither Dr. Geist nor Dr. von Bibra have in any way adverted to "the excess of phosphates," which Dr. Letheby has detected in the urine of persons occupied in the manufacture of lucifer-matches. We must not, however, omit to notice a difference in the progress of the complaint, according to whether the upper or lower jaw is the *sedes morbi*. We have seen that in the case of Barbara Keim, the processes, rami, and body of the lower jaw were more or less invested by a new morbid product. The manner in which this is deposited, increasing in thickness from above downwards, reminds one of a gravitating process; and it appears that the author himself surmised that the matter was first eliminated in a fluid state, and that, following the law of gravitation, it accumulated at the lower margin of the jaw. In the upper jaw, no new substance appears to be ever formed, after the bone has become denuded; "the superficial portion is invariably of a dingy blackish colour, the *lamina vitrea* is destroyed, and the spongy substance of the alveolar process is exposed. The bone appears rough, angular, sharp-pointed, extremely irregular, as if it were carious; it is filled with ichorous pus, and as this is discharged, small particles of bone escape, which give the pus a gritty appearance." Whether the pus, or the other matter discharged, contains the material out of which the new product on the lower jaw is formed, has not been ascertained; but the final result, the exfoliation of a more or less extensive portion of the maxilla itself, is the same in both cases, and is evidently the only means of cure, after the phosphorus has once fairly taken effect.

We have little to say of the diagnosis. The history of the case and the local symptoms clearly establish the nature of the disease. Dr. Lorinser, whose views are not adopted by Dr. Geist, assumes that the fumes acted by infecting the blood, and thus laying the primary foundation for a disease which remained dormant until an exciting cause fixed the spot for its outbreak; and he states a peculiar sallow, bloated complexion, combined with a dull expression of the eye, and with gastric derangement, to be diagnostic. The Nürnberg patients occasionally presented these symptoms; but in the majority of the cases they had healthy, florid complexions, which some retained to the last stage of the disease. The colour of the gums might give rise, especially when dysphagia supervenes from sympathetic affection of the parts involved in deglutition, to an assumption of syphilitic disease; but the fact that the phosphorus disease invariably attacks the bones first, would, with the history of the case, suffice to remove doubt on that score.

We have hitherto not proceeded in that systematic mode of analysis, which probably the authors of the work under examination would desire; but rather in that desultory manner which we deemed likely to embrace so much of the book as refers to the points at issue. Dr. von Bibra's contributions, which form the first part of the volume, we have not even as yet had occasion to speak of; although we by no means underrate their value. Still as his investigations are chiefly confirmatory of the more practical part of the treatise, and are condensed in such a manner—consisting for the most part of experiments and tables—as to allow of little further abridgment, we shall introduce such of his remarks as we may wish to present to our readers, in the course of our further observations on Dr. Geist's chapter regarding the pathology of the disease. But we should here state that the chemical and microscopical investigations of Dr. von Bibra, and his experiments as to the influence of phosphorus fumes on animals, are in a higher degree conducive to a proper elucidation of the disease in its different bearings.

Dr. Geist's second chapter (p. 210) on the causes of the affection, is one that deserves a deliberate examination; the more so as it can be the only means of arriving at a correct view of the malady, and of discovering the way by which it may be mitigated or avoided. The lucifer match certainly may soon be superseded by some other mode of ignition; and then, *sublatâ causâ*, the lucifer disease would probably cease. Still when we are told that a box of lucifers can be made and sold for half a farthing, leaving a profit to the manufacturer, it does not seem likely that anything much cheaper can be supplied. As yet, however, neither the lucifer manufactory, nor the match disease, is a matter of history; and it is therefore important to know what it is that gives rise to this new affection; for various conjectures *have* been made, and we find it attributed by some, and not without apparent reason, to rheumatic influences, to arsenic contained in the phosphorus, to phosphoric acid, and to the fumes of phosphorus, and to the lower degrees of oxidation of phosphorus.

Dr. Geist first deals with the question as to rheumatic influences. When the disease first made its appearance in Nürnberg, this view prevailed, owing to the extreme heat of the rooms in which the people worked, and their frequent exposure to sudden currents of cold air. In consequence of this opinion, precautions were taken to prevent the constant occupation of the workpeople in the hottest room; and while they were engaged in it, it was strictly forbidden to give rise to draughts by opening the windows. The disease, nevertheless, occurred as frequently as it had previously done. In the meantime, Professor Martius, of Erlangen, had discovered that the phosphorus employed in the manufactory in which the largest number of cases presented themselves, contained arsenic; and the conjecture arose that it was the arsenic and not the phosphorus, which produced those dire results. This view is advocated by Dupasquier of Lyons, who has observed none of the effects attributed to phosphorus, either in a phosphorus or in a lucifer manufactory, but has found that while sulphuric acid containing a proportion of arsenic was employed in the manufacture of phosphorus, the workmen were liable to painful contractions of the fauces, and to fits of vomiting and indigestion. As soon as pure sulphuric acid was used, these symptoms vanished.

"However, the local origin of the jaw disease," remarks Dr. Geist, in reference to this question, "which view of its nature soon came to prevail;—the specific relation of the phosphorus-fumes to the capillaries of the periosteum, which satisfactorily accounts for the development of the disease, and which, as we shall have occasion to see, was proved by direct experiments on animals;—the observations made by Fuchs on the influence of arsenic among the arsenic-smelters in the Hartz, among whom nothing at all resembling the phosphorus jaw disease has ever been experienced, either as the result of a general poisoning by arsenic, or as the immediate affect of an endosmotic process in bones exposed to the vapour of arsenic;—the very minute quantity of arseniuretted hydrogen contained in the fumes;—and finally, the circumstance that the disease has continued to occur, although for many years phosphorus has been employed which is not contaminated with arsenic;—all these reasons are too powerful objections for us still to continue to attribute to arsenic the causation of the disease." (p. 214.)

How far the chlorate of potash, which is extensively used in France and England, influences the health of the workpeople, Dr. Geist leaves undecided, though he admits the probability of its being the cause of complication of bronchitic and gastric symptoms.

The direct proof of the fact that the fumes of phosphorus are the cause of the malady, is offered by Dr. von Bibra; who has instituted a series of experiments on living animals, by exposing them to the influence of more or less concentrated vapour of phosphorus. The conclusion he arrives at is, that "when the fumes were very dense, they produced inflammation of the tissue of the lungs, and when less concentrated, bronchitis; that they enter the stomach and cause gastric disturbance; that they give rise to a decomposition of the blood; and finally, that, by direct contact with the periosteum, they cause it to inflame and to deposit new osseous matter." It is assumed that lower degrees of oxidation than that found in phosphoric acid are the agents in producing the affection of the jaw; as nothing of the kind is found in phosphorus manufactories, in which the atmosphere is impregnated with phosphoric acid. Dr. von Bibra thinks that the phosphorus volatilizes, and that the resulting combination with the oxygen of the atmosphere is hypophosphorous acid, to which the deleterious effects must be attributed. We cannot enter into the vague question as to whether Schonbein's ozone has or has not anything to do with the matter; although Bibra lays some stress on the observation, that there is a formation of ozone during the volatilization of phosphorus in the atmosphere.

It will now be intelligible why the workpeople employed in certain departments of the manufacture of lucifers are more liable to be attacked than others. The dippers are necessarily more exposed to the fumes of phosphorus, which, as the fused composition is poured on the slab before them, are evolved in great quantities immediately under their nostrils. The more rapidly the work is done, the greater will be the risk. The process of counting and packing also causes a considerable evolution of phosphorus fumes; and we find accordingly that in Nürnberg the counters and packers are affected like the dippers.

A singular instance is given by Pluskal (in the 'Œsterreichische Medicinische Wochenschrift,' 1846, No. 30), which proves that playing with lucifers may be productive of disease of the bone. A scrofulous girl, æt. 7, was in the habit of amusing herself by lighting lucifers and watching the phosphorescent appearance they exhibit in the dark; after a time a red swelling formed on the chin, which was gradually converted into an ulcer, discharging fetid pus. At the same time the gums and the front teeth became loose, salivation occurred, and in the course of a week the soft parts were destroyed down to the bone. Ten days later, three necrosed pieces of bone of the size of lentils were exfoliated; and on the succeeding days several small fragments followed. The sanious discharge gradually ceased; and three weeks afterwards the ulcer was healed. The author adds, that without medical treatment the affection would probably have made great ravages, and strongly recommends a solution of potash as the remedy which effected the cure.

We think that there is strong negative evidence contained in the above notices of the disease, in favour of its being a result of a direct effect of the phosphoric fumes upon the parts with which it is brought in contact. We have stated that the jaw disease was invariably preceded by carious teeth; and that all who had sound teeth were unaffected by the malady. Were it a constitutional malady produced by a general taint of the system, in consequence of absorption by the skin, the lungs, or the gastric mucous membrane, we should probably find other bones affected;* or we should see the jaws diseased without the establishment of a previous direct communication between the alveoli and the atmosphere. The only evidence wanting to render the proof complete, is that contained in the positive results obtained by Dr. von Bibra from his experiments upon animals. He introduced rabbits into wooden boxes, three feet by three, with a wooden rail in front, of just sufficient width to prevent the rabbits escaping. He divided the experiments into two series, which he terms chronic and acute. The temperature of the room in which the animals were kept, never exceeded 65° F.; and at the back of the cage was a Hessian crucible, in which a piece of phosphorus, of 25 or 28 grains, was placed, which was changed once a week, during which period about half had volatilized. In the experiments which he has termed the acute series, two thirds of the rail were covered up, so as to prevent the escape of the fumes; this measure was omitted in the chronic series. We have already mentioned the general results, as to the effect of the phosphoric vapour on the system; but in none of these experiments did the bones appear to be in the slightest degree affected. It is singular that in those animals which resisted the influence of the vapour longest, i.e., surviving for 25—30 days, instead of succumbing in from 8—12 days, when exposed to the more intense action of the fumes—an eruption showed itself on the abdomen, in the axillæ, and on the genitals. The rabbits, although continuing to feed well, became much emaciated; the hairs fell out, and the parts were covered with a thick crust, and exuded a constant watery discharge.

Dr. von Bibra's attention having been directed by Professor Dietz to the necessity of an exposure of the bone as a condition for the production of the deposit, he attempted to extract the teeth of rabbits; but these attempts either failed entirely, or the extraction was accompanied by a fracture of the maxilla. After repeated failures, two of these rabbits were exposed to the phosphoric fumes, on the chronic system. The inferior maxilla of the rabbit was broken on the right side between the first and second molar, in such a manner that (as the subsequent post-mortem examination showed) the root of the first molar was exposed; and by futile attempts at extraction, the third molar was broken off on a level with the alveolar process. The first and second molars of the other rabbit were extracted, but at the same time the maxilla was fractured at the corresponding spot. During the first few days, both animals showed signs of depression, but they shortly recovered their spirits, and even from the beginning of their imprisonment, they gnawed the turnips that were placed before them. After a time, a considerable swelling formed at the injured part, and the animals died, one soon after the other, after having been caged for a little more than eight weeks.

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* We scarcely think this a valid argument; since many constitutional diseases, plainly dependent on the existence of poisons in the blood, are no less localized in their results,—as shown in the review of Mr. Paget's Lectures, pp. 405-6 of our present number.—Ed.

"The post-mortem examination yielded the same results in both, as have been exhibited in the previous experiments, the chief character being a congestion of the venous system, a not very considerable injection of the muscular organs, and hepatization and tubercular deposition in the lungs. In the first rabbit, which had a fracture of the right side of the lower jaw, considerable inflammation of the soft parts was visible after removal of the integument; near the bone the muscles were in a state of suppuration, and partly infiltrated with a blackish, sanious matter. The yellow pus showed the ordinary pus-globules under the microscope; but in the dark substance no distinct forms were perceptible, and it appeared to consist of mere detritus. After the soft parts and the pus had been very carefully removed, the periosteum appeared almost detached (i.e. easily separable from the soft parts), and evidently inflamed; and underneath it there was a deposit of a new formation both on the external and internal surface of the bone, of about 3 or 4 lines in length. The extremities of the deposit were firmly agglutinated to the bone; but towards the middle it was easily detached, leaving the bone clean and smooth.

"The edges of the fracture itself were not united, and although the two pieces of bone did not fall asunder, this was only prevented by the new deposit and the periosteum; and when an experiment was made to test the strength of the preparation, it broke with facility.

"The question now arises as to whether this deposit was callus, or identical with the peculiar bony deposit presented to us in the lucifer disease; I am inclined to take the latter view." (p. 72.)

Now, in order to appreciate Dr. von Bibra's arguments, which are adopted throughout by Dr. Geist, it will be necessary to revert to his descriptions of the deposit found in the maxillæ of the workpeople who had suffered in consequence of exposure to the phosphorus fumes. We much regret our inability to present to our readers the elegant and careful drawings which accompany the work, and which are almost necessary to a proper understanding of the ensuing extracts. Dr. von Bibra, who has devoted much time to the chemical and microscopical examination of bone, has introduced some valuable remarks on the normal structure of the tissue; but as our space is limited, we must confine ourselves to the following summary of the nature of the new formation:

"The Haversian canals exhibit in part a larger diameter than those of normal bone, and are empty, except where the deposit appears smooth and compact, and is partially covered by periosteum. They are not parallel with the general direction of the bone, but are placed at right angles to the latter; they interlace with one another, sometimes expanding to form sacs, sometimes contracting, and end with open mouths on the surface. These mouths are more minute in the most recent deposit, and appear larger in older layers. The bone corpuscles are rounded off or angular, and their circumference is less decided; during the progress of the formation of the deposit, they are very large and their contour proportionably undefined. They appear filled and dark coloured; at first they are lighter, and they have ramifications like those of normal bone, which increase in number with the age of the deposit.

"The fundamental structure of the deposit is laminated; and several layers are distinctly seen resting upon one another. It exhibits rents (risse) with which the ramifications of the corpuscles are connected, and which may therefore be considered as continuations of the latter. Spots are also visible here and there, which Von Bibra looks upon as accumulations of earthy matter. This matrix of the new deposit is at first very brittle; after the deposit has been exposed to the process of absorption, it shows a powdery appearance, as if sprinkled with a coarse powder.[*]

"The chemical characters exhibited by the deposit, and by the bone itself, are best shown in the comparative view afforded by the following table." (p. 266.)

Bone of Lower Jaw.					New Deposit.		
Name.	Duration of disease.	Remarks.	Organic matter.	Inorg. mat.	Remarks.	Org. mat.	Inorg. mat.
Barbara Keim	73 days . .		. .	. .	. . .	48·78	51·22
Kunig. Burkhart	6 months .	uncovered . .	30·56	69·44	. . .	30·80	69·20
		covered by deposit	29·73	70·27			
Mary Rumpler	1½ year . .	1, left maxilla .	33·62	66·38	. . .	43.09	56·91
					Deposit partially reabsorbed . .	46·47	53·53
	9 months . .	2, right maxilla .	33·34	66·66	. . .	37·50	62·50
Maria Höger .	10 mos.—1 yr.		30·20	69·80			
Susan Huck .	1 year . .	excised portion .	31·18	68·82	. . .	34·68	65·31
	1 yr. 9 months (after death)	coronoid process .	31·30	68·70	Different portions of deposit	34·62	65·39
						40·00	60·00
Mary Klein .	1 yr. 4 months	exfoliation . .	39·29	60·71		27·42	72·58

Dr. von Bibra finds that the peculiarities of the osseous deposit in man are traceable in the new formation, artificially induced in rabbits. The external appearances in the two entirely correspond, except that in the cunicular maxilla the deposit is finer than in the human jaw. Both have the porous, spongy appearances above noticed; and their microscopical characters also agree with regard to the circular corpuscles and the Haversian canals. This new formation is not to be confounded with callus; we have already seen that it is but loosely connected with the true bone; there is no fusion of the Haversian canals, no transition from those of the bone to those of the new deposit, as is the case in callus, which is always firmly connected with the former.

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[*] The reviewer was fortunate enough to obtain from a man, who has lost two-thirds of his under-jaw by exfoliation, in consequence of the effects of the phosphoric fumes, a smooth piece of hard substance, of the size of a filbert, which the patient stated to have been detached from the inner surface of the chin. Upon examination under the microscope, it did not present the features of osseous structure, but appeared quite amorphous. Being treated with hydrochloric acid, it left an organic residue, which was equally destitute of all traces of organization. In this individual the whole lower jaw, with the exception of a portion of the left ramus, has been regenerated, including the left articulation. He had himself, as the bone protruded, sawn it across and taken it out by the mouth. As he considered the specimens as a curiosity, he would only part with the above-named new substance.

The general conclusions, which Dr. Geist arrives at relative to the proximate cause of the disease, are that it must be viewed as periostitis, and that the affection of the bone is merely a secondary consequence. He considers the direction of the medullary canals in the deposit, which is vertical to those of the bone, and the loose connexion between the two, as proofs that the new formation derives its origin from the periosteum only, and that it is in no way produced, or its production aided, by the bone. In reference to the deposit he remarks that it must be considered as possessing a lower degree of development than true bone. The abundance of organic material, as compared with the constitution of bone, is apparent from the table we have just given, of which the following numbers are the average:

BONE.		DEPOSIT.	
Organic constituents . .	31·42	Organic constituents . .	38·16
Inorganic ditto	68·58	Inorganic ditto	61·84
	100·00		100·00

In what relation this deposit stands to the new bone, in those cases in which exfoliation takes place, and recovery with regeneration of the osseous tissue ensues, our authors do not state; but it does not appear unreasonable to surmise that it is capable of further development, and that an absorption of the excess of animal matter may occur, as the action of the phosphoric vapours ceases, and the purely physiological process of reparation is induced. The power of reproduction is indeed more surprising in the human inferior maxilla, than in any other part of the frame; and the chief indication in the treatment of these cases appears to be to support the strength of the patient, so that the *vis medicatrix* may be enabled to throw off the noxious influences. It is evident that, from the long-continued operation of the causes, previous to the actual appearance of the jaw-disease, little can be done to eliminate from the system the phosphorus which has been absorbed. The necessity of antiphlogistic treatment is self-evident during the commencement of the local affection; although, from the necessary duration of the complaint, it must be important to avoid reducing the patient's strength too much. We need scarcely say that any operative proceedings would be improper, so long as the disease is running its course. Experience has demonstrated the uselessness of any surgical interference; and we can only assume that ignorance of the real nature of the disease could permit an attempt of the kind. The treatment of the actual disease must therefore be mainly symptomatic; and it will be unnecessary to enter into the consideration of the value of different remedies, as every case must be judged of in its separate bearings. We confess ourselves surprised that Dr. Geist should not have adverted to the necessity of copious ablution and perfect cleanliness, as an important point in the treatment; since one of the indications must evidently consist in removing as much phosphorus as possible. Whilst carrying out our personal inquiries into the nature and symptoms of the disease, a young man presented himself for examination, who was labouring under the effects of the fumes. Now, although he had not been engaged in the manufacture of lucifers for eighteen months, he smelt so strongly of phosphorus that he impregnated the atmosphere of the room; and the writer was able to perceive the smell of phosphorus in his own coat two hours later, after a walk of a mile and a half. This person had never taken a bath; and from his extreme poverty had probably scarcely changed his clothes during the eighteen months. Now, when we consider the extent of surface by which noxious matter can be removed from the blood through the skin, but which was probably rendered incapable of performing its functions by the accumulation of impurities, we should consider it one of our own first duties to secure the free elimination of the cutaneous perspiration, as well as to promote the action of all the other emunctories. It is not at all improbable that diuretics might prove more serviceable in the treatment of the disease than purgatives; however, as we do not wish to deal in theory, we leave these questions to be decided by future writers, who may have better opportunities of watching the rise, progress, and development of the disease.

It must be evident that much more important results are to be obtained by attending to the cause of the malady, and by anticipating its effects, than by attempting to cure or palliate them. Dr. Geist discusses the prevention of the disease in the forensic portion of the work, and makes the following suggestions:

"1. In those manufactories which produce such a large quantity of lucifers, that the process of drying is constantly going on, and therefore giving rise to a constant evolution of fumes, the drying-room should be entirely detached from the other workrooms.

"2. In those manufactories in which the drying is effected during the night, or during the absence of the workpeople, the drying-room may communicate with the other workrooms, and it will be merely necessary to air the former well, after the drying process is completed, to provide the room with a ventilating shaft, and not to employ it as a workroom. In both cases the drying should not be carried on at a higher temperature than 65° F.

"3. The composition should not be made, and the dipping not conducted, in the presence of other workpeople, but in a detached room.

"4. The counting and packing-room should be well ventilated, and not be too much crowded with workpeople.

"5. The same to be the rule with regard to the room in which the matches are arranged in frames.

"6. All the rooms must be ventilated three times a day for an hour at a time by opening the windows and doors, viz. before the work begins, during dinner time, and after the work is over.

"7. The workpeople to be prohibited keeping their victuals and consuming them in the workshops, because the fumes combine with them, and by being introduced into the stomach, give rise to gastric disturbance.

"8. The purification by ignition of the frames, crucibles, and other utensils, to which phosphorus and sulphur remain attached, should be prohibited." (p. 322.)

The attention which the continental governments, and more especially those of Germany, have long paid and continue to pay to sanitary questions is well known; but the English manufacturer dislikes this kind of paternal supervision; and the difficulty of insisting upon measures of precaution, which generally involve a primary outlay of capital, is notorious. We have found in the course of our inquiries a frequent neglect of all precautionary measures; but in the case of small manufacturers, it does not appear that a corresponding amount of injury ensues. A considerable amount of phosphoric fumes is requisite; and a continued and uninterrupted occupation in the impregnated atmosphere appears to be a necessary condition to the production of the jaw disease. We are happy to say, that what even the government measures have not yet effected in Germany, we have found carried out in a large manufactory in the city, in consequence of the wise and benevolent views of the proprietors. We would advert to this case the more, as both from our personal examination, and from the testimony of a distinguished physician who has long been acquainted with the parties, we are able to vouch for an absence of all collusion. We would advert to it also as a proof, that even among the lower orders a knowledge of the value of sanitary arrangements, and a due appreciation of their bearings, is gradually making its way, and enforcing conviction, in a manner which affords the most gratifying proof that the labours of our profession and others have not been thrown away. The proprietors of the lucifer manufactory in Princes square, Finsbury, employ fifteen girls and fifty boys, some of whom have been engaged there for eight years, and eleven men, some of whom have worked there for ten years, and no case of the disease has occurred among them. We saw one girl who had been exclusively engaged in dipping matches for seven years without being ill, and who still looked perfectly healthy and robust; others were pointed out who had equally preserved perfect health, although engaged in the manufacture of the matches for many years. The precautions used are, that the workpeople are required to wash their hands night and morning in soda, which our informant assured us, was the only means which completely removed all the phosphorus; they receive tea or cocoa night and morning, eat their meals at the manufactory, and work from morning to night without going home. The dippers, with the exception of the girl, above mentioned, wear sponges before their mouths. Still even these precautions would scarcely have prevented the influence of the fumes, unless proper means had been taken to prevent their accumulation; and this was done so effectually, that although our visit was quite unexpected, and occurred while the workpeople were all fully engaged, we scarcely perceived any unpleasant smell. The ventilation had been effected at a considerable outlay, by the introduction of large and numerous ventilating shafts, so that it is constant and effective. It is evident that a temporary opening of the windows must be inefficient to remove fumes which are being permanently and copiously evolved; it is only by providing a regular circulation of pure air, that they can be carried off, or sufficiently diluted. In the hope that the statement may be of service to others who labour in the cause of sanitary reforms, we shall conclude our analysis of Dr. von Bibra's and Dr. Geist's treatise, by giving the measurements of the ventilating shafts in the above-named manufactory. The names of the different rooms at once indicate the separation of the different stages of the manufacture from one another, and the objects to which they are specially devoted.

Sulphuring-room—open, shaft	6 ft. by	4 ft.
Cutting-room—open—two fanlights to open . . .	5 3	3½ 2
Packing-room—open—two fanlights . . .	4 3½	3½ 3
Filling (clamp) room—open—two fanlights, each . .	4	2

The above, although in separate uses, are all open and on the ground-floor, and extend the whole length of the building.

Mixing-room—shaft	3ft. by	3 ft.
Fanlight to open	4½	3½
Filling-room, up stairs—two shafts	5	4
	4	3½
Three fanlights to open	5	4½
	4	3½
	3½	3½

Five windows, all of which open about 4 ft. each way.

Drying-room—four windows, all of which open, and no one employed in that room, except two, who take in to dry, and fetch out when dry.

These rooms extend the whole length of the building.

Art. IX.

On the Causes and Treatment of Abortion and Sterility: being the Result of an extended Practical Inquiry into the Physiological and Morbid Conditions of the Uterus, with reference especially to Leucorrhœal Affections and the Diseases of Menstruation. By James Whitehead, F.R.C.S., Surgeon to the Manchester and Salford Lying-in Hospital.—*London*, 1847. 8vo, pp. 426.

We now proceed to examine the latter portion of Mr. Whitehead's treatise, in which alone the title of the work is strictly appropriate.

Statistics of Abortion. There are few facts better known in midwifery, than that abortion is a frequent accident of pregnancy, and that it is more common in the early months, between the third and fourth, than at any other time. But this simple statement of an acknowledged truth is not enough for Mr. Whitehead; who brings some very ponderous-looking statistical tables, with a striking of averages, to bear upon it; with the only result, however, of rather muddling these facts, and encumbering and obscuring the truth. Mr. Whitehead frames a table from the experience of 541 married women in Manchester, to show that the average age at which child-bearing commenced was 21½ years,—that the average period of conception was 2½ months after marriage,—and then comes the practical corollary, that "a wife in this climate, placed in favourable circumstances and in the enjoyment of health, may expect to bear children during twenty years,—namely, from the age of 21½ to 41½." What witch told Mr. Whitehead this? But we would put it to him whether, when he ventures to derive a general conclusion from numbers—the conclusion having the apparent precision and the incontrovertible accuracy which is the main value of the arithmetic,—he is quite sure that a larger number, say 50,000 instead of 500, would not yield a different result? In this table he has taken the females from the Manchester and Salford Lying-in Charity, and from his private practice, as the representatives of the women throughout England; and we hardly know how many more countries the word climate may include. Surely, he might as well strike an average of the number of fish in the Atlantic, from the contents of a small pond in his neighbourhood. Our own conviction is, that the results of statistics of this kind are fluctuating results; and the facts and truths, which they profess to substantiate, bear with them the monstrous paradox and fiction of being fluctuating truths and varying facts. This same source of fallacy belongs generally to Mr. Whitehead's statistics. He collects the histories of 2000 pregnant women (a most arduous task, and most praiseworthy in him), and he infers, as an average, that 37 per cent. of mothers abort before the age of 30 years; and that 87 per cent. of married women, who have reached the final menstrual crisis, have aborted during the child-bearing period. Mr. Whitehead, himself, thinks that the first is rather under the real average, and that the numbers from which he derives the last conclusion are "perhaps too limited." It is not without having ourselves endeavoured to work out conclusions on which we might confidently rely, on the very subjects which have engaged Mr. Whitehead's attention, and with a field at our command perhaps even larger than his, that we make these remarks. But the sources of error, particularly those which are inseparable from the testimony of the female, multiply as the inquiry proceeds; and what is doubtful, or even false, gets so mixed up with what is true, that it is difficult and almost impossible, on a large scale, to separate and record the latter alone.

Causes of abortion. Our author uses the term abortion in its widest sense; implying an escape of the ovum, before the full period of utero-gestation is accomplished, whether it takes place at the first month, or the eighth month, or any intermediate period. The causes of this most common accident of human pregnancy have been recorded with much care by most practical writers on the subject. Numerous predisposing and accidental causes have been particularized, and are well known. Of the predisposing causes, some attach to the mother, and others to the fœtus or its appendages. Of the former, those deserving the most study are connected with lesions of the utero-placental circulation, the poison of syphilis, and the diseases of the body and neck of the uterus. The latter, in Mr. Whitehead's opinion, less frequently originate in the fœtus, as a primary inherent morbid action, than are occasioned by some faulty state or disease of the uterus itself.

> "I can state, without hesitation, that in five out of every six instances of what are denominated 'blighted ova,' disorganized placenta, and hypertrophy or other abnormal condition of the membranes, organic disease of the uterus has been met with; and this, on inquiry, was generally proved to have existed at a period anterior to that from which the defect discovered in the aborted organs could reasonably date its commencement. There appears to be no reason for doubting, that not only disease of the product of conception, but also arrest of development, and consequent organic deficiency or malformation, are the direct result of a faulty condition of the organ or of the constitution upon which its growth and existence depend, and not the effect of any inherent imperfection, or of merely fortuitous causes." (p. 252.)

The influence of aloetic or strong cathartic medicines in disturbing the ovum and causing abortion, has been always included amongst the *accidental* causes; but Mr. Whitehead, with great truth, we think, entertains some doubt as to this result, unless some powerful predisposing causes are coincidently present. The most alarming symptoms, he says, are sometimes induced by the use of the bitter-apple, savin, rue, foxglove, and other drugs, administered with criminal intention, without in the least degree disturbing the function of the uterus during pregnancy. Even the ergot of rye is by no means uniform in its action. Mr. Whitehead relates a case in which abortion was successfully induced by it at the fifth month in a woman with great deformity of the pelvis. "It was given in three successive pregnancies; and, in each instance, labour-pains came on after eight or ten doses, and expulsion was effected at the end of the third day. It was perseveringly tried in a fourth pregnancy, in the same individual, and failed completely."

The following table of 378 cases of abortion, arranged so as to show the probable causes of the accident, or the diseased states associated with it, affords a brief epitome of this part of Mr. Whitehead's treatise (p. 256):

Causes of, and conditions associated with Abortion, in 378 *cases.*

Accidental agencies	44
Placenta Prævia	8
Constipation of the bowels	3
Retroversion of the uterus	1
Incurable disease	1
Vascular congestion	15
Disease of the lower part of the uterus	275
Obscure causes	29."

The first five divisions are but briefly discussed, and do not contain anything worthy of comment. We think that Mr. Whitehead ought not to speak quite so positively of a central implantation of the placenta over the os uteri being "almost invariably" attended by abortion before the fifth month; for our own experience does not accord with this positive assertion. Placental presentations, however, are hardly to be included within the subject of abortion, and we shall forbear, therefore, for the present, to speak further upon them.

Congestion of the uterine circulation. We believe that a congested state of the utero-placental circulation, and an extravasation of the mother's blood beyond its proper limits, is a far more frequent cause of abortion than Mr. Whitehead's table seems to show. Mr. Whitehead himself thinks that "its real average is somewhat greater;" and we cannot help fancying that many of the cases of abortion with disease of the os and cervix uteri were really *caused* by a repletion of the venous system of the womb. We might refer to Dr. Ingleby's work on 'Uterine Hemorrhage,' for a very good account of this subject; and we would express our conviction that, as a cause of early abortion, it prevails to a far greater extent than is generally admitted or supposed. A very interesting case (xxvii) is recorded by Mr. Whitehead, which expresses very graphically the symptoms, treatment, and probable cure of this source of miscarriage; and incidentally mentions a mode of relieving the painful venous swellings about the anus and vulva, which is well worthy of attention. The first five pregnancies in this patient terminated in abortion between the fourth and sixth months. "Each of these abortions had been preceded by distension of the abdomen, pain of the loins, piles, swelling of the labia pudendi, throbbing of the hypogastrium, and convulsive struggles of the fœtus in utero." In her sixth pregnancy she fell under the care of Mr. Whitehead, who met these symptoms by moderate venesection, repose, and anodynes, the effect of which was for a time successful. They recurred at two subsequent periods during the same pregnancy, and were again successfully combated in like manner; and the patient was delivered of a well-grown but still-born child, nearly at the full term of gestation. In a seventh pregnancy the same symptoms recurred twice, and were again relieved in the same manner; and a well-grown living child was brought into the world at the full time. The same symptoms recurred in an eighth and ninth pregnançy, each of which was brought to the same favourable conclusion, though the local congestions were more aggravated; but these, although scarcely at all benefited by topical bleeding, were so completely and instantaneously relieved by the application of a solution of nitrate of silver, that (as Mr. Whitehead was afterwards informed) the question of his integrity became a matter of serious discussion among the patient's friends, who inquired, naturally enough, why this remedy had not been earlier adopted.

By far the most important part of Mr. Whitehead's treatise on Abortion, which he appears to have worked at with much diligence, and to have elucidated with great general success, is that which treats of diseased states of the uterus, particularly of its vaginal portion. It must be acknowledged that this subject has not, until lately, received that attention which it so deservedly merits; and we think that Dr. James H. Bennet is fairly entitled to the credit of having taken the initiative, in bringing it before English practitioners. The importance and frequency of these complaints of the cervix, and their general treatment, were well known in France before Dr. Bennet's book appeared; and to our own knowledge several obstetric practitioners, both in London and Dublin, had noted and even figured the appearances of the os and cervix under different states of inflammation and ulceration. The use of the speculum, which at first was kept back by its supposed indelicacy, was being generally adopted; the diseases of the cervix, which are rendered by its aid so perfectly obvious, were beginning to be understood; and Dr. Bennet's book came out at the very nick of time, so that he singly took possession of the ground, which, in a short time, would probably have been occupied by more than one independent observer. We make this remark, because it appears that Mr. Whitehead, like Dr. Bennet, had learned, as a pupil at the French hospitals, the value of the employment of the speculum in uterine disease; and it had suggested to him an extended, and we may fairly add an original, inquiry into this class of complaints.* Mr. Whitehead's treatise is laid out upon a much broader scale than Dr. Bennet's; a far larger collection of materials has been necessary to compose it; and had he been contented to publish his cases, as they arose, in a weekly journal, with clinical remarks attached to them, he might have gained some brief reputation for priority of observation; but he would have missed his great object of writing a substantial and systematic work. The influence which the diseases of the cervix exercise on the gravid womb, in occasioning a premature expulsion of its contents, has been dwelt upon by Dr. Bennet in some papers and lectures published in the 'Lancet;' but in justice to Mr. Whitehead, we feel bound to say that his chapters on this subject bear intrinsic evidence of a long-continued patient investigation, worked out by his own industry, after his own design, and altogether unsupported and uninfluenced by any other observations than his own.

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* Since the above was written, we have seen a correspondence in the Lancet between Dr. Bennet and Mr. Whitehead; and the communication of the latter gentleman appears to confirm the opinion we have expressed.

The great prevalence of disease of the cervix in pregnant women, and its connection with abortion, may be judged of by Mr. Whitehead's observation, that it was found to exist in 275 out of 378 cases. These numbers are startling; and, we must candidly admit, far exceed our own experience on the subject. We have so constantly found sterility accompany ulceration of the cervix, that, although we have been long aware of the frequent occurrence of exceptional cases, we were hardly prepared for so formidable an array on the other side. Mr. Whitehead appears, however, to have examined almost all these women by speculum, either before, or within three or four weeks after, the abortion took place. But it strikes us at once, that the disease of the cervix in those who were examined after abortion, was probably, in a majority of cases, induced by the abortion, and was not the cause of the accident. We have so constantly noticed ulceration of the cervix follow and to be dated from an abortion, and the stretching which the cervix, in its undeveloped state, undergoes during abortion so readily accounts for it, that, like Dr. Bennet, we have looked upon this kind of relation as established. We cannot help thinking that there has been some confounding of cause and effect in Mr. Whitehead's cases. No less, however, than one hundred and forty-one of these women suffered the same symptoms in a second, and, in some cases, a third pregnancy, and by a preconcerted arrangement, were again seen and examined by Mr. Whitehead. When disease invades the uterus, there are some signs which are pathognomonic, and others which are accidental, affecting the general health.

> "Of the former class are: 1st. Leucorrhœal discharges, whether these be simply of a mucous character, or mixed in variable quantity with pus, sanies, or blood,—provided the latter be not the natural product of menstruation; accompanied, 2dly, with an indefinable, deep-seated aching of the lower belly. 3dly. A fixed pain of peculiar character, on one or both sides of the body, near the groin, occupying the situation of the inguinal canal, and being, generally, unaccompanied with swelling. 4thly. Aching of the loins, implicating the region of the kidneys and upper part of the os sacrum. 5thly. An involuntary and uncontrollable inclination to compress the lower abdominal viscera, by an effort which is expressively denominated 'bearing down.' 6thly. Rigors, lassitude, and remittent feverishness." (pp. 279-80.)

1. Our author devotes a chapter to a full consideration of these symptoms; and particularly to the first, leucorrhœa. He notices two varieties of this discharge; viz. mucous leucorrhœa, and purulent leucorrhœa. The former appears under two aspects; first, as a glairy, transparent discharge, like white of egg, which has an alkaline reaction, and is furnished principally by the cervix of the womb; and, secondly, as a perfectly white, opaque discharge, about the thickness of cream, and intensely acid, which is the product of the mucous membrane of the vagina. Purulent leucorrhœa is more frequent during pregnancy; it is a product of suppurative inflammation; it has an alkaline reaction, except when neutralized, or even rendered faintly acid, by a free mixture with vaginal mucus; it has a yellowish or greenish colour; and it stains the linen deeply, the stains not being easily removed. This form of leucorrhœa marks the presence of hypertrophy or ulcerations of the cervix uteri; and it is this condition of the uterus which is a common cause of abortion. A question of great practical interest, is whether purulent leucorrhœa may occasion symptoms like

blenorrhagia in the male; and Mr. Whitehead, like most men who have examined the question practically, does not hesitate, from his own experience, to answer in the affirmative. Its effect on the conjunctiva of infants is well known; and some interesting examples are related by Mr. Whitehead. But to decide the former question by experiment, he collected some of the matter from the surface of a granulating sore of the cervix, fourteen days after delivery, in a lady whose infant had purulent ophthalmia; and he applied this under the upper eyelid of a full-grown terrier dog.

> "At the end of the first day, the eye appeared dim, dull, and heavy; and there was slight vascular fulness. At the end of the second day there was evidence of inflammatory turgescence, the eyelids being swollen, and the ocular conjunctiva suffused: the animal appeared wishful of shunning the light. On the fourth day there was violent inflammation of the whole conjunctiva, with a plentiful secretion of a greenish, yellow pus. This affection was soon subdued by the application of the solid nitrate of silver, and subsequently by an opiate and aluminous collyrium." (p. 319.)

2. The second symptom, viz. deep-seated aching of the hypogastric region, is not a fixed pain, and is not increased by pressure; but it is felt sometimes behind the pubes, at others on one or other iliac fossa, sometimes between the two, and at others it is referred to the sacrum.

3. Fixed pain of one or both groins occupying the situation of the inguinal canal. We quite agree with our author in regarding these as nervous pains, connected with the round ligaments. It is, as he says, sometimes a dull aching pain, at others a stabbing pain; sometimes well defined and within a small compass, at others more extended and less distinct. We have often heard it spoken of as 'like a gathering,' but it is "generally traceable along the ramifications of the musculo-cutaneous, inguino-cutaneous, and external pudic nerves," and there is not any swelling or soreness, or pain on pressure. With reference, however, to the absence of swelling, Mr. Whitehead relates an exceptional case, in which a lady who had inflammatory induration of the anterior lip of the uterus, with disorder of the general health, complained, amongst other symptoms, of a deep-seated pain in the left inguinal region, and a painful swelling, the size of an orange, in the left side of the abdomen, which came on suddenly and simultaneously with pain of the stomach and palpitation. On one occasion Mr. Whitehead had the opportunity of examining this tumour, which he found to be the size of a pullet's egg, and situated half way between the left inguinal canal and the umbilicus. It disappeared under pressure and friction; and he convinced himself that it was caused by cramp of one of the inter-tendinous divisions of the rectus muscle on that side of the body. Mr. Whitehead looks upon this sign as "unerringly symptomatic of inflammation, ulceration, or induration of the anterior labium, or of the corresponding part of the cervix uteri." We know this symptom well; but although we have met with it in the latter cases, we have seen it well marked in hysterical women suffering only from vaginal leucorrhœa, with irregular or deficient menstruation. We lately saw a case of this kind, in which the so-called tumour had imposed on a medical friend; but it turned out to be nothing more than a gathering up of the lower compartment of the rectus muscle, which subsided when a warm sedative poultice was applied over it. We may mention that we have found these inguinal pains much relieved, when severe, by making a caustic

issue over the external abdominal ring, and dressing it afterwards with an ointment containing morphia.

4. Our author cites the anatomical arrangement of the nerves, which Dr. Lee thinks that he has made out, as explaining the lumbar, sacral, renal, hip, and thigh pains, which so constantly attend uterine diseases. He mentions, also, "an acute, smarting, or stabbing pain of the coccyx," or a central perineal pain, involving the lower fibres of the sphincter ani muscle, and accompanied with a compressing—but not a tenesmic—effort; and he regards it as a sign of "inflammation of the posterior labium uteri, or of fissured ulceration of the same part, or of one of the commissures."

5. The effort of "bearing-down" is a sign of uterine disease, which pressure alleviates. It is aggravated by moderate distension of the bladder, or feculent accumulation in the rectum; and the instinctive effort to strain procures for a time a sense of relief.

6. Rigors, lassitude, and remittent feverishness are looked upon by Mr. Whitehead as a sign of the blood being contaminated with pus.

> "That the purulent product of uterine disease is constantly liable to be returned into the circulation, is sufficiently proved by the fact, that, after its formation, it is detained for an indefinite period, upon the surrounding mucous surfaces, in which the process of absorption is always in active operation; and the constitutional irritation which is generally set up under these circumstances, bears forcible evidence of such transference having been effected. The condition of the system thus created, eminently predisposes it to violent attacks of disease from comparatively trivial causes, against which it has no adequate power to contend; and acute inflammatory affections and fever are more frequently attended with disastrous consequences, and arrive at a speedier issue, in constitutions thus tainted than in others." (pp. 298-9.)

Diseases of the gravid uterus considered as causes of abortion. Mr. Whitehead enumerates ten diseases of the uterus as particularly occasioning abortion; and he has arranged them according as they predispose to abortion at an early or a late period of gestation. This classification, as he says, is purely arbitrary. The following are the diseases:—

1. *Inflammation and superficial erosion* of one or both labia, and the external and internal surface of the cervix. Abortion usually at the seventh, eighth, or ninth month of pregnancy.

2. *Varicose ulceration*, generally affecting the back part of the anterior labium. It mostly causes abortion after the period of quickening.

3. *Œdema of the womb.* Abortion sometimes occurs at the latter months of pregnancy.

4. *Fissured ulceration*, with inflammatory hypertrophy of the labia. It predisposes to abortion about the middle period of pregnancy.

5. *Induration* of the cervix. Abortion at the third or fourth months of pregnancy.

6. *Endo-uteritis.* Abortion frequently during the first few weeks, or in the second or third month of pregnancy.

7. *Follicular ulceration* sometimes constitutes the leading feature of uterine disease in cases of abortion.

8. The *gonorrhœal virus.* The inflammation it occasions implicates the labia and adjacent cervix, and is especially liable to be extended to the lining membrane of the uterus. It is a frequent cause of abortion at an

early or late period, according to the extent and severity of the affection.

9. *Syphilitic disease*, in its primary, secondary, and tertiary stages, is capable of producing abortion at any period of the process; but most frequently, perhaps, at the sixth and seventh months.

10. *Prolapsus uteri.*

These diseases are fully and separately described by our author; and the treatment of each is included in the cases which illustrate them. The first of these diseases is very common, occurring in twenty-six per cent. of Mr. Whitehead's cases. He has described the appearance of the cervix, as we have often seen it, most accurately; and he observes that its perfect cure may be long delayed. The ulcer heals from its outer edge, and the margin towards the os uteri is the last to cicatrize; and if this be left uncured, the ulcer may again spread. Mr. Whitehead notices that inflammation of the neck and lips of the uterus without abrasion may occur as an acute disease during pregnancy; being attended with constitutional disturbance, aching of the pubes, and irritable bladder. The affected surface has an erisypelatous look; the cuticle appears tense and shining, but it sometimes breaks and peels off, leaving patches of excoriation, which suppurate, and not unfrequently pass into the granulating ulcer. We quite agree with Mr. Whitehead, in thinking that this is a common cause of the superficial red ulcer of the cervix. The treatment requires attention to constitutional remedies, which ought to be of a sedative and alterative kind; depletion by leeches, if the patient's strength will bear it; and the application of nitrate of silver to the sore at short intervals. Mr. Whitehead notices the immunity from danger, either to the mother or child, in the application of caustics to the lower part of thè uterus during pregnancy; a fact which Dr. Bennet had quite established.

Mr. Whitehead thus admirably describes the *varicose ulcer*.

> "It is generally met with in women of the bilious temperament and hard fibre, who have been subject to piles and profuse menstrual discharges, and to derangement of the biliary organs. The premonitory condition of the parts consists in a hardened and hypertrophied state of the cervix, which is traversed in various directions by a number of tortuous, dark-coloured trunks, about the thickness of a probe or a crow's quill, raised above the surrounding surface. Larger and more prominent points are here and there noticed, indicating the situation of inosculation of one branch with another; and, generally, at one of these points the ulcerative process is set up, which soon extends through the coats of the vessel, and escape of blood, in greater or less abundance, immediately ensues. The ulcer, which is not long after in being developed, presents an uneven, livid aspect, with irregular margins, near which a few tortuous vessels may be seen ramifying; it now secretes a quantity of pus, and often has small dark clots of blood or fibrin, the size of a pin's head, lying loose upon the surface. It usually occupies but one labium, the anterior more frequently than the posterior; but sometimes the whole circumference of the cervix is implicated." (p. 324.)

Mr. Whitehead remarks that the lesion of the veins of the cervix sometimes extends itself, inducing a general uterine phlebitis, which almost inevitably occasions abortion. We do not know this disease ourselves as occurring during pregnancy; and we wish that Mr. Whitehead had related the case or cases, on which he founds this important remark. The varicose ulcer causes much local and constitutional distress; and the discharge which accompanies it is at first glairy, then brownish and purulent.

Spurious menstruation, during pregnancy and lactation, is said to arise from it; labour is complicated with hemorrhage; and the lochia are profuse, and last for several weeks beyond the ordinary time. The treatment, according to our author, includes bleeding from the arm and local bleeding by leeches or cupping; the patient should strictly keep the recumbent posture; and 3 to 5 grains of calomel, with hyoscyamus or opium, followed by a saline aperient, should be given. The local treatment consists in applying to the ulcer a strong solution of the nitrate of silver; which is to be changed, after the acute symptoms have subsided, for the solid caustic. The discharge of blood is to be arrested by a strong solution of sulphate of zinc, with vin. opii and tincture of matico, applied directly to the diseased surface. "And this applies equally in all cases of passive hemorrhage, when the discharge is furnished from ulcerated surfaces, as well as in other chronic discharges, whether sanguinolent, purulent, mucous, or watery, issuing from the neck of the uterus." We may add, for ourselves, that gallic or tannic acid in solution, or incorporated with some ointment, in the shape of a vaginal suppository, have proved the most generally useful remedies for this purpose. This form of ulcer, in our opinion, when seen during pregnancy, is a local exponent of a general congestion of the uterus; and we believe that a simple granular ulcer may assume the characteristic marks of a varicose ulcer to the sight merely, from the placenta being fixed and formed near the neck of the uterus.

Œdema of the neck of the uterus does not often occur,—only in about 4 per cent. of Mr. Whitehead's cases; and our author very justly regards it as of no great importance in itself; but it marks a strong tendency to serous effusion into the cellular tissue and the large cavities. He associates this condition of the cervix with a dropsical state of the amnion; and he explains the watery discharges which occur at the latter months (the false waters as they are termed), as exuding from numerous points on the surface of the swollen cervix. That this fluid is not an accumulation between the amnion and chorion, as was formerly supposed, is the recorded opinion of M. Naegeli, who regards it as a product of the uterus. Mr. Whitehead does not notice the tendency to convulsions, which œdema, not only of the womb but of the upper parts of the body, so decidedly foretels; and the presence of albumen in the urine, with which, during the latter months of pregnancy, this condition is often associated. The treatment indicated is moderate general bleeding; with calomel, and digitalis, and saline aperients, combined or not with quinine. We would add, exercise in the open air, a spare unstimulating diet, the exhibition of James's powder or antimonial wine, as a substitute (in our opinion) for digitalis, and the warm bath.

The fissurated ulcer is an ulcer with an hypertrophied and hardened cervix, marked with clefts or fissures in its substance. This ulcer may last for years, and is a frequent cause of habitual abortion. The purulent discharge which accompanies it, becomes mixed with blood, as soon as the expansion of the uterus extends itself as far as the diseased parts; and in this way, we suppose, it becomes another source of spurious menstruation. The treatment in this and in the next disease, viz. *induration* of one or both labia, consists in local depletion, in the exhibition of alteratives, with the use of astringents and caustics to the diseased parts. Induration of the cervix causes dysmenorrhœa. Mr. Whitehead found nitrate of silver too feeble a caustic to cure this hardening, and he substituted successfully the acid nitrate of mercury. We would recommend him to employ potassa fusa, which we have found more manageable and useful than any other agent. Iodine with mercury, either painted in a liquid form over the cervix, or used as a suppository, is of much service when there are no acute symptoms or an ulcerated surface.

We have already made some remarks on *endo-uteritis*, and we again repeat that we believe Mr. Whitehead has perfectly mistaken the pathology of this disease. He thus describes the state of the lining membrane of the uterus in a female who had been a martyr to dysmenorrhœa. "The lining membrane of the uterus was soft, turgid, injected of a gray colour, and the whole surface was studded with raised spongy tufts of different sizes, in the form of grayish-looking granulations." It is the presence of the raised spongy tufts which, if the membrane be examined carefully under water, will be seen to give it an appearance exactly similar to that of the decidua vera at an early stage of its formation.

Follicular ulceration. The glands which are immediately concerned in this form of inflammation and ulceration, are those on the surface of the cervix externally; and a distinction in function and anatomical construction is implied, although hardly expressed, by our author, between these glands and those placed internally, in the cavity of the cervix. If we are right in this supposition, we find, in one particular at least, the views which we have promulgated and enforced in our own sphere, quite in accordance with Mr. Whitehead's. "I have strong reason," says Mr. Whitehead, "for believing that the function of these glands is intimately, if not solely, concerned in the phenomena of the venereal orgasm." For some time past we have entertained the same conviction. We believe that, under sexual excitement, they act in concert and sympathetically with the ovaries; and that, towards the close of the act of coitus, they yield their fluid, which, like the secretion from the succenturiate glands, dilutes the semen, and tends to liberate the spermatozoa. The cases which we have seen, where these glands have been inflamed, forming a number of scattered red points on the surface of a tumid cervix, have shown, in our opinion, the secondary effects of an action which has commenced in the ovaries, although seen in the lower part of the womb. Mr. Whitehead's observations have led him to think that anatomically the nabothian glands "consist of an erectile tissue, inclosing a number of cells or tubes, which probably have a fascicular arrangement and are highly organized." But this is not a subject for conjecture. If Mr. Whitehead be right, he ought to be able to speak definitely about them; and by artificial injection, as well as by microscopic examination, to make them out clearly. The glands which are contained within the folds of the ribbings of the channel of the cervix, are easily enough seen and examined after death. They are simple cæcal tubes, set deeply in the tissue of the cervix, with a small external orifice, which is frequently so closed, that the thick mucous contents collect within them, and bulge out upon the surface (the ova of Naboth). These are the glands which, we believe, yield the glairy mucous discharge like white of egg; and when this form of leucorrhœa is present, we know of no exception to its being directly produced by these glands, and by no other parts of the sexual passages. It is with us—and our author's doubt upon the matter does not shake our belief—a pathognomonic sign of an affection of the cervix. The pathology of the first-named glands is well described by Mr. Whitehead. Sometimes they are inflamed, and appear as numerous red, raised points, varying from twenty in number to ten, eight, or a smaller number; and occasionally they are prominent and callous, and assume a warty appearance.

In cachectic habits, with induration of the cervix, they present a worm-eaten appearance; the follicles sometimes inflame and suppurate; and by two or three of them coalescing, a deep excavated sore with callous overhanging margins is formed. "This probably constitutes," says Mr. Whitehead, "the commencement of what has been described as a corroding ulcer." With reference to this point we may say, that we have frequently seen these glands filled with strumous matter, which after a time appears to soften and break up, leaving a more or less deep sore behind. The womb, as an organ, is oftentimes the seat of tubercular deposits, which accumulate sometimes in the tubes, sometimes in the glands of the cervix, and occasionally the uterine glands become filled with it; and after a time, the entire mucous membrane appears to be destroyed, becoming rough, ragged, and ulcerating,—the strumous ulceration of the lining membrane of the uterus.

Gonorrhœal inflammation of the uterus. "Gonorrhœa," says Mr. Whitehead, "is much more frequently an affection of the uterus than of the vagina." It is extremely liable to cause endo-uteritis by its inflammatory action extending within the womb. Mr. Whitehead's opinion and views of the disease confirm, but do not go beyond, those which are now prevalent, both on the Continent and in this country. It is a very common cause, amongst the poor particularly, of granulating and other forms of ulceration of the cervix; and it will last in this state for a very considerable time. After the acute symptoms are relieved, the treatment ought to be local; and it may generally be cured by a few applications of the solid nitrate of silver applied through the speculum.

Syphilis. We looked with much interest to this part of Mr. Whitehead's treatise; hoping that his extensive experience in Manchester, and his accurate notice of cases, would have illustrated, more fully than hitherto had been accomplished, the effects of syphilis on the female system during gestation, and its influence on the offspring. There are few subjects which involve points of greater practical importance than this; and we confess that we sympathise with our author's disappointment, in his having delayed, until quite recently, to give it his particular attention. If we might venture to offer Mr. Whitehead our advice, it would be, that his future researches should set strongly in this direction; and we believe that he would do good service to the profession, and accomplish a most praiseworthy task, if he would amass a far larger number of cases than he has done, carefully and rigidly scrutinise them, and deduce the practical inferences which flow from them. Incidental notices of the effect of syphilis as a frequent cause of abortions, and the possibility of its being transmitted to the female and her offspring, when the male parent is labouring under secondary symptoms only, have been given by Mr. Colles, Dr. Maunsell, Mr. Acton, and one or two others. Mr. Whitehead has not advanced beyond this point; although his description of the signs of secondary syphilis, as shown on the vaginal portion of the uterus, is practical, and we believe quite correct. The following quotation enumerates them.

> "The local pathognomonic signs, to enumerate them in the order of their frequency, are:—1. Endo-cervicitis, or inflammation, to greater or less extent, of the lining membrane of the cervix uteri, with inflammation, excoriation, or ulceration of the labia around the uterine orifice. This appearance was noticed in nineteen out of twenty-eight cases. 2. A mottled or patchy appearance of the cervix, consisting of a number of dark red spots of irregular shape, surrounded by lighter coloured portions. They sometimes appeared highly irritable and excoriated, but not aphthous. The whole cervix was generally enlarged and slightly indurated, and there was evidence in most cases, of endo-cervicitis. This state of parts was noticed in eleven out of twenty-eight cases. 3. Aphthæ of the cervix, occurring in eight out of twenty-eight instances. The patches, which appeared perfectly white, of a circular or oblong shape, situated upon a dark red base, were easily detached by the aid of lint, and left a bright red surface of similar form and dimensions, having, in some instances, a very minutely granular aspect. These were associated with hypertrophy of the cervix, and sometimes endo-cervicitis. 4. Warts were witnessed in three of the cases registered in the preceding table—twice on the cervix, and once on the walls of the vagina. They were all associated with endo-cervicitis and hypertrophy of the cervix. These excrescences were witnessed, however, in many of the instances not tabulated. Inflammatory induration or hypertrophy of the cervix was present in most of the cases recorded in the table, and there was not unfrequently observed an extremely corrugated state of that part of the vaginal mucous membrane lying in immediate contact with the bladder, and beneath the arch of the pubis, with hypertrophy of the cellular structure surrounding the urethra and about the neck of the bladder." (pp. 372–3.)

There are three ways, says Mr. Whitehead, in which secondary syphilis may originate in the female, which we may thus abbreviate. 1. A female has a primary sore which is imperfectly cured. 2. Another may have a sore on or just within the os uteri, which she is not aware of. In both of these instances, secondary syphilis may obviously enough follow on the primary disease. 3. A man has secondary syphilis, which he directly transmits to a female. "The possibility of transference," says Mr. Whitehead, "of the venereal taint under this form will probably be disputed." It is this third mode of transmission which requires very full illustration as a cause of abortion. Our author relates only two cases of it, which, however, are worthy of notice. (Cases xlvi and xlvii.)

If our limits permitted us, we would willingly pursue this subject further; and we should have no difficulty in showing that a venereal taint, in a more diluted state (if this term be allowed us) than any of the recognised forms of primary, secondary, or tertiary, is capable of being transferred from the male through the influence of the semen, and may be concealed in the female system without producing any of the recognised signs of the disease in her, or even affecting the cervix; and may either be followed by a series of abortions, or by the appearance of syphilitic disease in the child three or four weeks after birth. Our own experience on this subject has led us to examine very closely for the history of syphilis, *supposed to be cured*, as a poison still influencing the ovum, and causing habitual abortion; and since we have done so, we have been surprised at the number of cases which have presented themselves to us. Attention ought to be directed to the examination of the aborted ova, which, we believe, will frequently bear marks of the syphilitic poison. Early abortions from this cause generally show marks of a slow death of the fœtus, which frequently is dissolved in the liquor amnii, leaving only the cord behind; and the chorion is surrounded with coagulated blood, which presents an irregular mamillated appearance when looked at from within the amnion, the ovum having previously been opened. Sometimes white patches are seen on the surface of the early placenta, and the parietal decidua appears to have been inflamed; and in one case which came under our notice, the ovum had degenerated on four occasions into the hydatiniform placenta (diseased chorion), which originated apparently from syphilitic poison. Some practical points, connected with the treatment of these cases, still need settling. Both parents may be quite free from any notable sign of syphilis; and yet the child may show decided marks of it either when born, or within one month of its birth. First, the child is to be cured; and the only difference in its management is the mode in which mercury ought to be administered.

This may be done in three ways: 1st, by inunction, after Sir B. Brodie's plan, which Mr. Whitehead approves; 2d, by small doses of the hyd. c. cretâ; and 3d, by salivating the mother, and influencing the child through the mammary secretion. Of these plans we ourselves give a decided preference to the administration of one or two-grain doses of gray powder twice a day; and we may safely allege that there are few cases in practice which are more uniformly gratifying in their results than these when so treated. The child gets plump and well-nourished, and the skin clears, in a comparatively short time. But a more difficult question arises in the management of the parents. Granting the necessity for the use of mercury, which, like our author, we hold to be of prime importance for the destruction of the poison, ought one or both parents to be put under its influence, before the chance of another pregnancy is again hazarded? As a rule, which admits, however, of occasional exceptions, we believe there is no security unless both parents are similarly treated; and our habit has been strongly to advise this step in the cases which have occurred to us. Mr. Whitehead judiciously observes that much depends on the mode of administering mercury; and that the mineral ought not to be dispensed with, even in debilitated constitutions, if the venereal taint shall be known to exist. In this we most fully concur; and we speak from extensive experience when we say, that the solution of bichloride of mercury, combined with cinchona and sarsaparilla, may be given to very feeble and cachectic persons with the best possible result. The beneficial effects of mercury may be securely obtained, and the constitutional powers be invigorated and restored at the same time. Mr. Whitehead does not think very highly of sarsaparilla, and he speaks with far more confidence of the rumex hydrolapathum combined with taraxacum and sassafras, in the treatment of secondary syphilis.

Prolapsus uteri. The last of the ten diseases which our author notices, is this displacement of the uterus, which interferes with gestation, by impeding the current of blood through the veins of the pelvis, and inconveniencing the proper action of the bladder and rectum. Like most practical men, Mr. Whitehead looks upon the atony of the vagina, and, we might add, a weak and insufficient support from the floor of the pelvis, as the cause of descent of the womb. He, like many others, deprecates the use of pessaries; and he directs his treatment for the cure of this descent, in all its stages, to a combination of constitutional and local measures; the latter consisting in the application of suitable remedies to any ulcerated or diseased surface, and the use of astringents, such as matico, tannin, &c., to the flaccid vagina. He has contrived a prolapsus tube, which patients can employ themselves, and by means of it, direct tents charged with these astringent ingredients to the upper part of the vagina. We know nothing of this prolapsus tube, beyond the figure which our author has had engraved of it; but it does not appear to us to possess any advantage over the curved glass tube, which Dr. Montgomery, we believe, first employed. The close approximation of the walls of the vagina, which is always seen on the withdrawal of a speculum, points out the absolute necessity, when injections are being used, that the orifice of the instrument should come in contact with the part affected; and this affords our author the pretext for a sneer against those "authors who have gravely recommended the patient to lie on her back, with the pelvis elevated, in order to receive the full effect of the material injected." In spite, however, of this, we beg to say, that if patients do lie on their back, with a pillow under the pelvis, and use a good Clarke's syringe, or still better, that lately contrived by Dr. Kennedy, they may easily, and do, to our certain knowledge, readily inject fluid to the upper dilated part of the vagina, and so submit the lower part of the womb and the entire vagina to the influence of the ingredient injected. There are, no doubt, many bad instruments and very much bad management employed in this common operation; and in many cases suppositories of tannin, &c., may be advantageously substituted. A table of thirteen cases of prolapsus uteri is appended to this paragraph.

Sterility. The chapter on Sterility, which figures so prominently in the title-page, is short, and contains but little that was not well known before. Mr. Whitehead limits his consideration of the causes of sterility to "those more frequent causes, consisting in diseased states of the uterus and of its organic product, and which, generally speaking, are susceptible both of demonstration and cure." Of these, that on which he most dwells is "chronic endo-uteritis, or what may be called irritable uterus." We have already referred to this affection, which Mr. Whitehead renders still more obscure, by confounding it with the disease which Dr. Gooch has described as irritable uterus. He says that this disease may prevent pregnancy in three ways,—

> "In the first place, the inflammatory action going on within the uterus, and which is liable to be aggravated under states of venereal excitement, may prevent the formation of the membrana decidua, and the ovum, even although impregnated, is necessarily thrown off without any manifestation of its existence in the fertilized state; secondly, the diseased condition of the lining membrane of the uterus may be extended to the Fallopian canals, obliterating for the time their internal orifices, so as to oppose an insurmountable obstacle to the admission of the spermatic fluid within them, and thus to render the fertilizing effort abortive; thirdly, the nature of the secretion furnished by the internal surface of the uterus or of the vagina, under certain states of disease, may be inimical to the active existence of the spermatozoa, occasioning their destruction before they arrive at the extricated ovula." (401-2.)

The first two conditions, which we believe involve an error, are not discussed in this place; but Mr. Whitehead refers particularly to the deleterious influence which certain morbid states of the uterine and vaginal mucus exercise on the spermatic animalcules. He quotes Donné's well-known experiments on this point, which appear conclusively to show that very acid mucus from the vagina, and even that which is secreted during pregnancy, occasion the speedy death of the spermatozoa. Mr. Whitehead thinks that the latter state is accounted for by the absence of the alkaline mucus from the uterus, which modifies the acid character of that which is secreted by the vagina. In chronic endo-uteritis, and perhaps in other forms of disease, the properties of the mucus from the uterus are quite altered; and, instead of partially neutralizing the acid properties of the vaginal mucus, it surcharges it with an excess of that which M. Donné has experimentally shown to be destructive to the zoosperms. We quite participate in M. Donné's and our author's views on this subject; and we are disposed also to look to constitutional treatment as the most efficacious means of altering this derangement of secretion. We need scarcely say, however, that should there be any ulceration or other disease of the cervix, the treatment of a local kind, which is appropriate for its cure, will tend materially to remove one symptom of it,—namely, sterility. The cases which Mr. Whitehead narrates, like those supplied by Dr Bennet, prove this point; which is sufficiently familiar to most who are in the habit of treating uterine disease. No notice is taken of the cure of sterility and dysmenorrhœa by the mechanical dilatation of a morbidly-contracted os uteri: and we must say, that this part of Mr. Whitehead's treatise is defective and unsatisfactory.

In bringing our review of Mr. Whitehead's book to a conclusion, we are glad to express the high estimate we have formed of its author, as a talented, truthful, and philosophical observer. We admire his independent self-reliance, although it sometimes runs to a fault; and we quite appreciate the hard work he must have undergone in collecting the materials for his treatise. Our readers will not, we trust, have inferred from the freedom and minuteness with which we have animadverted upon what we regard as its faults, that we desire to lead them to an unfavourable estimate of this production. On the contrary, it is because the most original portion of the work,—the product of Mr. Whitehead's own observations,—are so valuable, that we have thought it right to subject the whole to a close scrutiny; and to point out where we think that he has been led into error. We must remind Mr. Whitehead of the discrepancy we have indicated between the title and the contents of his work; and would urge him, on the next occasion on which he comes before the public, to think less of filling a book, and more of making a complete, but concise exposition of his subject, whatever that may be. We sincerely hope, and fully anticipate, that he will receive encouragement to extend his observations into other branches of uterine pathology, by a rapid sale of this his first work; and trusting that our recommendation of it to the profession will promote this result, we conscientiously and cordially give it.

Art. X.

1. *Elements of Chemistry, including the Actual State and Prevalent Doctrines of the Science.* By the late Edward Turner, m.d., f.r.s.l., & e., Eighth edition. Edited by Baron Liebig, Professor of Chemistry in the University of Giessen, and William Gregory, m.d., f.r.s.e., Professor of Chemistry in the University of Edinburgh. Part I.—*Inorganic Chemistry*. Part II.—*Organic Chemistry.—London*, 1847. 8vo, pp. 1394.
2. *A Manual of Elementary Chemistry, Theoretical and Practical.* By George Fownes, f.r.s., Professor of Practical Chemistry in University College, London. Second edition.—*London*. Fcp. 8vo, pp. 596.

It is not our intention to enter upon any critical analysis of the works before us. Our general estimate of their respective merits was expressed in our last Number; and any more minute investigation of them would be better adapted for the pages of a professedly Chemical journal, than for those of a Medical review. But we think that it will be useful to take the opportunity of placing before our readers a brief sketch of the actual state of chemical philosophy, and of the progress which certain theoretical opinions have been recently making among chemists. We refer them to the Eleventh Volume of the 'British and Foreign Medical Review,' p. 129, for a tolerably full exposition of these opinions, and of the influence they have exerted over many of the propositions which have been enunciated from the law of definite and multiple proportions, discovered by Dalton. We there took occasion to observe, "that since the period of the reception of the Daltonian *theory*, erected upon those propositions, many new views, involving some fundamental points of theory, have obtained currency amongst chemists; which, though by no means irreconcilable with that hypothesis, cannot be said to be comprehended under these principles." Those new views, together with the propositions upon which they bore, were there discussed. It will be our duty, now, to return to them, and to point out the influence which the further advances of modern chemistry have had over them; and we shall direct attention chiefly to the following questions.

1. As to the signification of the word *atom*, and whether it is, as Dalton supposed, an ultimate particle of matter.
2. Whether each of the elementary bodies is endowed with, and is to be distinguished by, certain fixed and inalienable properties, chemical as well as physical.
3. Whether the business of the analytical chemist is limited to the mere determination of the nature and proportion of the elements entering into the composition of any compound.
4. Whether the difference between one compound body and another may not in many cases be referred to something else, than a difference in the nature or proportion of the elements composing them.
5. Whether it is necessary at all times, in order to effect chemical decompositions, that there should be an approach of a foreign body, having an affinity for one or more of the elements of the body decomposed, and that it should therefore enter into combination with those elements.

6. Whether there is any hope whatever of the artificial formation of those chemical compounds, which are thought to result from the influence of the living principle only.

And 7. What help chemistry is furnishing, or may be expected to furnish, towards the interpretation of the phenomena of health and disease.

It will be seen, on reviewing these questions, that they involve most of the propositions admitted by the followers of Dalton; propositions which have been already discussed in Dr. Daubeny's 'Supplementary Treatise,' and in the review before referred to; it will be observed, too, that nearly all of them turn upon the questions, whether the particles of matter may not, by being differently grouped, assume different functions, and whether our control over these groupings is not more general than has been hitherto supposed.

1. It was put forth by Dalton, that the ultimate particles of all simple bodies are *atoms*, incapable of further division. In his time this proposition was considered to be essential to the support of his theory, and to the interpretation of the laws of definite and multiple proportions; since then, however, the advances of chemistry and physics have tended to shake the truth of the proposition, and to effect a very considerable change in the ideas connected with it. We may premise that there is no wish to get rid of the word atom, but that there is a disposition to assign to it a more extended signification, and to consider that an atom may be an assemblage of particles; the term is not a contradiction, for it still implies that the group or assemblage act as a simple particle, and that it cannot be further broken up without destroying those characters with which it is endowed as a collective body. To give a parallel instance, chemists have never held that the term, atom, was inapplicable to a chemical group or compound; thus it has been very common to speak of an atom of water, and an atom of sulphate of potash, although the former of these contains two particles, and the latter six; yet they are atoms, for they cannot be further divided without destroying those characters which distinguish them, the one as water, the other as sulphate of potash. So with similar groups, it is possible, nay, even probable, that an atom of what is termed an elementary body may be an assemblage of particles, grouped so as to make one collective body, possessed of special attributes, which would be destroyed were it further divided.

The facts which give a countenance to these views, have been derived from a careful consideration of the physical and chemical properties of matter; such as the uniform influence of heat or pressure upon the volume and elasticity of gases and vapours; and the relation which exists between the specific heats, the specific gravities, the equivalent proportions, and the combining volumes of bodies. All these relations seem to show, not only that an atom is an assemblage of particles, but that of the same substance there may be many kinds of atoms. Thus, for example, since it is found that, when gases and vapours are subjected in a like manner to the influence of heat or of pressure they undergo a like alteration in their volumes and elasticities,—indicating that the same volume, under the same circumstances, contains the same number of atoms, and that they become similarly displaced under similar disturbing influences,—may not the atom thus represented be called a *physical atom*, and may we not say that the physical atoms of all gases and vapours have a like size, form, and relation to each other? We cannot predicate so much with regard to the physical atoms of either solids or liquids; though it is possible that, even in these forms of matter, the physical atoms may be yet found to bear in each case a like relation. Gay Lussac has already indicated this, by showing that the contractions of equal volumes of sulphuret of carbon or alcohol are equal, or nearly so, for equal decrements of temperature, when they are examined under parallel circumstances, that is, at equal distances from their boiling points.

Again, since it is found that bodies will combine with each other in certain proportions only, and that when they combine in two or more proportions, the higher proportions are always multiples or submultiples of the lowest,—may not this lowest proportion be representative of an atom, and may it not be regarded, therefore, as a *chemical atom?*

Now if the chemical and the physical atoms were identical, there should not be any difference in the value of the combinations of any two bodies, whether it was determined by weight or by volume. Let us take a combination of oxygen and hydrogen by way of illustration; they combine in the proportion by weight of 8 parts of oxygen to 1 of hydrogen, to form water; and it is assumed that they unite atom for atom, and that these numbers represent the relative weights of the *chemical* atoms of oxygen and hydrogen.

Now if equal volumes of these gases contain equal numbers of *physical* atoms, and the physical and the chemical atom be identical, it follows that they should combine in the proportion of equal volumes, and the relation of the weights of equal volumes should be as 8 to 1. This, however, is not the case; oxygen and hydrogen, in forming water, will only unite in the proportion of 1 volume of the former to 2 of the latter. It is clear from this, that the physical and chemical atoms of these bodies cannot be alike; but that the physical atom of oxygen contains, or is a group of, two chemical atoms. So also with reference to the vapours of phosphorus and arsenic, their physical atoms are twice as large as their chemical; and the vapour of sulphur has a physical atom six times as large; while, in the case of mercury, the relation of the combining weight and volume is reversed, the chemical atom being composed of a group of two physical atoms.

Thirdly, it is possible that another group of particles may be represented by the specific heat of a body; this group is generally, though not always, allied very closely to the chemical group.

From such facts, chemists have gained a notion that an atom is not, as Dalton supposed, an ultimate particle; but that it consists of an assemblage of particles, and moreover, that there may be many kinds of such assemblages. Somewhat of this sort are the views of Ampère, Prout, Dumas, and Kane; and though it must be admitted that they are of a highly speculative character, yet they are necessary to explain the relations which exist between the conditions, the numbers, and the properties, here referred to.

The authors before us do not feel disposed to discuss these questions. In the treatise bearing Dr. Turner's name, we have this observation offered to us:

"When we consider that, by the term 'atom,' we must understand not only the indivisible particle of matter, but also the atmosphere of heat, electricity, &c.

with which it is surrounded, and when we reflect that these last must be variable, and that chemists are not yet agreed on the question whether matter is porous, or whether, as Le Royer and Dumas, Graham, and Kopp imagine, the space is entirely filled by atoms, without vacant spaces or interstices, it is obvious that much remains to be done before this subject can be profitably discussed in an elementary work." (p. 169.)

We cannot help remarking that there is a great admixture of old barbarities in this sentence, and a disposition to take hold of a doubt to avoid the discussion of a subject that might tend, perhaps, to overturn the favourite opinions of its editors. Grant but this faculty of grouping, and there may be very little or no occasion for the use of radicals, or for their cherished binary theory.

We dare not proceed to another question, which follows immediately upon the foregoing,—whether matter may not be infinitely divisible. But we may say, that such a view is, we believe, gaining ground; and that it seems likely to displace the old and highly-reverenced notion of the existence of an imponderable, subtle, and all-pervading medium—Ether. It is probable, too, that the supposition of a highly-rarefied form of matter, differing as much from the crude or concrete atom, as that does from the more crude mass, may give a more intelligible interpretation of physical phenomena than does that of such an inconceivable nothing.

Still less can we attempt an exposition of those more attenuated notions, wherein matter is regarded as a mere assemblage of mathematical points—centres of attraction and repulsion. This is an old hypothesis, which has been revived by Faraday, who considers that it offers a more intelligible view of the phenomena of heat, light, and electricity, than does the atomic theory. The facts which he considers to be opposed to the hypothesis of atoms, are those of the different conducting powers of bodies for electricity, especially when they are influenced by temperature; and of certain compound bodies occupying less space than did the elements composing them, or even than one of their many constituent elements before combination.

2. We proceed to the second question. It was generally admitted by the Daltonians, that there were a variety of elementary bodies, each distinguished by certain fixed and inalienable properties, chemical as well as physical. The researches of Faraday, Schönbein, Dumas, Berzelius, and many others, tend to prove, however, that this is not the case; many elementary bodies, and perhaps all, being able to assume two or more conditions in which both their chemical and their physical properties may undergo a great change. The elementary body carbon, for example, is met with in three states,—as the diamond, as plumbago, and as ordinary charcoal. The first is generally a colourless, transparent body, crystallizing in some form of the cubic system; it is exceedingly hard, a non-conductor of electricity, has a specific gravity of 3·5, and is burnt with the greatest difficulty. The second form, however, graphite or plumbago, is an opaque, black body; it crystallizes in six-sided prisms, a form belonging to another system; it is very soft, a good conductor of electricity, has a specific gravity of from 1·9 to 2·3, and is more easily burnt. And the third form, charcoal or coal, is amorphous, still lighter than graphite, and very easy of combustion. Berzelius, who has studied these different conditions of the elementary bodies and described them as *allotropic states* (from

ἄλλος, another, different, and τροπη, nature), asserts that most of the elements may be made to assume conditions in which their properties are completely altered; these conditions may be brought about by the action of heat, by contact with some other body, and by the method employed in their preparation.

The investigations which have led to these curious results, were called for by the discoveries of Gay Lussac, Maliguti, Dumas, and Regnault; who found that chlorine might be made to displace hydrogen, atom for atom, in an organic compound, showing that the former gas has taken on the function of the latter, although its habits and general properties were most dissimilar. This fact, which was not readily explicable upon the electro-chemical theory of Berzelius, led to the investigations above referred to. Draper has taken up the inquiry, and he finds that chlorine, like iron in the experiment of Schönbein, may be made to pass from a state of high activity, in which it possesses all its well known properties, into one of complete torpor, wherein these properties are lost; he believes that this is effected, when it displaces hydrogen, by its proximity, or contact, or relation to some other highly electro-positive body, as the carbon, for example, of the organic compound. Again, it is curious that while nitrogen, in its common and free state, is a perfectly passive body, exhibiting no disposition to enter into union with any substance; yet that it does under some circumstances assume an active character, and will then combine readily with hydrogen and oxygan, forming ammonia, nitric acid, and nitrate of ammonia.

Upon the electro-chemical theory it is somewhat difficult to account for these facts. Its advocates are obliged to admit, either that one body may, under certain circumstances, part with its normal electrical influence, and take on an opposite one, or that it has the faculty of having a very weak state of it exalted by its relation to some other body. In either case the admission is a strain upon acknowledged facts. Others have argued that these differences arise from a different mode of grouping, either with respect to the particles in forming the atom, or the atoms in forming the mass.

We think that the authors before us are not altogether clear in their expression and interpretation of these facts; for they say that—

> "If each atom of an element possess the properties of that element, it is contradictory to suppose that mere aggregation can confer new chemical properties; since we are only acquainted with aggregates of atoms, and these aggregates, ex hypothesi, possess the same properties as the atoms of which they are made up. It may be said, indeed, that our hypothesis is not demonstrable, and that, for aught we know, the atoms may not have the properties of the aggregates which they form; but this is at least as little capable of demonstration as the ordinary view; and in favour of the latter, we know that very great changes in aggregation do occur without affecting the chemical properties of elementary bodies. Thus sulphur, for example, is still sulphur, whether it be solid, liquid, or gaseous." (p. 176.)

Yet upon the next page they remark:

> "In conclusion, it may be observed, that, as there is reason to admit some kind of grouping of the atoms of elementary bodies, as necessary to their existence with the properties which we consider to belong to the elements, so a different grouping, with different properties as its results, may be considered possible, although, if the prevalent ideas respecting atoms be correct, it is very

far from being probable, and certainly has hitherto received no support from experiment. Moreover, we shall see hereafter, that in sulphur and some other elements, modifications called allotropic actually exist, in which we can only suppose the difference of properties to arise from a difference in the aggregation of the atoms to form molecules. But, in all known cases of allotropic modification, and these are now pretty numerous, the modified forms retain the essential properties of the element, and do not exhibit any appearance of transmutation. The principle, however, of difference of properties, as a consequence of a difference either in the number or the grouping of the same elements, combined in the same proportion when applied to *compound bodies*, is one of the highest importance, and plays a most essential part in the chemistry of organized bodies." (p. 177.)

We find ourselves unable to make out the real views of the authors; but we suspect that they believe in the influence of a different mode of grouping, for they admit this with reference to physical properties; whilst they hesitate over such an admission, from fear of danger to some cherished theory.

Let us pause here, and ask whether, in regarding the properties of matter as dependent in some cases upon the arrangement of the atoms, we are not going back again to the fancies of the alchemists, and getting together facts for the structure of a new belief in the doctrines of transmutation. A few attempts have indeed been made to demonstrate the truth of these doctrines. Dr. Brown has placed before the Royal Society of Edinburgh an account of some experiments, in which he thought he had transmuted carbon into silicon, and iron into rhodium. His idea was, that the particles of carbon were identical with those of silicon; that, being arranged in one way, they produced a body having the properties of carbon, and being arranged in another way, they produced a body having the properties of silicon; and so with reference to the particles composing iron and rhodium. It does not appear, however, that other chemists have succeeded in obtaining similar results; and there is reason for believing that he has been misled by some impurity in his materials.

Upon this subject the editors of Turner judiciously remark,—

"It would be going too far to declare that this transmutation is impossible; but it is evident that it is not consistent with our present views of the nature of elements." (p. 176.)

3. In answer to our third question, Whether the business of the analytical chemist is limited to the mere determination of the nature and proportion of the elements entering into the composition of any body,—it may be replied that, in the days of Lavoisier, and even still later, in those of Dalton, this might have been held by chemists as a sufficient aim; but in our time the duty of the annalist is not permitted to rest here. He regards this merely as the prelude to another duty. Analysis only teaches him the *composition* of a body; but he seeks to know its *constitution* also, that is, how the atoms were arranged in it. Let us give an example. Analysis has proved that crystallized benzoic acid consists of—

Carbon . .	68·85
Hydrogen .	4·92
Oxygen . .	26·23
	100·00

Reduce these proportions to atomic numbers, and its *composition* is expressed by $C_{14}H_6O_4$; this is its *true* or *empirical formula;* but chemists assert that this does not represent its *constitution*, and hence they frame other formulæ—*hypothetical* or, as they term them, *rational formulæ*,—which shall represent the particular opinions they entertain as to the mode in which the atoms are grouped. Thus Dumas, who thinks that one atom of its oxygen is readily displaced, writes it thus $C_{14}H_6{}^{O_3}_{O}$. While Liebig, regarding it as a compound of a radical, benzoyle, with oxygen and water, expresses it thus, $C_{14}H_5O_2+O+aq$; and Berzelius, not admitting of the existence of oxygen in any organic radical, would consider it as the hydrated teroxide of a radical composed of $C_{14}H_5$, and he would represent it thus $C_{14}H_6+O_3+aq$.

Amidst these differences, the young chemist will naturally ask for the rule or the law which is to guide him.

Berzelius would have him to be directed by certain opinions which he had advanced, respecting the electro-chemical properties of bodies; and he would have the elements of all compounds arranged so as to exhibit a double structure, a combination of molecules in which there is a play of opposite electrical functions.

Liebig entertains notions somewhat similar; but he goes farther. He endeavours to trace them to, or from, some known compound; and watches their reactions with other bodies, so as to judge thereby of their probable constitution.

Dumas, however, is altogether regardless of these opinions; he turns to the facts of the inquiry, observes the order of substitution, and endeavours to express it by uniting the elements into one group, which he calls, or associates under, a type.

It is evident that there is a great opportunity here for the full display of the fancy and ingenuity of the theoretical chemist; it is a temptation, moreover, to which he cannot help yielding; and we find him fooling it to the top of his bent. Groups of elements are depicted under the alluring terms of compound radicals, hypothetical combinations, probable reactions, and so on; of the truth of which there is not the merest shadow of proof, no, not even in the ardent conceit of their own authors. We need not say how, in playing with these so-called radicals, and striving to make them the representatives of every chemical structure, facts are perverted, warped, and strained, until the great truths of chemical science are almost lost sight of. Did we think it necessary to illustrate this, we would turn to any one of the series of organic compounds described in the works before us; and of all these, there is not one, perhaps, so fully illustrative of the hasty, fanciful, and yet laborious effort at radical-making, as that given at page 779 of Turner, wherein the editors have described a number of bodies under the head of "Hypothetical compounds of cyanogen and carbonic oxide." Now there is no proof whatever of the reality of such a combination; and yet they are bold enough to assume as much, and to call the hypothetical thing urile or urilic acid, and then to make it the basis or radical of a series, in which the uric acid, alloxan, alloxantine, dialuric, hydrulic, theonuric, and alloxano-sulphurous acids, and uramile are grouped; whilst, to make their doctrines more intelligible, they have tacked on to it a set of formulæ, which we think they have most inappropriately termed *rational.*

Some mode of grouping, however, must be adopted; and the great question which agitates the chemical world is, whether we shall use the binary plan of Berzelius and his followers, or whether we shall adopt the ideas of Dumas, and agree to arrange the elements of all bodies as if they were formed into a single structure.

The editors of Turner adopt the former plan; and we cannot do better than illustrate it in their own language. We shall, therefore, quote their views as regards the constitution of acids and salts:

"Since the sixth edition of this work was published, new views on this important class of bodies have begun to prevail. The researches of Graham on the phosphates, those of Liebig on the constitution of the organic acids and their salts, and the experiments of Dumas, Clark, Frémy, Thaulow, Péligot, and many others, have gradually converged to the point of recalling to the recollection of chemists certain profound views, first suggested by Davy, in regard to chloric and iodic acids and their salts, and afterwards applied (apparently without previous knowledge of what Davy had done) by Dulong, to the salts of oxalic acid. These views have the inestimable advantage of uniting all acids into one series, and all salts into another; nay, these two series may even be considered as one.

"In regard to acids, the first point to be noticed is, that all so-called oxygen acids, in the free, or what may be called the *active* state, contain hydrogen. On referring to the description of mineral acids, for example, it will be found that they are described as combining with water when separated from their combinations. Oil of vitriol is SO_3, HO; nitric acid NO_5, HO, &c. The latter, indeed, cannot exist in the supposed anhydrous state, NO_5; and this is the case with a large majority of all known acids. Sulphuric acid and phosphoric acid, no doubt may be obtained anhydrous, SO_3 and PO_5; but it is worthy of especial notice, that in this state *they do not possess the properties of these acids*, and only acquire them on the addition of water. The compound of dry sulphuric acid and ammonia SO_3, NH_3, is *not* sulphate of ammonia, but a distinct compound. Moreover, these anhydrous acids combine with water, with the greatest vehemence, and then assume their active characters. The principal exceptions are carbonic acid and chromic acid; but, on the other hand, none of the organic acids can exist without water, that is, without hydrogen.

"It is obvious that hydrogen is essential to the hydracids. Now the view which I wish here to explain considers both these classes of acids as hydracids, and thus unites in one class or series, bodies having the most perfect analogy in properties. According to this view, therefore, the general formula of a hydracid is $X + H$; X being an acid radical, which may be either simple or compound. Thus, in hydrochloric, hydriodic, and hydrosulphuric acids respectively, X is represented by Cl, I, or S. In hydrocyanic and hydrosulphocyanic acids, X is represented by $Cy = C_2N$, and by $CyS_2 = C_2NS_2$, respectively.

"In the hydrated oxygen acids of the preceding pages, to which alone, and not to the anhydrous acids, this theory applies, X is always a compound, and always contains oxygen. Thus in hydrated sulphuric acid, commonly so called, and represented by SO_3, HO, X is represented by SO_4; in nitric acid, NO_5, HO, $X = NO_6$; and in metophosphoric acid, PO_5, HO, $X = PO_6$: and the true formulæ of these acids are SO_4, H, NO_6, H, and PO_6, H, respectively.

"Further, among the organic acids to be afterwards described, we find a corresponding constitution. In acetic acid (hydrated) $C_4H_3O_3$, HO, $X = C_4H_3O_4$, in hydrated formic acid, C_2HO_3, HO, $X = C_2HO_4$, &c.

"The next point to be noticed is, that acids exist, the general formula of which is $X + H_n$; that is, in which X combines with two or more equivalents of H, and which are called polybasic acids. Those acids, above described, in which there is 1 eq. of H, are called monobasic acids; where 2 eq. of H are present, the acid is bibasic; with 3 eq. of H, tribasic; and so on. Examples of this kind are pyrophosphoric acid, PO_5, 2HO, which is bibasic, its true formula being PO_7, H_2; phosphoric acid, PO_5, 3HO, which is a tribasic acid, PO_8, H_3; and arsenic acid, AsO_5, 3HO, also a tribasic acid, AsO_8, H_3.

"It is, however, among the organic acids that we find the most striking examples of polybasic acids; meconic, cyanuric, citric, and tannic, being all tribasic, and represented by $X + H_3$; while tartaric, komenic, fulminic, and mucic are represented by $X + H_2$.

"Moreover, there are also polybasic acids which contain no oxygen, analogous in this respect to hydrochloric and hydrocyanic acids. Thus, ferrocyanic acid is represented by $Cy_3Fe + H_2$; and ferridcyanic acid by $Cy_6Fe_2 + H_3$.

"It will be obvious at a glance, that this theory of acids possesses the advantages of simplicity and of uniting in classification a vast number of bodies, similar in properties, which have formerly been arbitrarily separated. These views are still further extended in a chapter on the theory of organic acids. (p. 694.) But the chief advantage attending it is, that it enables us to effect the same union into one class of all the salts of the acids containing hydrogen. It is in examining the salts, moreover, that we find the strongest arguments in favour of the theory as applied to acids.

"A salt is formed, whenever one of these acids is neutralized by a metallic oxide, by ammonia, or by an organic base, or combines with them without being neutralized.

"Now, when a salt is thus formed, one phenomenon constantly occurs; this is the separation of *water*. In the simplest case, namely, where the hydracid of an elementary body acts on a metallic protoxide, the origin of the water is quite obvious. When hydrochloric acid, for example, acts on oxide of silver, chloride of silver is formed, and water is eliminated: $HCl + AgO = AgCl + HO$. There is here no doubt that the water is produced by the reaction.

"But when hydrated sulphuric acid acts on the same oxide, although the phenomena are exactly the same, a different explanation is commonly given; and the water is assumed to have pre-existed in the acid, thus, SO_3, $HO + AgO = SO_3$, $AgO + HO$.

"It is contrary to all sound principles of reasoning to adopt two explanations of facts precisely similar, where one will suffice; and as, in this case, only one explanation of the former case is possible, we must apply the same explanation to the latter. This is done by the new theory; and the following formulæ will show the identity of the reaction in the two cases:

$$H\,Cl + Ag\,O = Ag\,Cl + HO.$$
$$H\,SO_4 + Ag\,O = Ag\,SO_4 + HO.$$

"In both cases the water is formed by the union of the hydrogen of the acid with the oxygen of the oxide; and consequently, in both cases the hydrogen of the acid has been replaced by the metal.

"*Here then is the theory of salts.* A salt is formed when the replaceable hydrogen of the acid is replaced by its equivalent of a metal. Consequently, acids may be viewed as the hydrogen salts of their radicals, and thus acids and salts, in regard to their constitution, will form but one series." (Pp. 567–70.)

This gives a clear exposition of the modern theory with regard to the constitution of acids and salts; well has it been called the "binary theory," for it views all these compounds as composed of two antagonistic molecules, one of which performs the function of an electro-negative body, the other that of an electro-positive one.

Among the advantages of this theory which have been enumerated, are the following:

1stly. It reduces these two, and apparently dissimilar, sets of combinations to one class.

2dly. It offers an explanation of the remarkable law, that in the formation of neutral salts, bases always combine with as many equivalents of acid, as they themselves have of oxygen. It is evident from the above theory, that this must be so, in order that the oxygen of the base and the hydrogen of the acid may unite to form water. So, on the other hand, the number of elements of replaceable hydrogen in the acid is the exponent of the quantity of the base required to saturate.

3dly. It affords a simple explanation of the action of a metal on an acid solution; there being generally hydrogen gas evolved, merely by displacement.

4thly. It furnishes, according to Daniell and others, an explanation of the fact, that when a current of electricity is sent through certain saline solutions,—sulphate of soda, for example,—the acid with hydrogen appears at one pole, while the alkali with oxygen appears at the other; now, say they, we can hardly imagine that the electric current had decomposed both the salt and the water, but rather that it had acted on the salt only, liberating, as it would have done with a chloride, the radical at one pole and the metal at the other; but as, in this case, neither of them is permanent, they react on water, the radical taking hydrogen to form an acid, whereby oxygen is liberated, while the metal appropriates oxygen to form the alkali, and thus sets hydrogen free. If a salt of a permanent metal had been used, then no hydrogen would have been evolved; and if both radical and metal were permanent, then neither of these gases would have been liberated.

Again, it offers an explanation of the action of an acid in facilitating what appears to be the decomposition of water only, when a current of electricity is passed through an acid solution.

All these facts appear to furnish very powerful arguments in favour of the theory of binary grouping; nevertheless, we are bound to hesitate before we admit it, and to ask, whether they may not be explained, quite as easily, by the advocates of the opposite theory, and whether there are not many facts which have received no interpretation at all from it. We think, for example, that it is not sufficient to explain the peculiarities in the constitution of the phosphates, without supposing that there are three distinct phosphoric radicals; nor will it explain how it is that certain compounds generally admitted as acids, such as carbonic, chromic, boracic, titanic, and silicic, should possess acid characters, and yet not contain hydrogen. Liebig gets rid of this difficulty very cavalierly, by boldly asserting that they are not acids. It strikes us, too, that a very powerful objection might be raised to this *radical doctrine,* upon the ground that it creates a multitude of hypothetical bodies. Look, for instance, at his present system of organic chemistry, a system of "compound radicals;" and yet, out of all the supposed molecules, which are there made to play the most important parts, and which look so satisfactorily upon paper, giving so complete an interpretation of organic changes, and so full an expression of the constitution of organic compounds, how many of them have been seen? Only two,—cyanogen and kakodyle; the rest are merely hypothetical. But, say the editors of Turner,

> "Were our knowledge of organic chemistry more complete than it yet is, all organic compounds would probably admit of being arranged in groups, of each of which groups a compound radical would form the origin; the individual members of these groups arising from the combination of this radical with elementary bodies, and from the union of the compounds thus formed with other compounds."

Upon this assumption they have made the hypothetical radical a sort of standard, around which they have rallied a group of compounds; and they have endeavoured to express the constitution of these compounds, by representing the radical as one of the electro-chemical elements of it. Thus they regard the radicals of sulphuric and nitric acids, cyanogen, sulpho-cyanogen, ferro-, and ferrid-cyanogen, &c., as *acid or electro-negative radicals*, analogous in their properties and mode of combination to chlorine. Under the head of *basic or electro-positive radicals*, they put ammonium, ethyle, methyle, kakodyle, certain compounds of platinum and ammonia, &c., which are supposed, like potassium, to have the power of uniting with oxygen, chlorine, and sulphur, to form basic compounds. While there is a third class which may be called *neutral radicals*, such as benzoyle, cinnamyle, &c., which form acids with oxygen, and volatile oils with hydrogen. These do not appear to have any precise inorganic analogues, though they are parallel to the elements, sulphur, carbon, phosphorus, arsenic, &c.

For our part, we do not think that this mode of grouping, or as it may be termed, this hypothetical system, would be permitted to stand for one instant, but for the convenience it apparently offers in the classification of a large number of complex details; we say apparently, for even here, upon a close survey, the imperfection of the system is most manifest. We see, for example, that on the one hand it associates bodies possessed of the most opposite constitution and properties; while, on the other, it separates those which have an acknowledged relationship. We may perceive, likewise, that the latter difficulty is strongly felt, and that an effort is made to meet it by tacking such bodies on, in the shape of a tailpiece or appendix.

Dumas and other chemists are therefore strongly opposed to these views. They will not admit that the elements of compound bodies are ever grouped in such a binary manner; but rather maintain that they constitute one system or structure, from out of which any of the elements composing it may be removed and others made to take their place, without a destruction of the original system, or even an alteration in the primary essential attributes of it.

The doctrines of Dumas have been brought forward under the terms "laws of substitution" and "theory of types." We shall see that the former are simple expressions of facts, and that the latter endeavours to account for them.

These doctrines had their origin in an experiment made some years ago by Gay Lussac; who found that when beeswax was exposed to the action of chlorine gas, it lost some of its hydrogen and took in an equivalent volume of chlorine. Dumas subsequently found that the same thing occurred when oil of turpentine was subjected to the influence of this gas; and that 4 volumes of hydrogen were replaced by 4 volumes of chlorine, the compound changing from $C_{20}H_{16}$ to $C_{20}H_{12}Cl_4$. This led him and others to a further prosecution of such inquiries; and in the end they established this great fact,—that there were many organic compounds, the elements of which might be replaced by other elements, first to a small extent, subsequently to a greater, and finally in some cases altogether, without the compound losing any of its *fundamental* properties; that is, the products will continue to be constructed on precisely the same plan as the originals, will obey the same laws of combination, and will be broken up by stronger affinities into compounds, parallel with those which result from the action of similar affinities upon the parent substance. Thus, to give an instance, hydrated acetic acid, $C_4H_4O_4$, will, under the influence of dry chlorine, have 3 equivalents of its hydrogen displaced, thereby forming chloracetic acid, $C_4HCl_3O_4$, a compound which possesses the same number of elements, follows the same laws of combination, has the same fundamental properties, and may be broken up, as we have said, into parallel groups; for if acetic acid is heated with potash, it gives rise to $2CO_2$ and C_2H_4, and if chloracetic acid is similarly treated it produces $2CO_2$ and C_2HCl_3.

Upon such facts as these the "laws of substitution" have been framed, and the "theory of types" propounded. The former is but a simple expression of experimental facts. It merely asserts that there is a relation between the number of elements lost by a compound, and the number of elements gained. The latter, however, goes further; it attempts to give an explanation of these facts. It considers organic bodies as being formed of particles which may be displaced by others, without the body being destroyed; that it is, in fact, on pain of being destroyed, that such a displacement occurs. We must admit, says Dumas, on reflection, that in every case of substitution the molecule has remained intact, formed of a group or system in which one element has taken the place of another. This system he compares to a structure, or an edifice, out of which it is possible to take any portion of its materials, and to replace them with others, equivalent for equivalent, without interfering with its form or general plan. A little reflection will show that this replacement is absolutely necessary, in order to preserve the integrity of the original structure.

The term "type," of Dumas, refers merely to the general plan of any group or structure, and to the number of elements composing it; it is a sort of model which serves as a guide for the arrangement of chemical compounds. When, after any kind of substitution, the group retains its original plan and *number* of elements, he regards it as belonging to the same "*molecular or mechanical type;*" and if it preserves its chemical relations also, that is, if it still behaves (like the original) as an acid, or base, or neutral body, and is similarly acted upon by stronger affinities, he then says that it belongs to the same "*chemical type.*" We need not cite instances of these, for in mineral chemistry almost every act of decomposition is one of substitution. We may observe, moreover, that, in this division of the science, there is generally a very close relation between the electro-chemical characters of the bodies which displace one another. Not so, however, in organic chemistry; here the reverse is most common. We find chlorine, for example, taking the place of hydrogen, although upon electro-chemical views it is perfectly antagonistic to it; and that, too, without destroying or even altering the fundamental properties of the compound. A consideration of these facts has led Dumas to believe that it is not the *nature* of the elements, but the *position or arrangement* of them, which determines the properties of a compound; and, further, that the elements of a group are not arranged so as to form antagonistic molecules, as Berzelius and his followers maintain, but that they are united into one system. Here it is, that the theory of types swerves from the electro-chemical theory; and here it is that Dumas puts into doubt the function, and often the existence, of an organic radical. He will not admit that compounds are ever endowed with such electrical functions, or that they can be made the bases of more complex groups; for their composition, he says, is not permanent; they are, like other bodies, susceptible of change by substitution, and this without losing their original properties. Nothing, however, hinders him from retaining the name organic or compound radical, for such molecular groups as are capable of substitution, or reciprocally of being substituted, for elementary bodies.

A very slight glance at the facts of substitution will indicate that there is a close relation between them and the facts of isomorphism; both indicate that the elements of a compound may be displaced by other elements, without any loss of form or of fundamental properties. Dumas infers that these facts express the two great leading principles of chemistry; that they originate from the same cause; and that in due time they may be generalized under one common expression.

Some curious observations, giving countenance to these views, have recently been made by Dr. Blake; they are very important, and we should like to see them confirmed by other investigators. He reports that all isomorphous substances, introduced into the blood of a living animal, produce analogous effects, and give rise to the same reactions in the animal economy. This law he has verified by an extended series of experiments with the salts of manganese, lime, magnesia, iron, cobalt, potash, ammonia, antimony; the acids of phosphorus, arsenic, bromine, chlorine, iodine, sulphur, and selenium. He finds, also, that those bodies which are isomorphous with any of the substances existing in the blood, give rise to the least marked reactions; whilst, on the other hand, bodies which are not so related, produce very powerful effects. Arsenic and phosphoric acids, for example, may be injected into the veins in very large quantities without occasioning any marked phenomena; but a grain of chloride of palladium, or two grains of nitrate of baryta, are quite sufficient to arrest the movements of the heart. (Report of Brit. Association, 1846, p. 41.)

As with Liebig in the case of radicals, so Dumas here proposes to make use of his theory of types as the basis of a classification. He would associate all bodies containing the same number of atoms, united in the same way, under the head of a "molecular type," and he would regard them as a natural family; while his subdivisions or genera would consist of compounds, which preserve the same "chemical type." Each chemical type or genus would bear a name; and this name should appear in each of its modifications. It is upon this principle that he uses the names, ether, and chlorether; acetic acid, and chloracetic acid; olefiant gas, and chlorolefiant gas, &c.—names, the object of which is to set forth the permanency of the types, notwithstanding the substitution of chlorine for hydrogen. Such a system of classification and nomenclature would enable us to form some idea of the composition and properties of bodies; for it would represent them to us as casts from the same mould, with different materials. It would be like saying, here is a Venus of Milo, in brass, lead, or plaster. From such a point of view we should regard common alum as a type, and all other alums as cast from the same mould; hence we say chrome alum, iron alum, manganese alum, and soda or ammonia alum.

Such are the views of Dumas and his followers (see the Periscope); and we may thence perceive as we did from those of Berzelius and Liebig, that the business of the chemist is not limited to the mere determination of the several elements and their proportions in a compound; but that he seeks, further, to know the manner in which they are grouped, so that he may interpret the reactions of one substance upon another, and be thus enabled to unfold some of the mystery of organic changes.

It must be conceded, however, that the chemist, in his efforts to effect this, has often carried his opinions too far, and has thereby done much harm. He has made calculation take the place of experiment; and relying upon a show of figures and the rearrangement of a few questionable formulæ, he has rested satisfied that he has thus effected the solution of physiological problems, which could not be done without much observation and experiment; nothing is easier to do than this, and nothing appears more rational or brilliant; but nothing can be more uncertain, or more prejudicial to the interests of either science.

In reviewing the opinions which have been here discussed with reference to the manner in which the elements of bodies are grouped, we cannot conceal from ourselves that they represent two distinct sets of ideas: one sanctioned by the authority of the past, and supported by the eminent and illustrious philosopher, Berzelius: the other reared upon generalizations of the most extensive character; appealing to nature and to experimental facts in proof of its integrity; pointing, not only to the probable, but often to the certain changes which must result from the operation of certain agents; telling us how it may be, that a few elements can build up the endless varieties of organic structure; and, withal, reducing the expression of that structure to the simplest formula. These views, however, are very modern views; and, like all new things which demand a destruction of the old and familiar, are slow in making progress. At present, chemists are not quite disposed to adopt the whole of the doctrines advanced by Dumas; though all are agreed upon the importance of them, and they frequently strive to modify them to their own text; thus, in the edition of Turner before us, we find the following:

> "But while we admit the importance of the doctrine of substitution as embodying a vast array of important facts, it is necessary to mention that the distinguished French chemists, above alluded to, appear to have extended their theoretical notions on this subject further than seems warranted by facts: and, in particular, appear to conceive that the facts of substitution are in some way inconsistent with the idea of the existence of compound radicals. Now, this does not appear to be at all the case. It is certain, as quoted above, that some compound radicals do exist, possessing all the properties of elementary radicals; and even in the case of those radicals whose existence is hypothetical, it is not easy to see how the facts of substitution should be incompatible with their existence. We shall, therefore, avail ourselves of all the facts connected with substitution, without, on that account, either adopting all the theories of substitution, or rejecting those views of the existence of compound radicals, which, in the present state of our knowledge, so materially facilitate the classification of the details of organic chemistry. (p. 684.)

Professor Graham also admits that,—

> "The reference of bodies to a common type has often an advantage over their classification under a common radical, or according to any theory of constitution, as it involves less that is speculative. The former asserts only that the bodies contain the same number of atoms, and have a common constitution, but says nothing as to what that constitution is. Hence a type may be denoted by an empirical formula of the simplest kind, expressing nothing but the elements, and their number, in which changes by substitution can be distinctly exhibited. It

is useful, in the present uncertain state of our knowledge respecting the constitution of organic compounds to have such a mode of expressing compounds, and exhibiting their relations in composition, but it does not supersede rational theories of constitution." (Elements, p. 723.)

And Kane, after generalizing upon the facts of substitution, observes:—

"Considered in this way, the theory of types is an important addition to our ideas on the constitution of organic bodies. It serves to attach, under a few simple principles, numerous classes of compounds, whose composition would otherwise appear very complex and anomalous, and will probably, when applied to the study of such bodies, as, not containing compound radicals, give only their molecular group as a mass to our examination, become a source of still more important additions to our knowledge." (Elements, p. 783.)

In a few words, then, we may dismiss our third question, by saying that the business of the analytical chemist is not limited to the mere determination of the nature and proportions of the elements which enter into the *composition* of a compound; but that he seeks to know the manner in which they were grouped, so that he may express its *constitution* also. At the present time, there are no fixed rules to guide chemists in this determination; and hence the disagreements which are ever occurring in their mode of expressing it. Hence also the necessity for two sets of formulæ—one, *empirical*, which shall be expressive of the true analytical results and show the *composition* of the body; the other *rational*, which shall set forth the peculiar views of the author with regard to its *constitution*.

4. Another proposition, formerly entertained by the Daltonians, was, that the difference between one body and another could only be referred to two causes, namely, a difference in the elements, or in the number of elements composing them; so that all bodies, composed of like elements in like proportions, must necessarily be alike in all their properties; and, conversely, all bodies possessed of dissimilar properties, must necessarily be composed of different elements or of a different number of elements.

In the course of this review we have had occasion to allude to many facts which are inconsistent with the truth of this proposition; the allotropic states of the elements, for example, are completely opposed to it; and so are also the investigations of Berzelius and others, who have pointed out that there are compound bodies, possessed of most dissimilar properties, notwithstanding that they are composed of the same elements, united in the same proportions. Instances of this sort are of frequent occurrence in organic chemistry, especially among the compounds of carbon and hydrogen; thus, there is a series of bodies consisting of these elements united equivalent for equivalent, in which the carbon and hydrogen, as determined by analysis, are in the proportion of 85·7 and 14·3. To this series belong the hypothetical body methylene, olefiant gas, oil gas, several products obtained from the condensed liquor of oil gas, etherole or light oil of wine, etherine or the solid camphor of etherole, amilene, oleëne and elaëne (two products obtained by the action of sulphuric acid on fats), naphthene, cetene, &c.—bodies, the physical and chemical characters of which are most dissimilar, notwithstanding that they are composed of the same elements, united in the same proportion.

Again, there is a group of carbo-hydrogen, in which the elements are united in the proportions of C_5H_4, and to which most of the volatile or essential oils belong, as oil of turpentine, lemons, juniper, athemanta, sabine, orange-peel, orange-flowers, neroli, cedra, and the volatile oils of copaiva, elemi, storax, pepper, cubebs, and probably many others. All these have the same per centage composition; they consist of carbon and hydrogen in the proportions of 88·46 and 11·54; though their properties are most dissimilar.

For such compounds as these, now so numerous in organic chemistry, Berzelius has proposed the general appellation of *isomeric* (from ισος, equal, and μερος, part); a term which is expressive of an equality of ingredients. In mineral chemistry we have not any good examples of isomerism; we may, however, refer to the three forms of phosphoric acid, PO_5; to the three acids of sulphur, viz., hyposulphurous S_2O_2, pentatheonic S_5O_5 of Fordos and Gélis, and the acid S_5O_5 of Wackenroder; and to the peroxide of hydrogen HO_2 of Thénard, and ozone HO_2 of Schönbein; as offering a few probable instances of it. Among organic compounds, however, they are exceedingly numerous; and, in studying them, chemists are enabled to recognise two distinct classes of isomeric bodies.

In the first, the compounds not only contain the same atoms united in the same proportion, that is, the same relative number of them, but they contain the same absolute number also; and hence, the equivalent or combining proportions of such compounds must be the same. For example, acetic ether and hydrated butyric acid are bodies which have the same ultimate composition $C_8H_8O_4$, though one is an agreeable neutral liquid, and the other an unpleasant fatty acid. Again, hydrated benzoic acid, and the hydruret of salicyle, are very different bodies which have the same composition and are represented by the same empirical formula $C_{14}H_6O_4$. It is to such compounds as these, that the term isomeric is strictly applicable.

Belonging to the second class are bodies which possess the same elements and the same relative number of them, but not the same absolute number; and, consequently, their equivalents or combining proportions are not the same. Thus, aldehyde has the same per centage composition as the two bodies just referred to, namely, acetic ether and butyric acid; but, while the empirical formulæ of the latter are $C_8H_8O_4$, that of aldehyde is only half these numbers, namely, $C_4H_4O_2$. Again, cyanic, fulminic, and cyanuric acids have the same composition in 100 parts; yet their empirical formulæ, as determined by their combining proportions, are C_2NHO_2, $C_4N_2H_2O_4$, and $C_6N_3H_3O_6$ respectively, or as 1, 2, and 3 of the first set of proportions.

The members of this class are by far the most numerous; the carbohydrogens and essential oils just mentioned belong to it. It will be seen that such bodies are not, strictly speaking, isomeric; hence, another term, *polymerism*, has been proposed to designate them. A better term, we think, would be *polyisomerism*.

We perceive from these facts, that the advances of modern chemistry have broken in upon the Daltonian proposition just quoted; and that the difference between one compound and another must in many cases be referred to something else, than a difference in the nature or properties of the elements composing them. We may perceive also that there is an intimate relation between the facts of allotropism and those of isomerism; indeed, those elements which present the most marked allotropic characters are the very elements which constitute the bases of isomeric bodies. Carbon, sulphur, and phosphorus, for example, are bodies which have been recognised as presenting, in each case, three distinct allotropic states, and these are the bodies which constitute the bases of isomeric compounds. May we not, therefore, look for a similar interpretation of them? In the case of isomerism the facts are explained by supposing that the elements are differently grouped, and an appeal is often made to the radical theory to express this difference; it does so, according to Liebig, in the following manner. It represents acetic ether as a compound of oxide of ethyle and acetic acid, thus $C_4H_5+O+C_4H_3O_3$; and butyric acid as being composed of $C^8H_7O_3+HO$; while it regards aldehyde as a hydrated oxide of acetyle, C_4H_3+O+HO. Again, benzoic acid is represented as composed of water and the oxide of a hypothetical radical, benzoyle, or $C_{14}H_5O_2+O+HO$; and the hydruret of salicyle as being made up of an element of hydrogen with another hypothetical radical, thus $C_{14}H_5O_4+H$. We refrain from making any further comment upon these views, but rest satisfied with the admission that a different grouping has to do with the different properties; and so we pass on to our fifth question.

5. An opinion was generally entertained, a few years ago, that, when one body caused the decomposition of another, it did so, and could only do so, on account of a superior affinity which it, or its constituents, possessed for one or more of the elements of the decomposable body. It was supposed also, that, in the act of decomposition, it seized these elements and combined with them, so as to form one or more new substances. In modern times, this opinion has been completely refuted. Even as far back as 1818, Thénard discovered a compound, peroxide of hydrogen, consisting of two equivalents of oxygen, united with one of hydrogen, the elements of which are so loosely combined, that it readily undergoes decomposition upon the approach of many bodies, although these bodies do not enter into combination with it or its elements. Gold, silver, platinum, mercury, and many other metals resolve it instantly into water and oxygen, which last escapes with violent effervescence; and, more singular still, the oxides of these metals exert a similar influence, and, in the act of decomposition, their oxygen also is liberated and the metals reduced.

Another compound, the bisulphuret of hydrogen, was found by Thénard to be decomposed in a manner very similar to the last; and a third, the nitro-sulphate of ammonia of Pelouze, may be mentioned as endowed with analogous properties.

Again, in the decomposition of chlorate of potash by heat, it has been noticed that oxygen is liberated with much greater facility, if a little peroxide of manganese or oxide of copper, or even fine sand, has been previously mixed with the chlorate.

In all these cases the bodies appear to effect decomposition merely by their presence; they neither give to, nor take from, the compound. On this account they have been named "*contact decompositions.*" Berzelius has applied the term "*catalytic*" to them; and Liebig has studied them under the name of "*motion decompositions.*" Instances are very frequent in organic chemistry. They are also very important, and have been studied in the second part of Turner under the head of "transformations or metamorphoses of organic compounds," the terms being limited to such decompositions as take place without the violent action of a foreign body, often occurring without the addition of any such body, and seldom requiring more than the access of air for a time, and the presence of a ferment,—a substance which acts by its presence, but does not by its elements contribute to the formation of the new product.

"The processes usually included under this head are, fermentation, including putrefaction, and eremacausis, or decay.

"There is something in the very nature of organic compounds, which fits them for undergoing this succession of metamorphoses. All organic compounds contain three, most of them all four, of the organic elements; and having, moreover, a high atomic weight, they must of necessity have very complex molecules. If, as is probable, the smallest molecule of sugar contains $C_{12}H_{11}O_{11}$, that of stearic acid $C_{68}H_{66}O_{7}$, of uric acid $C_{10}H_{4}N_{4}O_{6}$, of indigo $C_{16}N_{5}NO_{2}$, while that of albumen contains at least $C_{48}H_{36}N_{6}O_{14}$ (besides a little sulphur and phosphorus), and possibly ten times more, then it is easy to see how such complex molecules, unlike the comparative simple formulæ of inorganic nature, must possess a most unstable equilibrium; or, in other words, must be very prone to decomposition, whether from the tendency of their elements to enter into new and simpler forms of combination, or from the action of external attractions on the complex molecule.

"Now, it is observed that the organic compounds which are most prone to change, and which are spontaneously decomposed, are all compounds of nitrogen. It is also ascertained, that organic compounds, destitute of nitrogen, do not enter into fermentation, putrefaction, or decay, unless a nitrogenized body be present, which first enters into decomposition, and then acts as a ferment, exciting decomposition in the non-nitrogenized body.

"This is well illustrated in the fermentation of grape-juice. This fluid contains sugar; but a pure solution of sugar does not ferment. The fermentation of the sugar is here caused by the presence of an albuminous compound (a compound containing nitrogen) dissolved in the juice, which first undergoes that kind of fermentation which is called putrefaction, and, when in this state, excites fermentation in the sugar.

"There are two theories of its action. According to that of Berzelius, it acts by contact, catalytically; that is, by contact with the sugar, it somehow causes the latter to be decomposed, just as sulphuric acid, in contact with alcohol, causes it to be resolved into ether and water. But this is not strictly an explanation; for it merely states the fact to be explained. And, besides, the action of sulphuric acid on alcohol is not correctly described. The acid first combines with the ether, and, on raising the temperature a little, the new compound is destroyed, and the ether distils over.

"The other theory is that of Liebig. He observes, that, in innumerable cases, the contact of a body in a state of change, or of motion in the particles, causes a state of motion in the particles of another body in contact with it, and thus produces decomposition. Thus, when peroxide of hydrogen, in a state of decomposition, is put in contact with oxide of silver, instead of yielding oxygen to that oxide, it reduces it to the metallic state; that is, the motion of the particles of the peroxide of hydrogen, being mechanically communicated to those of the oxide of silver, the equilibrium of the latter compound is overthrown, and the oxide of silver loses all its oxygen in a solution which we should have expected to yield oxygen to it. Again, nitric acid cannot dissolve platinum; but it dissolves an alloy of platinum and silver, because the particles of the silver, while entering into combination, communicate mechanically a like state to those of the platinum.

"Now, when a ferment or exciting body comes into contact with sugar, the motion of the particles of the ferment being mechanically communicated to those of the sugar, which are held together in an unstable equilibrium, that equilibrium is destroyed, and the particles of the sugar assume new forms of combination, in this case alcohol and carbonic acid, which are more stable under existing circumstances.

"The elements of the ferment do not contribute to the formation of the new products, alcohol and carbonic acid. The ferment is itself resolved into new products, and gradually disappears. If it have disappeared before all the sugar is decomposed, then the fermentation stops until new ferment is added. In sweet wines, the ferment has been exhausted before the sugar. If, on the other hand, the sugar is all decomposed and some ferment is still present, then a new fermentation, or rather decay, sets in, and the alcohol becomes converted into acetic acid. This is the reason why weak wines are generally acid." (pp. 688–91.)

Such is Liebig's theory of fermentation; and he considers it sufficient to explain all such phenomena in a scientific manner. He argues that many bodies may act as ferments; in fact, any decomposing matter, whose particles are in a state of motion. In this way, yeast (decomposing gluten) is a ferment to sugar; so is curd, putrid flesh, or other animal matter. Emulsin is a ferment to amygdalin; diastase to starch.

By an extension of this theory, Liebig endeavours to account for many of the phenomena, both healthy and morbid, which are going on in the living body. Thus, he regards digestion as a species of fermentation set up by the decomposing epithelium, which is thrown off from the inner coat of the stomach; and he believes that the formation of butyric acid in the cow is due to the fermentation of the sugar and starch of its food. Again, he views the ill consequences of a dissecting-room puncture as but the result of a fermentation, occasioned by the putrid matters thus introduced into the system; and, he says, "as little can it be doubted that some poisons, as contagions and miasmata, act as ferments to the blood, or to the substances in it." (p. 693.)

The immunity of some individuals from fever, or from the exanthemata, and the exemption of most from a second attack of the latter, is due, according to him, to the absence of the fermentable element, and to its having been exhausted by the previous attack.

It may be fairly objected to these opinions, that they indicate a very great reliance upon the influence of physical agents, and an almost total disregard of vital ones. Thus, Liebig is blind to the fact that in yeast there is a living structure, that a like organism is found when curd produces the fermentation of sugar, that myriads of living creatures are present wherever putrefaction and fermentation are going on, that the coat of the stomach is a living tunic, and that most pathological changes can be traced to a living product, to a new or an altered organism—a cell.

It is curious, however, when we reflect upon these doctrines of Liebig, that we should find ourselves, in the nineteenth century, going back again to one of the most ancient theories in medicine. Hippocrates, the father of physic, regarded fevers, and some other diseases, as resulting from the action of a morbid matter in the blood, which, by a process of concoction, was brought after a short time into a state fitted to be expelled by the body. It was then thrown off by hemorrhage, by sweat, by alvine discharges; or it was deposited upon the surface of the body in the form of abscess, or cutaneous eruption: and these evacuations and eruptions constituted the crisis of the disease. This piece of old-fashioned humoral pathology has appeared, in one shape or another, at various times; nevertheless, although its advocates can boast of its antiquity and of its numerous and illustrious followers, and although its present form be comparatively definite and satisfactory, we must still remember that its basis is rather in speculation than in a broad foundation of fact.

But, besides fermentation, other chemical changes are brought about by the influence of decomposing organic matter; such as *putrefaction* and *eremacausis*.

> "In putrefaction the access of air is necessary, at all events, to commence the process; and here the elements of the ferment, or exciting body, probably do contribute, with those of the putrefying body, to form some of the new products, which are very varied. This process, although essentially the same as fermentation, is commonly characterized by the offensive smell of some of the products, among which are ammonia, sulphuretted hydrogen, and perhaps phosphoretted hydrogen, since those bodies which are prone to decomposition, are commonly compounds, containing nitrogen, sulphur, and phosphorus." (p. 691.)

From this it would seem that putrefaction differs from ordinary fermentation in two respects: first, the ferment *does* contribute some of its elements to the decomposing matter; and, secondly, the elements of the putrefying body resolve themselves into the simplest combinations.

> "Eremacausis, or decay, is a slow combustion, and is throughout dependent on a supply of oxygen from the air. Any moist organic body, exposed to the air, soon enters into eremacausis, and during this process, gives out a volume of carbonic acid equal to that of the oxygen absorbed.
>
> "The decay of wood and the production of mould from dead leaves are good examples of eremacausis; as are also the conversion of alcohol into acetic acid, and that of pyroxilic spirit into formic acid,—which, although sometimes called fermentation, are cases of pure oxydation.
>
> "Those substances which check fermentation, putrefaction, and decay, are called antiseptics. Such are strong acids, corrosive sublimate, and other metallic salts, creosote, and arsenious acid. They appear to act by entering into firm combination with the fermentable body, and thus opposing an obstacle to the motion of the particles." (pp. 692–3.)

It is easy to give these facts another interpretation, and to say that they do it by checking the growth or life of certain organisms.

We must content ourselves with these quotations, expressive of Liebig's views, and refer our readers to the work before us, and to Liebig's Lectures in the 'Lancet' for 1844, for a more complete exposition of them, and of the applications which he has made of them. We refer them also to pages 680 to 688, for a very full and clear account of the changes which organic bodies undergo when they are subjected to the influence of strong decomposing agents.

6. In another department of our science, the advances of the modern chemist have been particularly successful; namely, in the production of organic bases, some of which are identical in composition and properties with natural ones.

These discoveries, due chiefly to the researches of Wöhler, Liebig, Hoffmann, Laurent, Zinin, and Fownes, have tended to overturn the opinion of our predecessors, that organic compounds, resulting from the operation of the living force, or from processes taking place in the living body, could not be produced by artificial means; that, in fact, the effect of the chemical force was always to break up organic compounds, and to reduce them to simpler combinations. It is now found, however, that the chemist can, in many instances, bring about an action of an opposite character;

he can often build up as well as pull down, and thus make the molecules of simpler groups coalesce and form a more complex one. At present, this power is but feebly manifested, yet it gives hopes of becoming stronger, as may be seen from the following generalizations, which we quote from Turner:

> "The artificial alkaloids are formed in several different ways. As, however, they all contain nitrogen, if we except the oxide of kakodyle and the oxide of kakoplatyle, ammonia is generally directly or indirectly concerned in their formation. Artificial urea is produced by a spontaneous metamorphosis of cyanate of ammonia; melamine and ammeline, by the action of potash on sulphocyanide of ammonium; sinnamine and the allied bases, by the action of ammonia on oil of mustard; amarine, picrine, and lophine, by the action of ammonia on the oil of bitter almonds and its derivatives; furfurine, by the action of ammonia on furfurole; and thialdine and selenaldine, from aldehydammonia, by means of sulphuretted and seleniuretted hydrogen. Naphthalidine, seminaphthalidine, aniline, and toluidine are obtained by the action of sulphuret of ammonium on nitronaphthalase, nitrobenzide, and nitrotoluide. But some alkaloids are formed without the aid of ammonia. Thus, sarcosine and kreatinine are obtained by the action of bases and of acids on kreatine; glycocoll, by the action of acids on hippuric acid, or of potash on gelatine; aniline, picoline, and leucoline, by the destructive distillation of nitrogenized compounds; in which latter case, however, ammonia may probably act; and aniline, chloraniline, bromaniline, with some other allied compounds, are obtained by the action of potash at a high temperature on isatine, chlorisatine, bromisatine, and their congeners. It is probable that other methods of producing basic compounds will be discovered; in the mean time, it is interesting to observe, that the known methods are chiefly processes of reduction or deoxidation; insomuch, that several of these artificnl alkaloids are destitute of oxygen, and in all the proportion of oxygen is small, as it is in the natural alkaloids." (p. 1214.)

And, with respect to the views which spring out of the study of these, it is observed:

> "Of the constitution of the vegetable bases, nothing certain is known; but the modes of formation of the artificial alkaloids have suggested certain views as possible, or even probable.
>
> "In the first place, it may be considered very probable, that all the alkaloids are formed by substitution from some other substances; as, for example, analine from nitrobenzole, and toluidine from nitrotoluole.
>
> "In the next place, some of the alkaloids may be viewed as compounds of amide, as, for example, aniline, which has the composition of phenylamide—$C_{12}H_7N = C_{12}H_5 + NH_2$. The other artificial bases, analogous to aniline, may be regarded in the same way; leucoline as $C_{18}H_6 + NH_2$, or toluidine as $C_{14}H_7 + NH_2$; being derived, on this view by substitution of amide, either for hydrogen in the carbo-hydrogens $C_{12}H_6$, $C_{18}H_7$, and $C_{14}H_8$, or for nitrous acid in the compounds $C_{21}\left\{\begin{matrix} H_5 \\ NO_4 \end{matrix}\right\}$, $C_{18}\left\{\begin{matrix} H_6 \\ NO_4 \end{matrix}\right\}$, and $C_{14}\left\{\begin{matrix} H_7 \\ NO_4 \end{matrix}\right\}$. This class of alkaloids, which contain no oxygen, seems to have the simplest constitution.
>
> "But, thirdly, there is, among the artificial alkaloids, a substance which may be taken as the type of a somewhat more complex class, in which the hydrogen of the fundamental compound of the series has been replaced by some compound of nitrogen and oxygen. This substance is nitraniline, which is $C_{12}N_2H_6O_4$,
>
> $= C_{12}\left\{\begin{matrix} H_6 \\ NO_4 \end{matrix}\right\}N.$
>
> "Fresenius suggests that many of the natural alkaloids, containing 4 eq. of oxygen and 2 of nitrogen, may have a constitution analogous to that of nitraniline, in which 1 eq. of hydrogen is replaced by 1 eq. of nitrous acid. For

example, strychnine, $C_{44}N_2H_{22}O_4$, may be $C_{44}\left\{\begin{matrix}H_{22}\\ NO_4\end{matrix}\right\}N$. Quinine, menispermine, staphisaine, and delphine, may all be associated with strychnine.

"Chelidonine $C_{40}N_3H_{20}O_6$, may be, according to Fresenius, binitrichelidonine, $C_{40}\left\{\begin{matrix}H_{20}\\ 2NO_3\end{matrix}\right\}N$; 2 eq. of hydrogen being replaced by 2 eq. of hyponitrous acid.

"Where the alkaloid contains 2 eq. of oxygen and 2 of nitrogen, the replaceable body may be deutoxide of nitrogen. Thus, caffeine, $C_8N_2H_5O_2$, may be nitre-caffeine, $C_8\left\{\begin{matrix}H_5\\ NO_2\end{matrix}\right\}N$; and to this class would belong cinchonine and sinapoline. Urea may be added, as it may be $C_2\left\{\begin{matrix}H_4\\ NO_2\end{matrix}\right\}N$.

"Another class may contain protoxide of nitrogen as the replacing substance, Thus, harmaline, $C_{24}H_{13}N_2O$, may be nitra-harmaline, $C_{24}\left\{\begin{matrix}H_{13}\\ NO\end{matrix}\right\}N$; and theobromine, $C_9H_5N_3O_2$, may be binitra-theobromine, $C_9\left\{\begin{matrix}H_5\\ 2NO\end{matrix}\right\}N$.

"Lastly, hydrogen may even be supposed to be replaced by nitric acid; and on this supposition, jervine, $C_{60}H_{45}N_2O_5$, might be nitro-jervine, $C_{60}\left\{\begin{matrix}H_{45}\\ NO_5\end{matrix}\right\}N$.

"The bases above mentioned, if such be their true constitution, are derived from radicals containing no oxygen, as aniline from benzole, &c. But there are others which are probably derived from radicals containing oxygen. Such bases are morphine, codeine, narcotine, and others. Of these, however, in this point of view, so little is known, that, as Fresenius observes, it is safest, for the present, to consider them as simple alkaloids, analogous to aniline, nicotine, or sinnamine,

"The views of Fresenius have a high degree of probability, and will probably aid in enabling us to discover the true nature of the alkaloids, He points out, that, on his theory, various facts connected with the alkaloids are easily explained. Thus, for example, it is easy to see, on his theory, that, when heated with potash, ammonia is not given off; that their power of saturation cannot be proportional to their amount of oxygen or of nitrogen, as it is well known not to be. On the other hand, it appears to be proportional to the amount of nitrogen which is not in the form of an oxygen compound, such as NO, NO_2, NO_3, NO_4, or NO_5. This nitrogen generally, perhaps always, amounts to 1 eq. in 1 eq. of the base, as may be seen in Fresenius's formulæ. The simple alkaloids, such as aniline, contain only that 1 eq. of nitrogen, just as ammonia does.

"It must here be remarked, that the alkaloids all appear to be of the same general type as ammonia, and that, like ammonia, they unite directly with hydrogen acids, while, to combine with oxygen acids, they take up 1 eq. of water. This leads to the conclusion, that if, in the case of ammonia, there is formed a compound analogous to metals, ammonium, which unites with salt radicals, while its oxide combines with oxygen acids, the same is probably true of the alkaloids. Thus, when ammonia acts on hydrochloric acid, we have $NH_3 + HCl = NH_4 + Cl$, chloride of ammonium; and when aniline combines with the same acid we may suppose the change to be analogous; $C_{12}H_7N + HCl = C_{12}H_8N + Cl$, chloride of anilium. Again, sulphate of ammonia, NH_3, HO, SO_3 is usually considered to be sulphate of oxide of ammonium, NH_4, O, + SO_3; and so, in like manner, sulphate of aniline, $C_{12}H_7N$, HO, SO_3, is probably sulphate of oxide of anilium, $C_{12}H_8N$, O + SO_3. If true of one, this is probably true of all.

"Such is the present state of our knowledge with regard to the alkaloids; and, although it is still very imperfect, yet it is evident that we are now making some progress in this department of science, and may hope speedily to advance much further." (p. 1231.)

Upon this question, they further observe, that—

> "It is now quite conceivable that we may discover the means of producing, for example, a body having the composition and chemical properties of albumen, or gelatine. But there is no reason, derived from the present state of our knowledge, for thinking that we shall ever be able to give to such a compound the form of an organized tissue." (p. 1308.)

And again we find at p. 693:

> "While these facts prove that we can imitate some of the results of organic life, yet their very nature shows that there are conditions in the animal and vegetable economy, which differ from those which we can command. In all these cases we can only obtain one organic product from another, and the compounds which we imitate are in no case such as form a part of organic tissues. They are rather products of decomposition and excretions, such as urea and butyric and formic acids, approaching in simplicity of constitution to inorganic products. We cannot, like plants, form organic tissues from inorganic matter: we can merely decompose some organic products, and cause some metamorphoses, but we are still as far from penetrating the mystery of the formation of albumen or fibrine, or the production of a cell. We cannot doubt that albumen and fibrine are chemical compounds, formed by the action of chemical laws; but these laws act under a modifying influence, namely, the vital force, and hence produce results which we cannot imitate in the laboratory, and which may for ever remain beyond our power."

7. With respect to the last question which we have proposed for consideration, it cannot be doubted that the chemist has obtained results, from his elaborate investigations into the composition of the various parts of plants and animals, from his analyses of their numerous products, and from his inquiries into the variety of changes that organic matter is capable of undergoing, which have thrown a strong light over the phenomena of health and disease. These results have pointed to the relation which exists between the composition of all living beings and that of their food; they have demonstrated also that many of the functions of the living body are due to the action of the chemical force, and are subject to the influence of chemical laws.

We refer our readers to this edition of Turner, and especially to a chapter in it "On the changes which occur during the life, growth, and nutrition of vegetables and animals," for a very full account of the numerous points which bear upon this question. The following is a brief summary of them:—

The food of Vegetables consists entirely of inorganic compounds; and no organized body can serve for their nutrition until it has been resolved, by the processes of decay and putrefaction, into certain inorganic compounds, as water, carbonic acid, and ammonia. It is necessary, also, that plants should be supplied with certain mineral substances, as silica, lime, potash, magnesia, soda, oxide of iron, and sulphuric and phosphoric acids. According to Liebig, the whole of the carbon appropriated by vegetables is derived from carbonic acid supplied to them chiefly by the atmosphere, but partly also by the water absorbed by the roots.

It is probable that the fixation of carbon is a gradual process, having successive stages; that carbonic acid is first reduced or deoxidised, so far as to yield oxalic acid; then, with the aid of water and a farther reduc-
is converted into malic, or citric, or tartaric acid; from which and
compounds sugar, gum, starch, and woody fibre are easily deduced;

and there is good reason for believing that the chief function of the alkalies in plants is to promote these metamorphoses.

The hydrogen and oxygen of vegetables is derived from water; and it is worthy of notice that the great mass of vegetables consists of carbon and water, or of carbon with oxygen and hydrogen in the proportion to form water.

The nitrogen of vegetables is derived chiefly, if not exclusively, from ammonia; which is supplied to them in rain, or from the soil in the water absorbed by the roots.

There is evidence that humus is not absorbed as such by plants, but rather that it decomposes or decays, and furnishes carbonic acid and ammonia to the plant, together with mineral saline matters. Humus may also act by absorbing ammonia, and then giving it out to the roots of the plant.

These views seem to point to the chief use of manures; namely, to supply the plant with mineral substances, and with ammonia, which is the great stimulant to vegetable life. In using manures, therefore, we should always consider the mineral wants of the vegetable which is to furnish any particular crop; thus, potatoes require ammonia and magnesia; hay, phosphate of lime; wheat, silica and phosphate of lime; while, for almost all plants, potash and ammonia are highly beneficial, and sulphates and phosphates indispensable.

Many manures are chiefly valuable for the ammonia they yield, such are guano, nightsoil, urine; but it must never be forgotten that if plants are supplied, either from the soil or in the manure, with the indispensable mineral salts, namely, the alkalies, silica, phosphates, sulphates, lime and magnesia, they will supply themselves with ammonia and carbonic acid from the atmosphere. The effect of supplying ammonia in the manure is simply to stimulate the growth of a plant, to make it complete its growth in a shorter period; a matter of the utmost importance in our climate, but of much less in tropical and southern climates, where the summer is so much longer.

We do not, at present, appear to know anything for certain of the changes which take place in the juices of vegetables, by which the numerous products of the vegetable kingdom are formed; but out of the inorganic kingdom they glean the elements for the formation of sugar, gum, starch, and albumen. These, the final products of vegetation, become the food of animals; the last serving for their nutrition, the former for the support of respiration.

The life of Animals is distinguished chemically from that of vegetables by the circumstances, that in the former oxygen is constantly absorbed and replaced by carbonic acid, while in the latter, carbonic acid is absorbed, its carbon retained, and its oxygen given out.

In animals two processes are constantly going on: respiration, by which the animal heat is maintained; and nutrition, by which the wear and tear of the system is repaired.

Respiration is essentially the slow combustion of carbon and hydrogen, which, in combining with oxygen, form carbonic acid and water, and give out heat.

The carbon is derived chiefly from the tissues which are constantly undergoing waste. This is almost the only source of it in carnivora, whose food consists of compounds rich in albumen and fibrine; and as the exercise of the vital functions is necessary to supply this waste, there is reason why they should use such an enormous amount of muscular motion. On the other hand, in herbivora, their food is composed of but little of the albuminous compounds, only just enough to repair a small waste; and hence we see that no such amount of motion is called for in them.

In hot climates, where less heat is required, the appetite is more feeble, and the foods generally made use of contain less carbon; whereas, in cold climates the appetite is keen, and the food highly carbonised to meet the wants of the system in the production of animal heat.

It is in the form of bile, chiefly, says Liebig, that the carbon undergoes combustion; he, therefore, does not admit that the bile is an excrementitious product. He believes that it is reabsorbed from the lower part of the alimentary canal, and enters the circulation to be burned. Liebig further believes that his recent discovery of a large amount of lactic acid in the juice of flesh, renders it highly probable that the bile is resolved into lactic acid and other products before it is finally consumed.

In endeavouring to explain the formation of the bile, he thinks it of no moment whether we derive it from the albumen, fibrine, &c., of the food, or from the materials of the tissues; their composition being identical. In his 'Animal Chemistry,' Liebig endeavoured to show, taking the then admitted formulæ for bile and for blood, that the latter fluid, with the addition of water and oxygen, might be resolved into choleic acid and urate of ammonia, that is, into bile and urine. The recent discovery of the presence of sulphur in choleic acid renders a revision of these formulæ necessary; but still Liebig believes that this may indicate the sort of change which is perhaps effected.

Suppose this to be well founded, Liebig says, the tissues which are consumed are resolved into bile and urate of ammonia; the former is secreted from the liver and reabsorbed, the latter in some animals, as serpents and birds, is expelled unchanged; but in man and quadrupeds, in which the amount of oxygen inspired is much greater, it also is oxidised, yielding finally carbonic acid, ammonia, and urea. Should the supply of oxygen in the human body be insufficient, or otherwise appropriated, it is deposited as uric acid and gravel; but if the supply of oxygen be somewhat greater, but still deficient, oxalic acid is the result, and mulberry calculus occurs; but if much exercise be taken and abundance of oxygen supplied, the oxidation of the uric acid is completed, and nothing is left but urea or carbonate of ammonia. This would argue that the true cause of uric acid and mulberry calculi was a deficiency of oxygen.

The urine of herbivora contains benzoic acid, when they are in full exercise and the supply of oxygen is great; but hippuric acid when they are at rest or stall-fed.

The bile of the herbivora is much greater than that of the carnivora, inasmuch as they cannot appropriate it.

Soda is necessary to the formation of the bile; and where the supply of common salt is deficient, the metamorphosis of albuminous compounds can yield only fat and urea. It is worthy of remark, says Liebig, that if we wish to fatten an animal, we must carefully avoid giving much salt in its food.

With respect to the changes which food undergoes in digestion, he observes, that as we cannot discover the solvent principle of the gastric juice, we must admit that the food is digested by a process similar to fermentation.

As regards the process of respiration, he says there is reason to believe that the globules of the blood possess the property of absorbing oxygen in the lungs, and of conveying it to all parts, where it acts on the tissues destined to undergo change, and produces a metamorphosis, by which new soluble compounds are formed, the chief of which, containing carbon, yield bile in traversing the liver, while the nitrogen accumulates and is separated from the arterial blood in the kidneys in the form of urea or uric acid. But, besides this, other portions of the tissues are oxidised, and carbonic acid formed, which passes on into the venous blood. It is not known, he says, what chemical compound in blood absorbs and carries oxygen; but it is conjectured by him, that it is a compound of iron, analogous to the protoxide. It is certain, he says, that iron is indispensable to the blood; and it is remarkable, that sulphuretted hydrogen and hydrocyanic acid both instantly destroy the power of the blood to perform its functions, —hence their terrible energy as poisons when inhaled. Now these compounds both act on protoxide, protochloride, and other analogous compounds of iron, immediately depriving them of their characteristic power of acting on oxygen. With regard to the means whereby the carbonic acid so formed is conveyed to the lungs, Enderlin has proved, at the suggestion of Liebig, that the common phosphate of soda, which exists in the blood, is admirably adapted for this purpose, and that this, in all probability, is the compound which effects it.

"It is truly remarkable, that, while both phosphate of soda and phosphate of potash exist in the food, the former alone should occur in the blood; and this is especially wonderful when we consider that the juice of the flesh, which is only separated from the blood by various thin membranes, permeable to liquids by endosmose and exosmose, contains much phosphate of potash, and little or no phosphate of soda. It is evident that the vessels or cells must possess in their peculiar membranes a power of selection, or of allowing some salts to pass in one direction only, and others in the opposite.

"Another circumstance worthy of notice is, that when earthy phosphates act on salts of potash, they always produce an acid salt, namely, tribasic phosphate of potash, with one eq. of *fixed* base, PO_5, KO, 2HO; while, when the same earthy phosphates and phosphate of potash act on salts of soda, they invariably produce an alkaline salt, namely, the phosphate of soda above mentioned, with 2 eq. of fixed base, PO_5, $2N_2O$, HO.

"Now the former of these, the acid phosphate of potash, the only phosphate that can be formed by an animal from food containing salts of potash without soda, such as inland plants, is the essential salt of the juice of flesh, but totally unfit for the formation of blood. The phosphate of soda in blood *cannot be replaced* as to its functions by phosphate of potash. Here we see the beautiful discoveries of Graham concerning the phosphates, discoveries which at no distant period might have been regarded as possessing only a purely scientific interest, furnishing an explanation to some of the most important links in the chain of vital processes. There can be no doubt that the function of the acid salt, the phosphate of potash, in the juice of flesh, and apparently also in the gastric juice, is as important as that of phosphate of soda in the blood. Probably a part of that function is to ensure the constant acidity of these fluids, as phosphate of soda does the constant and essential alkalinity of the blood, on which, as we have seen, the power of absorbing and giving out carbonic acid, in other words, respiration, depends. And we see, too, that if this be so, the phosphate of potash of the juice of flesh and of the gastric juice cannot be replaced, as far as its function is concerned, by phosphate of soda." (p. 1320.)

Animals cannot, therefore, live without soda; no blood would be formed unless they partake of common salt, which reacts on the earthy and potash phosphates contained in their food, whereby are formed chloride of potassium and the common alkaline phosphate of soda; the former is found in their flesh, the latter in their blood.

> "It is," says Liebig, "truly a spectacle worthy of admiration to see the essential properties of two of the most important animal fluids, the blood and the juice of flesh, thus secured by the existence of a difference, at first sight insignificant, between the relations of phosphoric acid to two alkalies which so much resemble one another that they may be mutually replaced, each by the other, in a multitude of cases, nay, do actually replace each other in many plants." (p. 1331.)

Such is a condensed account of the chief of the new views put forward in the eighth edition of Turner. They are expressive of the peculiar opinions of Liebig, a chemist who has done more, by bold, and sometimes extravagant, speculation, to advance the sciences of chemistry and chemical physiology, than any man living. Not that he has of himself, as far as we know, raised a single stone which shall endure in the permanent fabric of either science; but he has done this,—he has pulled down the old building, has cleared the ground from prejudice and error, he has sketched out a plan of operation, and he has awakened the energies of men who will raise an edifice for all time.

We cannot at present attempt a criticism of the views which are here advanced; for it would involve the addition of a long discussion to an article already exceeding its destined limits. But we will take leave to say, that most of them are as yet unproven, that many of them are doubted, and others absolutely denied; that they have been raised, in many cases, upon uncertain foundations, such as the chance coincidence of a few chemical formulæ, the curious resemblance of some laboratory-product to an organic compound, or upon the most imperfect notions of matters which pertain neither to ancient nor to modern physiology. Theories founded upon such bases cannot be expected to stand; they must either turn to the wind of every new doctrine, or they must fall. Even since the last edition of this work, the whole of the section, which we have here epitomised, has been remodelled to suit the new views of Liebig. The editors, for example, have omitted all those brilliant-looking formulæ, those refined calculations, which served then to explain so fully and so accurately the formation and the transformation of animal tissues. Views, too, which were then proclaimed as among the most important of modern discoveries, are now set aside and spoken of as inconsistent and untrue. We allude especially to the proteine theory of Mulder; a theory which, in the edition of 1842, was spoken of as having been tested and confirmed by experiments in the Giessen laboratory, and was even made the groundwork of various calculations and reasonings; this theory, in the edition of 1847, is said to be untenable and irreconcileable with known facts. For, in the first place, the theory (say the editors) assumes that—after deducting the sulphur, phosphorus, and salts,—albumen, fibrine, caseine, &c., have exactly the same composition; but it is found from the analyses of Thénard, of

Michaelis, of Vogel, and of Fellenberg, that fibrine contains more nitrogen than albumen, &c.

Secondly, if the first action of potash produces proteine, as Mulder says, and in the way he says, we ought to get an amount of proteine nearly equal to that of the original compound; but this is not the case; it is always in very small quantity, and the quantity differs for albumen, caseine, and fibrine; indicating that there are other products, not yet examined, which are also formed.

Thirdly, on Mulder's theory, proteine does not contain sulphur; but recent researches have shown that a compound free from sulphur cannot be obtained by Mulder's process.

And, fourthly, the investigations of Laskowski, Rüling, Verdeil, Walther, Fleitmann, and others, show that the amount of sulphur present in albumen, caseine, fibrine, &c., is three or four times as great as it would be if Mulder's formulæ were correct, and that the quantities are different from one another.

Under these circumstances, the editors think that the composition of the albuminous compounds is not yet established so accurately, as to permit them to infer their formulæ.

Mulder, however, while admitting that some inaccuracies existed in his previous analyses, and that he had overlooked a little sulphur which pertinaciously adheres to proteine, still maintains that the vegetable and animal albuminous bodies contain an organic group free from sulphur. His present formula for proteine is $C_{36}H_{25}N_{4}O_{10}+2HO$. And, secondly, he maintains that there are two groups to be considered as higher degrees of oxidation produced from proteine.

It will be seen from this, that the group contains less hydrogen and nitrogen than he imagined when he made his former expression of its composition ($C_{40}H_{31}N_{5}O_{12}$); these he combines as amides in albumen and fibrine, which, he says, are compounds of proteine with sulphamide and phosphamide.

He still maintains, also, that their oxides play the most important part in the functions of nutrition and respiration.

We do not pretend to decide between these rival opinions; but we cannot help saying that if Mulder's views are so entirely wrong, Liebig was wrong also in adopting them so fully in his earlier writings; nor can we help expressing regret at the ill feeling and the truly unphilosophical spirit with which this controversy has been conducted.

In conclusion we freely recommend to our readers a perusal of this edition of Turner; being convinced, as we have before said, that it gives a true and complete account of the facts of modern chemistry; and although the theories which have been advanced in it are not fixed upon so sure a footing as they might be, yet they are put forth in such a manner as is most likely to engage the attention of the physiologist, and to lead him to make further inquiries. It is on this account that Liebig deserves our thanks; for he has, by his bold though unproven speculations, led to the discovery of facts which have alike enriched the chemist and opened a new mine of research to the physiologist.

ART. XI.

Elements of General and Pathological Anatomy, presenting a View of the Present State of Knowledge in these Branches of Science. By DAVID CRAIGIE, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Edinburgh, and Honorary Consulting Physician to the Royal Infirmary. Second Edition, enlarged, revised, and improved.—*Edinburgh*, 1848. 8vo, pp. 1088.

WE rise with feelings of considerable disappointment from the perusal of Dr. Craigie's ponderous volume. It would be too much, perhaps, to assert that we have gained nothing by its study; but our profit has been limited to the refreshment of our memory, in relation to facts almost forgotten except by the admirers of the dark ages of normal and morbid histology. We have not met with a *single new fact or new idea* in the whole of this volume; nay, more, we have hardly found a fact or idea of ordinarily modern date. If some ten or twelve pages were omitted, we conscientiously believe the volume might have been published ten years ago. This statement may appear strong regarding a work, which, we are told, has "been greatly increased by the introduction of new matter under the proper heads, in order to carry forward to the present time (4th Nov., 1847) the information acquired since the appearance of the first edition;" but our readers will soon judge whether it is not true.

We cannot proceed to the consideration of the actual work, without noticing a singular statement in the advertisement prefixed to it, with regard to the value of plates illustrating microscopic objects.

> "By some it may be expected that this work should have been illustrated with delineations, more especially in reference to microscopic anatomy. These, however, would have added so much to the expense of the work, without otherwise increasing its value, that it was thought best for the present to dispense with their assistance."

A suggestive sentence, truly! We confess that we are among the contemptible "some" who think that the blood, the nervous, areolar, muscular, osseous, and cartilaginous tissues, the membranes and glands, the products of inflammation, tubercle and cancer, and other subjects too numerous to mention, falling within the scope of general and pathological anatomy, cannot be properly explained without diagrams or plates illustrating their minute structure and arrangement. If we are wrong in the expression of this opinion, we err with Muller, Henle, Valentin, Gluge, Vogel, Lebert, and all the best anatomists in Europe, to say nothing of our own countrymen, Todd and Bowman, Sharpey, Carpenter, Goodsir, and Hughes Bennett. As, however, there is no internal evidence that Dr. Craigie is aware of even the existence of such men as Todd, Sharpey, Carpenter, Goodsir, and Hughes Bennett, it would be unjust to blame him for the non-introduction of what, in the innocence of his heart, he seems to consider would have been a dangerous novelty; and we willingly accept his *naïve* assurance, that the plates alluded to *would only have increased the value of his work by adding to its expense.* Moreover, after showing in so satisfactory a manner the reason why there should be no plates, we can hardly understand the force of the observation that it is only "*for the present*" their assistance is dispensed with.

It would be an unprofitable and useless act of barbarity, both to Dr. Craigie (for whose labours in other departments of medical science we entertain the highest respect), and to the numerous readers of our Journal, to present them with a lengthened analytical review of this volume. We must, however, in justice to ourselves, show them that our opinion is based on no light or superficial grounds; and as it unfortunately happens that almost every chapter in the book is equally open to the objections we have brought forward, we will commence with Book I, Chapter I.

The first chapter is devoted to the consideration of the *Division of Textures*. There is something startling in finding that our author regards Bichat's division of tissues, slightly modified, as the best to be adopted at the present time. The theories of cell-development and of the general structure of tissues should properly find a place in this chapter; and we have a faint idea that they are referred to in the following sentences; at all events, there is no other allusion throughout the entire volume to the structure of a cell. The view regarding organization is, moreover, hardly one that we should advise a student to adopt:

> "These organized solids agree in the possession of certain general characters. Their internal structure consists of a union of solid and liquid matter, which is observed to exude in drops more or less abundant from the surface of sections. The solid parts are generally arranged in the form of collateral lines, sometimes oblique, sometimes perfectly parallel, sometimes mutually intersecting. Such lines are denominated *fibres*, and occasionally *filaments*. In other instances the solids are observed to consist of minute globular or spheroidal particles, which are shown by the microscope to be cells or membranous cavities with a central nucleus, connected generally by delicate filaments. Most of these solids, anatomists and microscopical observers have attempted to resolve into what they conceive to be an ultimate fibre or last element; but this inquiry leads beyond the bounds of strict observation.
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> "Most of the solids may be demonstrated to be penetrated by minute ramifying tubes or blood-vessels, which traverse their substance in every direction, and in which is contained the greater part, perhaps the whole, of the fluid matter found in the solids. In a few in which ramifying vessels cannot be positively demonstrated, their existence is inferred by analogy from those in which they can. The filamentous, fibrous, or globular or cellular arrangement, with the distribution of arborescent vessels, constitutes organization. The substances so constructed are named *organized tissues* (*telæ*, *textus*) or *textures*, or simply *tissues*." (p. 15.)

If Dr. Craigie will refer to the 'Philosophical Transactions' for the year 1841, he will find a paper by Mr. Toynbee on the non-vascularity of cartilage, which may induce him to alter some of the opinions maintained in the above extract.

But let us move our ground, and proceed to Chapter II, *On the fluids of the human body*. Do our readers wish for a clear and succinct account of the microscopic appearances presented by the blood? Here they are:

> "According to the results of microscopic observation, it [the blood] consists of red particles suspended in a serous or sero-albuminous fluid. On the shape of these red particles various opinions have been maintained. Generally represented as globular, Hewson describes them as flattened spheroids or lenticular bodies, a view which is confirmed by the observations of Prevost and Dumas, Beclard, of Hodgkin and Lister, and Mr. Wharton Jones. The opinion of Home and Young, that the flattening of these globules is a process posterior to the discharge of the fluid, is not improbable.[!] These particles have, since the time of Hewson,

> been almost universally represented as consisting of a central transparent, whitish globule, inclosed in a red, translucent vesicle, which gives them the shape of an oblate spheroid. In man and the mammalia they are circular discs, often with a depression on the sides. The diameter of these particles is estimated by the subdivided scale of Kater, the micrometer of Wollaston, and the eriometer of Young, at $\frac{1}{5000}$, and by the common micrometer at $\frac{1}{1700}$ of an inch. (*Phil. Trans.*) Mr. Gulliver estimates the average thickness of the human blood-corpuscle at $\frac{1}{12400}$th part of an English inch, and the diameter at $\frac{1}{3200}$. This description applies to the blood circulating in the vessels.
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> "The flattening of the corpuscles is greatest in reptiles, amphibia, and fishes; and is most remarkable in the salamander. In birds the red globules are flattened, but in less degree than in the amphibia. The red particles are largest in the amphibia. In birds, reptiles, and fishes they are smaller. In mammalia they are smallest. (Gulliver apud Hewson, p. 226.]" (pp. 18-19.)

The statement in the preceding sentence that, measured by the subdivided scale of Kater, the micrometer of Wollaston, and the eriometer of Young, the diameter of the corpuscles is the 5000th of an inch, while by the common micrometer it is the 1700th of an inch, conveys exactly the same amount of information as if we were to say that the Nelson monument was found by trigonometrical measurement to be 150 feet high, but that by a common three-foot rule its altitude was determined at 500 feet.

We do not know whether Dr. Craigie has disproved the existence, or whether he has never heard, of the colourless corpuscles of the blood. This is left an open question, no reference being made to them. This trifling deficiency is however atoned for by the richness and novelty of his views on other points regarding this fluid. Thus we are told that the clot, when deprived of its colouring matter, is "homogeneous, but void of traces of organic structure, and consists chiefly of albumen or fibrin, or a substance partaking of several of the characters of both." Again, "Modern chemistry has shown that [the colouring matter of the blood] is a particular substance insoluble in water, but susceptible of suspension in it to a certain extent, and consists of animal matter combined with peroxide of iron. It is distinguished by the name of *zoohematine.* Deprived of this, the globules are estimated by Bauer at $\frac{1}{2000}$ of an inch in diameter."

We will not weary our readers with any more of Dr. Craigie's observations on the blood, nor will we insult their understanding by an exposition of his most deficient knowledge of this important fluid.

Of the urine, we read that, besides containing urea, "it contains also a little uric acid, which is probably produced from the urea, as it can scarcely be said to be a constituent of healthy urine." Of the presence of hippuric or the absence of lactic acid we read nothing.

The bile is disposed of in rather less than two lines.

The morbid states of the fluids are treated in a similar manner.

The morbid conditions of the blood occupy three pages, which are devated to the consideration of that fluid in plethoric, anæmic, and inflammatory diseases, and to a notice of oil, urea, and pus in the blood. Regarding urea, we are told that "it may be detached from the serum by treating the latter with nitric acid, when crystals of nitrate of urea are found." We have a painfully strong conviction that they would not be ry easy of detection by the simple process with which Dr. Craigie has ished us.

The morbid states of the urine are, in several parts, not correctly described; nor is the first statement at all correct, that the most usual of them is excess of urea. No reference is made to the fibrinous casts observed in Bright's disease, or to the presence of epithelial scales which are almost characteristic of certain affections. No test, except that of fermentation, is given for sugar. So much for the fluids, which occupy in all just *twelve pages.*

Of the two succeeding chapters on *Filamentous or cellular tissue,* and on *Adipose tissue,* we can only say that our author has left these subjects in much the same state in which they existed in 1828, when his first edition appeared. There is no reference to the labours of Todd and Bowman on areolar tissue (Physiological Anatomy and Physiology of Man, vol. i, p. 73), or, in fact, to any one recent author. We learn that the *cellular* serosity "is understood to be derived from the minute colourless capillaries, named *exhalants;*" and that "in the cellular tissue of those parts which are free from fat, as in the eyelids, the prepuce, the *nymphæ* and *labia,* and the scrotum, it is said to be more abundant than in others. The peculiar structure of those parts which is cellular, may render any excess of serous fluid more conspicuous; for it is matter of observation that in many persons, otherwise healthy, these parts are not unfrequently distended with serous fluid." Not deeming it requisite to offer any remarks on the accuracy of the opinion expressed in the last sentence, we proceed to the consideration of the chemistry of this fluid.

> "This fluid has been generally said to be of an albuminous nature; and if it be identical with the serum of the blood, from which it is believed to be secreted, this character is not unjustly given it. Bichat, who maintained this opinion, injected alcohol into the filamentous tissue of an animal previously rendered emphysematous, and found in various parts *whitish flocculi,* which he regarded as coagulated albumen. He also obtained the same result by immersing a portion of the scrotum in weak nitric acid; and when a considerable quantity of this tissue was boiled it furnished much whitish foam, which Bichat regarded as albuminous. These experiments, however, are liable to this objection, that the effects in question may have arisen from coagulation of part of the filamentous tissue itself, which contains a considerable proportion of albuminous matter. The best mode of determining the point is to obtain the fluid apart, and to try the effect of the usual tests on it when isolated from the tissue in which it is lodged." pp. 32.)

Now, we ask our readers,—is it right, is it justifiable, or even decent, to publish such utter absurdities in the present day? Scrotum soup, cooked by Bichat and admired by Craigie, applied as a test for albumen! In his first edition the sentence runs precisely the same. He then hinted, even as he does now, at "the best mode of determining the point," &c.; but the twenty years that have since elapsed have left the question in his mind as undecided as ever. A reference to almost any work on animal chemistry, published during the last ten years, would have afforded an effectual relief to these chronic doubts.

The remarks on adipose tissue and on fat generally, abound in important errors and omissions. There is not the slightest allusion to margarine, which, mixed with oleine (or, as it is spelt by Craigie and others, elaine), forms the great bulk of human fat, nor to the fatty acids, which, united with soda, exist in the blood. There is no reference to the labours of Varrentrapp, Bromeis, Redtenbacher, or Gottlieb, in this department of animal chemistry; Chevreul is his latest authority. We are often puzzled to ascertain the process by which our author arrives at his conclusions: thus, in page 53 we read, that Bichat "contends that no fat can be recognised in the arterial blood, and adduces the fact that none can be distinguished in blood drawn from the temporal artery. It may be doubted, nevertheless, whether adequate means to ascertain this point were adopted." We regard Dr. Craigie's doubt on this point as a very rational one, and are therefore somewhat astonished at finding the following statement in the next page: "that fat does not exist in arterial blood in health, or is in very minute quantity, may be therefore admitted as an established point." From the specimens we have recently quoted, in connection with Bichat's mode of conducting a chemical analysis, we are not in the least surprised to find that *he* discovered no fat. Simon and other chemists have been more fortunate.

We will leave our author in the undisturbed possession of the doctrine, that "the arteries of the adipose membrane secrete fat," and proceed to notice the sections of these two chapters devoted to morbid conditions of areolar (or filamentous) and adipose tissue. Here Dr. Craigie generally shows himself well acquainted with his subject; although on minor points —and, indeed, on some points of the highest importance—he exhibits his entire ignorance of the labours of modern pathologists.

The following is a very fair account of the course that inflammation pursues when it attacks areolar (Craigie's filamentous) tissue:

> "At first the vessels become distended with blood, which moves rather slowly, and is accompanied with a throbbing or beating sensation. This is attended with more or less swelling of the part, heat, and pain; and, if it be near the surface, with redness. In the second place, the distended and overloaded state of the vessels never continues long without giving rise to more or less change in the blood in the part. Serum is poured out into the cells, often sero-albuminous fluid; sometimes blood even is extravasated. The sero-albuminous fluid is separated into lymph and serum. The former gives rise to the hardness usually observed. Thirdly, if the process continue, the secretion of sero-albuminous fluid is followed by that of purulent matter; and sometimes the serum first effused appears to be afterwards converted into purulent matter.
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> "This purulent matter is usually contained within a body of lymph, more or less regular, and which forms a sort of boundary between it and the solid or uninflamed part of the tissue. If this boundary be complete, so as to surround and inclose the purulent matter, it is denominated a cyst. This may take place either in acute or chronic inflammation.
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> "After the matter has been deposited in the manner now described, it evinces a tendency to proceed towards the nearest surface. This may be either the skin or any of the mucous membranes. At first it may be seated at so great a depth, that it is impossible to recognise its presence. In a short time, however, it may be felt by the practised finger. In most cases, even when there is much hardness, it is generally possible to predicate the presence of purulent matter The tendency to advance to the surface is connected with a tendency in other parts of the purulent tumour to contract; and, as the former process advances, the latter keeps pace with it, so that, in general, when the tumour bursts or is opened, the extent of the hollow of the abscess has sensibly and considerably been diminished." (pp. 35-6.)

We do not, of course, intend to justify the use of certain terms adopted in the above extract; but, translated into the medical language of the present day, we maintain that it would form a good description of what

Dr. Craigie meant to describe. Thus, when we are told that "serum is poured into the cell; often sero-albuminous fluid," we must recollect that there are no true cells (except fat-cells) for the reception of the serum; and that "sero-albuminous" is an unmeaning word, as all serum is albuminous. For the term lymph, as used above, we should say plastic exudation. Moreover, we doubt whether serum (independently of fibrine) can be converted into purulent matter. With this key to the peculiarities of his style, the reader may consult this portion of the volume with advantage.

Inflammation of adipose tissue is treated of in greater detail than we recollect to have seen elsewhere. Dr. Craigie seems to have observed several cases, and has given an excellent account of the disease, to which he applies the term *pimelitis.* We can only find room for the following extract:

> "The most remarkable circumstance in the pathological history of *pimelitis* is the extreme rapidity with which it generally proceeds to sero-purulent infiltration and disjunctive destruction. In several cases this has been known to take place within thirty-six hours from the appearance of the first symptoms of uneasiness. In others it occurs in the course of about seventy-two hours; and in very few cases is this event protracted beyond the fourth day.
>
> "Very nearly at the same rate may be estimated the fatality of the disease. Whenever the inflammation is very extensive, and especially if it occur in middle-aged or elderly corpulent persons, death is very likely to ensue in the course of the third, or, at most, the fourth day. In young and robust persons, on the contrary, and in whom the adipose tissue is not much loaded, the disease is slower in progress and less frequently fatal in termination." (p. 66.)

The three following Chapters are devoted to the consideration of the arteries, veins, and capillaries. We will first notice their general, and then their pathological anatomy.

The account of the structure of blood-vessels is not brought down later than to the time of Mondini, who, if we are not mistaken, wrote some thirty years ago. There is a certain degree of moral courage, that almost demands our admiration, in finding any one writing on the structure of the blood-vessels, without referring to the investigations of Henle, which have been taught for the last six or seven years by every lecturer on general anatomy, or to the work of Rauschel and the article by Schwann, on this subject. This deficiency, however, affords a sufficient reason for proceeding forthwith to the pathological anatomy of the blood-vessels. Separate sections are devoted to the following affections of the arteries: 1, adhesive inflammation; 2, arteritis diffusa; 3, chronic inflammation; 4, ossification; 5, atheromatous deposition (without the slightest reference to its actual pathological nature, as shown by Gluge, Rokitansky, Gulliver, Vogel, and others); 6, steatomatous deposition (to which we must apply a similar remark); 7, aneurism; 8, cirsoid aneurism; 9, wounds; 10, aneurismal varix; 11, varicose aneurism; 12, obstruction; 13, obliteration; and, 14, arctation or obliteration of the aorta at a definite point.

As we are anxious to avoid any semblance of hypercriticism, we will merely remark that Dr. Craigie is sadly behindhand, even on the subject of the fourteenth section, on which he has specially written elsewhere.

"Of this species of obliteration," he observes, "I met with one case in my own sphere of observation, and I have collected from various other sources nine other cases; and since that time three or four more cases have been published; so that thirteen [or fourteen?] cases altogether have now been recorded and described." (p. 110.)

We do not profess to know the exact number of these cases at present on record; but this we do happen to know, that Rokitansky, in a note to page 589 of the third volume of his 'Pathological Anatomy,' published in 1842 (about a year after the appearance of Dr. Craigie's memoir), recorded sixteen cases, including those collected by our author. It only takes one year for the labours of Dr. Craigie to be known and appreciated by the greatest morbid anatomist of the age; six years have now elapsed, and the researches of Rokitansky are still unknown in Edinburgh.

The morbid changes occurring in the veins are considered under the heads of circumscribed or adhesive inflammation, spreading inflammation, varix, osseous deposition, and the formation of concretions. There is some good sound matter on the diseases of the veins, although the deficiencies and omissions are perhaps more wonderful than in some other parts of the volume. The following remarks on *phlebitis* are well worthy of consideration:

"I have seen the disease so often take place after application of the finger to the wound in the vein at the bend of the arm, in the common operation of venesection, that I cannot doubt that it is often produced in this manner. The perspiration on the finger acts like an irritant poison to the cut edges of the vein, and thereby causes inflammation. It was also a very common accident after injecting saline solutions during the epidemic cholera of 1832. In the veins of the womb, after parturition, it may follow the forcible revulsion of the placenta; or, the sinuses being left open and patent, air from the atmosphere, or from the decomposition of the blood in the uterus, may enter these canals, and irritate or inflame their coats. In this organ it is most common along the lateral regions of the womb.

"The circumstances under which *phlebitis* may take place, may be enumerated in the following order:

"1st, after venesection, especially when the finger is applied to the wound, so as to touch the divided edges of the vein; 2nd, after amputation, especially when there is much fingering, or when a ligature is put on a vein; 3d, after laceration of a vein, as in certain lacerated wounds; 4th, after any venous tube has been laid open by ulceration or erosion, as in cancer or ulceration of the womb; 5th, after laying open the uterine veins, as in child-bearing; 6th, after deligation of a vein, as in the operation for varix, the old operation for castration, in which all the vessels were tied in one mass, and after operation on the hemorrhoidal veins." (p. 125.)

Dr. Craigie states that he has found inflammatory products, as lymph and purulent matter, with clots of blood, within the sinuses of the brain, in the following circumstances:

1. In certain cases of inflammation of the internal ear, and of the petrous portion of the temporal bone, when inflammatory action had spread to the internal jugular vein, in the temporal fossa.

2. In certain cases of gangrenous inflammation of the lungs, when suppuration takes place in the brain: and when the agents of this process appear to be the venous canals of the lungs, opening into the gangrenous or suppurating portion.

3. In cases of hypertrophy of the spleen.

It is possible that this department of morbid anatomy might have been rendered more complete if our author had made himself acquainted with the researches of Arnott,[*] Dance,[†] Puchelt,[‡] and Stannius.[§]

Under the diseases of the capillaries, we find the first notice of the process of inflammation. It is true the term had been frequently used in the earlier part of the volume, but no explanation of its meaning had been vouchsafed. There is no reference to any experiments or microscopic observations on inflammation, subsequent to those of Kaltenbrunner (1826). It is needless to say, that such an article must be altogether worthless, in reference to our present knowledge of the inflammatory process. We are mistaken if much more recent views on inflammation have not been given during the last few years (at all events, long since the time of Kaltenbrunner) in the hall of the College of Physicians of Edinburgh. But, whether Dr. Craigie has to go forth in search of knowledge, or whether it is brought to his very doors, the result is unfortunately much the same; for we shall presently have occasion to show that he is, or appears to be, just as ignorant of the progress of microscopic research in Edinburgh, as in Vienna or Paris.

His views regarding the formation of pus are brought down to the times of Hunter and Home; and no description, either chemical or microscopical. of that fluid is attempted. In this probably he has acted judiciously. As there is something extraordinary in Dr. Craigie's ignorance of the process of ulceration, we feel we are performing an act of kindness, in informing him that the Professor of Anatomy in the University of the very town where he for years was an extra-academical teacher, has made the process of ulceration a special study, in his 'Anatomical and Pathological Observations.'

In order to bring the chapter on the erectile tissues down to the present date, a long description of the helicine arteries of the penis is inserted. Need we inform our readers that Müller's opinions on this point have been satisfactorily disproved by Valentin, Berres, and more recently by Erasmus Wilson? From the consideration of the penis we naturally proceed to that of the vagina. Dr. Craigie very correctly remarks, "that hemorrhage from the vagina, whether intentionally or accidentally inflicted, is always most profuse and copious, and difficult to be restrained. From this cause, various females in this country have died before adequate means to suppress the hemorrhage could be adopted. This is manifestly dependent on the plexiform arrangement of the multiplied venous vessels by which the vaginal mucous membrane is surrounded." (p. 177.)

On osteo-aneurism, we have nothing later than Scarpa. The articles on this subject by M. Nelaton,[||] and Mr. Stanley's[¶] paper 'On Pulsating Tumours of Bone,' are surely not unknown to our author.

The Ninth Chapter is devoted to the consideration of the *exhalants* in their healthy and morbid conditions. In common fairness to our author, we can hardly omit all notice of this portion of his work; for the exhalants appear to have been to him a subject of much care and inquiry; and, if we may draw an inference from the fact, that, in 1828, this chapter began with the questions—"Are there such vessels as the exhalants described by physiologists? Is their existence proved by observation or inspection? If not, what are the proofs from which their existence has been inferred?"—and that now, in 1848, the identical questions are repeated in the identical words, it would be, that Dr. Craigie's indefatigable inquiries, for a period of twenty years, have not been crowned with success. As we are not aware that others have been more fortunate, we can recommend this section to any of our readers who may be desirous of making themselves acquainted with what has been said and speculated regarding these imaginary vessels. It is assuredly the last chapter that will ever be written upon them.

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[*] Medico-Chirurgical Transactions, vol. xv. London, 1829. We must do Dr. Craigie the justice to state, that, since writing this portion of the work, he has met with, and availed himself of, the memoir. See p. 872.

[†] Archiv. Gén. de. Méd., 1828-9.

[‡] Das Venensystem, &c.

[§] Ueber die Krankhafte Verschliessung grösserer Venenstamme. 1839.

[||] Gaz. des Hôpitaux, May and June, 1846.

[¶] Medico-Chirurgical Trans., vol. xxviii.

The three following chapters are open to the same objections as the earlier ones; they are devoted to the consideration of the lymphatic system, the lymphatic glands, and the process of organization. We extract two brief sentences. Of tubercle, or, as our author terms it, tyroma, we read, that "it presents to the glass no arrangement of cells or vessels, but a confused mass of substance, with no mark or trace of organization." If, by the phrase "no arrangement of cells," Dr. Craigie means to imply, that no cell-formations are present in tubercle, and that it exhibits no regular microscopic elements, we fear there will in these days be a large majority against him.

The other sentence we have marked is the following. We quote it as an illustration of the confused views our author must hold on the formation and nature of tissues generally:

> "Many anatomists have imagined that each texture has a proper matter, or *parenchyma*, by which it was supposed to be particularly distinguished, and which was conceived to consist of minute inorganic solid atoms. Whether this opinion be well founded or not, it is perhaps of little moment to inquire. At present it is certain that it is not susceptible of demonstration." (p. 225.)

With this choice *morçeau*, we conclude our remarks on the first book of this ponderous volume.

The Nervous System in its healthy and diseased conditions occupies the Second Book. In his first edition, Dr. Craigie brought down the history of the microscopic investigation of the brain and nervous tissue to the time of Everard Home (1825); in his present edition he alludes to the labours of Ehrenberg, Treviranus, Müller, Valentin, Weber, and Remak. He might have noticed those of Todd and Bowman, and several recent German writers.

We shall merely give a single illustration of the backward condition of the information communicated to us by Dr. Craigie on this important subject.

In his first edition, he refers to the labours of Rostan and Lallemand, on softening of the brain: to these he now adds an allusion to those of "Bouillaud, Bright, Durand Fardel, and other authors." As this is a subject that has recently attracted considerable attention, let us see how far it has been brought up to the present state of our knowledge. Nothing beyond the bare mention of the names of Bouillaud, Bright, and Durand Fardel is to be found; there is nearly a page and a half of new matter (the accumulation of twenty years), without any reference to their labours. But theirs is a happy fate. There is not even the record of the names of

Andral, Bennett, Carswell, Cruveilhier, Eisenmann, Fuchs, Gluge, Rochoux, Rokitansky, Sims, and others who have contributed largely to this department of our knowledge. It is rather singular that Dr. Bennett's memoir should have been overlooked by our author, because it was published in an Edinburgh journal, to which we observe that Dr. Craigie himself is a not unfrequent contributor: had the oversight been committed by an English writer, it would have been much more excusable. If he had carefully studied these works, which, we maintain, any one writing on this subject, in a treatise on morbid anatomy, ought in bounden duty to have done, it is possible that his additions might have considerably exceeded a page and a half.

Gladly leaving the book on the Nervous System, in the hope of finding more attractive matter as we proceed, we are checked at the very outset of Book III; even at its title.

It is a book on the *stereomorphic* or *kinetic textures*. We recollect hearing Sir Charles Bell state that he had been compelled to reject a student who went up for his Latin examination, because when asked what *nobis* was, he replied, the genitive case of *nob*. Had he been asked what a *kinetic tissue* was, his fate would probably have been as certainly sealed. And we have no hesitation in expressing our conviction that many men of far higher literary attainments than the aforesaid student would not at first recognise the meaning of the term. Under this objectionable name there are included "muscle, sinew or tendon, white fibrous system, yellow fibrous system, bone, cartilage, and fibro-cartilage." Here, at all events, we thought we should find abundance of new matter, for all these substances have been re-examined by competent observers during the last few years.

Although, from our past experience, we were not too sanguine in our expectations, we confess to a certain amount of astonishment on finding Sir Everard Home's description of muscle quoted by our author, as that usually received at the present day. We hoped to have found an account of the researches of Bowman, Skey, Gulliver, Henle, Valentin, Bruns, Gerber, and other recent authors on this subject, noticed and systematized. But nothing of the sort met our eye. It is true that in page 397, we are told that "among microscopic observers it has been recently the practice to distinguish muscular fibres into three sorts," but Dr. Craigie's views of the meaning of the word "recently" are so very extensive, that it is impossible to hit the era referred to, especially as no name of a later date than that of Home is mentioned. Two pages are devoted to these recent investigations, but as few of our readers will wish to hear any more of their researches, after having heard that they "distinguish muscular fibres into three kinds," we shall pass them over without further comment, and we do this with the least regret, because we are quite unable to understand them. In a very early part of this article we ventured to differ from Dr. Craigie in reference to the utility of microscopic delineation of texture. The histology of muscle, as described by Todd and Bowman, by Sharpey, and by Carpenter, is perfectly intelligible; but clear as their descriptions are, few (we doubt if any) readers would clearly understand their meaning, if it were not for numerous illustrations. If Dr. Craigie, instead of revelling amongst the ancient volumes of Malpighi, De Graaf, Ruysch, Bonetus, and Morgagni, had devoted a small portion of his time to the study of any recent work on minute anatomy and physiology, even of some manuals we could name, he would have spared himself the disgrace of this exposure, and us the painful duty of inflicting it.

On the subject of bone we read that its minute anatomical structure "was again investigated by Deutsch in 1834, and by Müller and Meischer in 1836." Very good; but has nothing been done during the last fourteen years? Have Henle, Bruns, Schwann, Bowerbank, Tomes, Todd and Bowmann, and Quekett added nothing to our knowledge of osseous tissue?

Regarding the teeth, we are informed that "as a variety of bone [they] demand attention. Every tooth consists of two hard parts; one external, white, uniform, somewhat like ivory, the other internal, similar to the compact structure of bone." Without stopping to inquire why the teeth have only the above solitary claim on our attention, we will proceed to examine how far Dr. Craigie is supported by other writers in assigning only *two* parts to these organs. All the *three* substances which we, and probably every writer on anatomy except Dr. Craigie, regard as entering into the composition of teeth, were described by Malpighi under the names of *materia tartarea*, *substantia filamentosa*, and *substantia ossea*, corresponding respectively to our cementum, enamel, and dentine. The cementum was overlooked by subsequent observers, till Purkinje rediscovered it in 1835. Hence, while we congratulate Dr. Craigie on bringing down the history of the teeth later than the time of Malpighi, we must express our regret that he has not succeeded in getting within at least thirteen years of the present date. We had marked for extraction a very remarkable passage in page 441, on the enamel, which we are sure would never have been written if Dr. Craigie had availed himself of the students' manuals to which we have already referred; but in mercy we forbear, and proceed to the next page, where we read, that "the development and growth of the teeth is a process of much interest." We grant that it is so, and for that very reason it is entitled to be fully and *honestly* discussed. Two accounts of the process are given, one by Dr. Craigie, and the other by Bichat. Our readers shall have the benefit of the former:

> "At what time the first rudiments of teeth appear, seems not to be determined with accuracy. In the fœtus, between the seventh and eighth month, I can merely distinguish in the centre of the vascular membrane of the alveolar cavity a minute firm body like a seed. I have, however, seen the crowns of teeth formed in fœtuses, which I have reason to believe had not attained the seventh month." (p. 442.)

Now every first-year student knows, or, at least, is taught,—and we cannot help thinking that Dr. Craigie knows perfectly well,—that the one great authority on this subject is Professor Goodsir, a gentleman who teaches healthy anatomy in the very university where, for a short period, Dr. Craigie taught morbid anatomy. Mr. Goodsir's memoir 'On the Origin and Development of the Pulps and Sacs of the Human Teeth,' appeared in the 51st volume of the 'Edinburgh Medical and Surgical Journal,'—a journal that, we should think, could not be unknown to our author, since, as we have already remarked, many of his own articles have been published in it. We have strived to help him out of the dilemma of either being ignorant of the article (which he hardly could be), or of wilfully suppressing facts from unworthy motives, by presuming that he deemed it was not expedient to recognise the existence of that journal, or anything contained in it, after the exposure that the editor received some seven or eight years back from the hands of Alison, Clark of Cambridge, Conybeare, Herschell, Holland, Prichard, and Roget, in consequence, if we recollect rightly, of a notoriously unfair review. This plea, however, we regret to find, will not serve him; as articles of his own have appeared in it since that date; and on close examination we see that Mr. Goodsir's paper appeared a full year before the crisis referred to. These are the facts of the case, our readers may form their opinions thereon.

We regret to add, that in the Fourth Book, "On the membranous, inclosing, or investing tissues," we can substantiate a similar charge in reference to Mr. Goodsir's observations on the intestinal villi. (See pp. 552-5.)

In a former page we took occasion to remark that Dr. Craigie's views of the meaning of the term "recent" were somewhat extensive. We may give an illustration from his section on the skin, in which he tells us that the corion "presents, further, a number of minute conical eminences (papillæ), which, according to the recent observations of Gaultier and Dutrochet, are liberally supplied with blood." The *recent* observations of Gaultier were published in 1809 and 1811, and those of Dutrochet in 1819.

Are the labours of Breschet and Roussel de Vauzême, Flourens, Gurlt, Henle, and Krause unknown in Edinburgh?

As Dr. Craigie is not aware that serous membranes possess an epithelium, it would be a mere waste of time to allude to his views regarding them. We may, however, remark, that the section devoted to the morbid states of these membranes contains a considerable quantity of good matter, although few of the subjects are brought up to the present time. Regarding meningeal apoplexy (p. 732) we actually have a reference to Mr. Prescott Hewett's paper, published in 1845. An excellent memoir on this subject by Prus has, however, been overlooked.

The two remaining books are new, that is to say, they have no counterpart in the first edition. We regret to add, that they present few redeeming features. For all that we can see, they might have been almost entirely written several years ago. The first of these books is on the Glands, and the second on the Lungs and Heart.

After a tolerably complete sketch of the history of the anatomy of the glands to the year 1830, when Müller published his celebrated work, we are told that, since the date of his monograph, "various facts of considerable value, chiefly microscopical, have been added by subsequent observers." After a long and very unnecessary account of Müller's nine orders of glands, we arrive at a disquisition on the "Anatomical peculiarities of the sturgeon;" this is all very well in its way, but hardly in place in the present volume. In page 770, we find the first reference to Henle; all that we hear of him, however, is that he entertains strong objections to the use of the word *acini*.

It altogether exceeds our comprehension to understand why, when he seems acquainted with Henle's work, he should have made no better use of it than he has done. If he had read the description of the glands by the last-named author, he would have found much to induce him to modify the views he seems to entertain. Dr. Craigie appears quite unconscious that a gland can exist, which has not a tube opening constantly on a free surface.

From our previous experience of our author's habits (see pp. 513-4), we are not surprised at finding no reference to Mr. Goodsir's memoir "On the ultimate secreting structure and the laws of its function."

The individual glands are next considered, and we find no less than twenty-four pages on the structure of the kidney. This is exactly double the space devoted to the consideration of the fluids of the body, both in their healthy and morbid states. A strange disproportion surely. We are much mistaken if we have not previously seen these twenty-four pages in print somewhere, we think in a comparatively recent number of the 'Edinburgh Medical and Surgical Journal.' If this be the case, we ought to have been informed that this article on the kidneys is a reprint. The editor of that journal has, however, amply revenged himself by publishing in his last number (and like Dr. Craigie, without the slightest acknowledgment) our author's section on the lungs. We should call this piracy; we do not know what name is applied to it in the north. But even if Dr. Craigie and the journalist acquiesce in such a proceeding, they should recollect that they are guilty of an act of injustice towards their medical brethren, in inducing them to pay twice for actually the identical articles.

The diseases of the different glands are in most cases better described than their anatomy. Thus we find a very complete account of the diseases of the pancreas, extending over twenty-two pages. He treats separately of

1. Inflammation and its effects, adhesion and suppuration.
2. Simple induration.
3. Chronic induration.
4. Hypertrophy.
5. Softening.
6. Atrophy.
7. Concretions in the ducts or duct, and
8. Heterologous growths.

There are stronger indications of research in this section, than in any we have yet arrived at. We may remark that we do not regard Dr. Craigie as justified in placing fatty degeneration of the pancreas under the head of "Encephaloid disease." He will find that Engel* (to whom he frequently refers) expressly states that "the pancreas frequently undergoes a fatty degeneration." It is to be regretted that he has overlooked Dr. Claessen's† work, which is, undoubtedly, the most important that we yet possess on this subject.

The diseases of the liver and kidneys are very ably discussed. They might, however, have been condensed with advantage, if there had been fewer references to Tulpius (1652), Tyson (1678), Job A. Meek'rem (1682), Tolet (1693), and other gentlemen of the old school.

Our allotted space will not allow us to enter into the consideration of his last book, "On the Lungs and Heart." Although, in many respects, not brought up to to the present state of our knowledge, it is by no means

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* Anleitung zur Beurtheilung des Leichenbefundes, p. 375.

† Die Krankheiten der Bauchspeicheldruse, 1844.

PART SECOND.

Bibliographical Notices..

ART. I.—*Portraits of Diseases of the Skin.* By ERASMUS WILSON, F.R.S., &c. Fasciculus II. With Four Plates.—*London*, 1848. Folio, pp. 6.

THIS second part of Mr. Wilson's truly splendid work fully deserves a continuance of the high commendation, which we had the satisfaction of bestowing on the first. The subjects included in it are *Lupus exedens* in two stages, *Lichen agrius*, with *Melanopathia* and *Leucopathia.* A case of the first very unmanageable disease, which is classed by Mr. Wilson as a tuberculous affection of the derma, is recorded at length; and it may be interesting to our readers to be informed that he succeeded in effecting a temporary cure of it (the date is yet too recent to allow him to speak with confidence of its permanency, the disease having already returned once after a period of quiescence,) by the internal use of the solution of iodide of mercury and arsenic, and by the local application of chloride of zinc. The delineation of *lichen agrius*, which, like all these plates, is an actual portrait, is peculiarly instructive; as it shows the disease (commonly known by the name of Baker's or Washerwoman's, Grocer's or Bricklayer's *Itch*) in three distinct stages; which have been ranked by Willan and Bateman under as many different genera. In the dry and chronic state in which it is often seen in the two former of these classes, it is described by those authors under the designation of *psoriasis diffusa;* in the active stage, when pouring forth an abundance of ichorous fluid, and then supposed to be more characteristic of the two latter, it is classified as *eczema impetiginodes;* and when from increase of irritation the discharge assumes a purulent appearance, or small pustules are developed around the circumference of the inflamed skin, the eruption is translated into the genus *impetigo.* Under the titles *Melanopathia* and *Leucopathia*, Mr. Wilson describes the very curious disorder of the chromatogenous function, which manifests itself in the augmentation or diminution of the natural pigment. Many cases of the former disorder have been recorded, and the term *nigrities* has been applied to them; whilst cases of the latter have also been noticed under the designations of *partial albinism*, *ephelis alba*, or *achroma.* The case described and portrayed is a very remarkable one, as combining *both* these conditions. The hue of the skin generally has been gradually becoming darker for some years; but there are light patches on the face and body, in which there is a total absence of pigment; whilst, as if to compensate for these, there are peculiarly dark patches elsewhere. Thus one of the nipples is destitute of pigment and surrounded by a white areola, whilst the other with its areola is almost black. No other disorder of the skin, or derangement of the general health could be detected in this instance. As the existence of the colourless patches on the face produced a disagreeable disfigurement, Mr. Wilson endeavoured to re-excite the chromatogenous function by pencilling the bleached surface with the acetum cantharidis; this application, however, was followed by the curious effect of producing an undue development of pigmentary matter, so that the parts that were previously uncoloured assumed the hue of those more deeply tinged than the ordinary skin.

We trust that this admirable work may meet with the encouragement it so well deserves; and that its author may not be prevented from carrying it on with the spirit he has shown in its commencement, by the want of those solid marks of appreciation, without which he cannot be expected to proceed far, in so expensive an undertaking.

Art. II.—*On Poisons in relation to Medical Jurisprudence and Medicine.* By Alfred S. Taylor, f.r.s., Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital.—*London,* 1848, Fcap. 8vo, pp. 856.

It had been our intention to have given a fuller notice of this work in our present Number; but a careful examination of its contents has led us to postpone our review of it until our next volume; since we deem the subject one of such great importance, in its relation to the theory and practice of medicine, as well as on its more obvious bearing on juridical investigations, as to require a more full and complete discussion than our disposable space would have permitted. In the mean time, however, we can most unreservedly recommend Mr. Taylor's treatise to our readers as the most complete, and at the same time, condensed system of Toxicology extant; and as the one which, we feel assured, will henceforth rank in this country as the chief authority on the subject. Although published in the size and form of the Manuals, on which certain writers have thought proper to pass a wholesale condemnation, this treatise contains a quantity of matter equal to that of one thousand pages of ordinary type in the large octavo size; and is quite entitled therefore to take rank with the systematic works of Christison, Orfila, and other toxicologists. Its information is in almost every respect brought down to the latest date; the principal deficiency which we have noted in it being in regard to Mr. Blake's valuable investigations on the "Action of Poisons," carried on at the suggestion of the British Association, and published in successive volumes of their Reports. We shall fill up this deficiency in our Review of Mr. Taylor's work.

Art. III.—*On Indigestion; its Pathology; and Treatment by the Local Application of uniform and continuous Heat and Moisture. With an Account of an improved Mode of applying Heat or Cold in Irritative and Inflammatory Diseases.* By James Arnott, m.d., Physician to the Brighton Dispensary.—*London,* 1847. 8vo, pp. 107.

We have long been of opinion, in common we doubt not with many of our readers, that too little use has been made of Heat and Cold as therapeutic agents. The most superficial glance, if it be sufficiently extended, over the phenomena of Nature, shows that there are no means in more constant and efficient operation for accelerating, retarding, or modifying the ordinary vital operations of various kinds, than changes of temperature. It is scarcely to be supposed that agents so powerful as regards the healthy or normal changes in the organism, should be otherwise than potent remedies for diseased states of the system; of which a large proportion essentially consist in a simple disturbance of the proper balance between the several processes, and are curable by a due regulation of them; whilst in many other cases (as when a *materies morbi* is introduced into the system) our means of cure chiefly consist in stimulating some of these processes to an unusual temporary activity for the purpose of eliminating the noxious matter from the economy.

The value of regulation of temperature has been fully acknowledged by all philosophical writers on medical and surgical therapeutics; unequivocal proofs being derivable from those disorders of the nutrition and circulation, commonly grouped together under the head of inflammatory, which affect the surface of the body, or parts immediately subjacent. But even in these cases, the full influence of the remedy in question has been seldom or never obtained, for want of an apparatus that shall keep up with efficiency and certainty the steady application of the desired temperature, with or without pressure, moisture, &c. The nearest approach to complete success in the application of cold for the repression of inflammatory action, has been where the system of *irrigation* has been employed; this, however, is attended with so many inconveniences and discomforts, that it has been for the most part only in severe cases, e. g. in wounds of the joints, that it has been usually employed.

This being the state of the case hitherto, we are extremely glad that Dr. James Arnott, to whom we are indebted for the suggestion of many improved modes of practice in which physical agents are available, has turned his attention to the subject, and has devised a simple apparatus by which the *dose* of temperature may be exactly regulated, and its efficient application readily secured for such a period as may be desired. This apparatus consists of a bag of thin Mackintosh cloth, or a large bladder, furnished with an entrance and exit-pipe of vulcanized India rubber; the entrance-pipe is connected with a reservoir containing water heated or cooled to the desired temperature; whilst the exit-pipe discharges the liquid that has passed through the bag, at any rate that may be required. By thus maintaining a slow but continued current through the bag, a uniform temperature of its contents may be secured; and by very simple contrivances, for an account of which we must refer to Dr. Arnott's treatise, this regulation of temperature may be combined with pressure and moisture to any degree that may be thought advisable. We can see no objection whatever to this apparatus, save its want of portability; and this is a difficulty which can scarcely be overcome, since the reservoir, which is its most bulky portion, cannot be dispensed with. The application of the bag to the body itself, involves no inconvenience or discomfort whatever; thanks to the extreme flexibility and elasticity of the vulcanized India-rubber tubes, which allow the freest movement in every direction.

We earnestly hope, therefore, that Dr. Arnott's valuable suggestion may be generally acted on; and that the profession at large will give a fair trial to heat and cold as therapeutic agents. It is one important purpose of Dr. Arnott's treatise, to show that their employment need not be limited to inflammatory or irritative disorders which lie at or near the surface; but that, by the method he suggests, their influence may be brought to bear upon the organs contained within the cavities of the body; and he selects dyspepsia as a disease which is particularly amenable to this kind of treatment. We suspect that his therapeutics may be better than his pathology; for we find him asserting that "vascular excitement or phlegmonoid irritation of the inner coat of the stomach, is one of the principal morbid conditions present in dyspepsia,"—a position in which we do not think that he will find a very general concurrence. We are much more disposed to believe in his assurance of the beneficial results of his method of treating indigestion, than his interpretation of the *modus operandi* of his remedy; and we trust that it will not be long before the extended experience of the profession at large shall have tested the value of this curative agency, which comes before them with so many recommendations in its favour. For the details of the methods which Dr. Arnott's experience has led him to adopt, and for many valuable suggestions as to the more extended application of the treatment, we must refer to the essay itself.

Art. IV.—*Deafness practically Illustrated; being an Exposition of Original Views as to the Causes and Treatment of Diseases of the Ear.* By James Yearsley, Surgeon to the Metropolitan Institution for Diseases of the Ear.—*London*, 1847. Crown 8vo., pp. 182.

We have failed to discover in what the originality of Mr. Yearsley's views consists, unless it be in their exclusiveness. "In the following pages," he says (Preface, p. vi), "I have endeavoured to show that almost all diseases of the ear originate in a morbid condition of the mucous membrane of the throat, nose, and ear, which becomes affected from a variety of causes, among which cold, the exanthemata, and stomach derangement stand pre-eminent." If, for "almost all," we read "a large proportion," we shall have nothing else than the opinion of all aurists of experience and intelligence. That the character of Mr. Yearsley's mind renders it peculiarly liable to be impressed by a dominant idea, must be apparent, we think, from his extraordinary predilection for tonsil-cutting. In the present treatise he informs us (p. 129) that he has "now removed upwards of *two thousand* morbid growths from the throats of more than *fourteen hundred* patients;" a vast variety of ailments being of course cured thereby,—among these, "though last not least, the imperfect development of health and strength in youth."

Although we can by no means coincide with Mr. Yearsley in his estimate of the value of his own improvements in diagnosis and treatment, yet we consider his treatise as well deserving the attention of those engaged in aural surgery; since it certainly affords additional materials for an improved acquaintance with an obscure class of disease.

ART. V.—*The Natural History of the Human Species, its Typical Forms, Primæval Distribution, Filiations, and Migrations.* By Lieut. Col. CHARLES HAMILTON SMITH, K.H. K.W. F.R.S. F.L.S. Illustrated by Thirty-four Coloured Plates, with Portrait and Vignette.—*Edinburgh*, 1848. 12mo, pp. 464.

THE high reputation of Colonel H. Smith as a zoologist, and our knowledge of the extensive opportunities which he has enjoyed of collecting original information, made us earnestly desire the publication of his long-promised treatise on the Natural History of Man; nor have we been disappointed, so far as regards the interest of the book, although we must express our doubts whether its character is well adapted for its purpose. It was intended to form a portion of the 'Naturalist's Library;' and in size, form, and embellishment ranges with that popular series. Unlike the other works which it includes, however, this treatise is mainly devoted to an exposition of Colonel Smith's theoretical views respecting the relationship of the different branches of the human family; and these views differ so widely from those which have lately been making progress in this country and elsewhere, chiefly in consequence of Dr. Prichard's elaborate Researches, that we cannot fairly estimate their value, without a much fuller body of evidence in their favour than the limits of this treatise afford. Colonel Smith takes but little account of the influence of climate, manners, food, &c., in modifying the configuration and complexion of man; nor does he seem to attach much importance to linguistic relations as indicative of affinity. But he takes his stand upon physical resemblances and differences; and regards all the varieties now existing as the result of the intermixture, in various degrees and combinations, of three primary types,—the Caucasian, Negro, and Mongolian. In order to account for this admixture, it is necessary to suppose a much more extensive original diffusion of all these types, than has usually been admitted; and a very ingenious and elaborate account is given of the migrations of the several tribes, the evidence of which appears to us to be very slender. Colonel Smith, moreover, dates back the origin of the human race to a period much more remote than geologists are usually disposed to admit; namely, to the latter part of that period when our own country and the adjoining portions of Europe were peopled by numerous gigantic pachyderms now extinct, since which time the earth has been the scene of vast changes that have modified its surface in a very considerable degree.

The volume contains a large collection of very interesting facts, many of them the result of personal observation during the author's residence in different parts of the globe; and it may therefore be read with great profit even by the advanced student of ethnology. But these facts are so mingled with speculations, and pure hypotheses are so frequently enunciated as verities, that it will be difficult for the general reader to discriminate between them; and we must recommend that the treatise should be read with caution, as an *ex parte* statement of the question. It is a great merit of Dr. Prichard's treatises, on the other hand, that they present a candid statement of the facts of ethnography, in a form quite distinct from the conclusions founded upon them; so that the reader may take the latter without the former. We must not omit to mention that the numerous illustrations, chiefly original, add very considerably to the value of the book; and if their execution be not first-rate, it must be admitted to be very fair, considering its price.

Art. VI.—*Chemical Technology; or Chemistry applied to the Arts and Manufactures.* By Dr. F. Knapp, Professor at the University of Giessen. Edited, with numerous Notes and Additions, by Dr. Edmund Ronalds, Lecturer on Chemistry at the Middlesex Hospital, and Dr. Thomas Richardson, of Newcastle-on-Tyne. Vol. I. Illustrated with 212 Wood Engravings.—*London*, 1848. 8vo, pp. 568.

The volume before us belongs to the series of illustrated "Standard Scientific Works," of which we noticed the first specimen in our previous Number. We are glad to be able to pronounce a less qualified verdict of approbation upon this work, than we could give in regard to the translation of Müller's Physics. The original treatise is one of great value; it has been carefully translated by gentlemen themselves well versed in the processes it describes, and consequently familiar with their technical language; and it has received from them numerous important additions, in which are described the most recent improvements in the various chemical arts as practised in this country. The work consists of a series of monographs upon the various branches of chemical industry; which are collected into groups according to the nature of the materials employed, and of the products which are to be obtained from them.

The first subject treated of, as being of importance to every manufacturer, is the most efficient means of employing fuel in the generation of *heat;* and under this head some very useful additions have been made by the Editors in an Appendix, regarding the ventilation and heating of buildings both public and private. The chemical process concerned in the production of heat, being identical with that practised for obtaining artificial *light*, the latter naturally claims the second place in the arrangement; and here again we have a large amount of additional matter supplied by the Editors, relating to recent improvements in the manufacture and use of coal-gas, and to other methods of illumination lately introduced. These additions are copiously illustrated by elaborate wood-engravings, executed in the same style with those of Professor Knapp's text, which are peculiarly clear and effective.

The second group comprises the production of the *alkalies* and *earths*, the agents concerned in their manufacture, and their various applications in the arts. Somewhat incongruously, however, as it seems to us, we find described under this head the processes employed in the manufacture of *acids*, which, as it seems to us, are well worthy of constituting a separate group. In a work of this kind, however, the details of arrangement are of little consequence; the whole class of subjects being of so artificial a character, that no strictly natural method of grouping could be carried out with advantage; and a good index at once supplies the means of turning to the information sought for with reference to any particular topic. The portion of the second group treated of in this volume, includes the manufacture or production of sulphuric acid, common salt, soda, muriatic acid, chloride of lime, potash, borax, saltpetre, gunpowder, nitric acid, and soap. These subjects are so well and fully treated of by Dr. Knapp, that the editors have not thought it necessary to introduce any considerable additions under these heads.

This work, we feel sure, will be welcomed by all who are practically concerned in the operations of which it treats; as likely to give effectual aid in that process of substitution of science for empiricism, which is rapidly taking place as a consequence of the increased and increasing intelligence of our manufacturers, and of the freer competition to which they are now subjected. Nothing is more adverse to improvement than monopoly; witness the eagerness with which the agriculturists of Britain are now availing themselves of valuable suggestions, which were but a few years ago sneered down by all but a few of the more far-seeing—because more enlightened—of their number, as the worthless speculations of the mere theorist, not to be put into comparison with ancestral wisdom and with the universal experience of the practical cultivator, from which, it was supposed, there could be no appeal. At the present time, we find the copperworks of Swansea, which have pretty nearly a monopoly in this department of British metallurgy, *wasting* an amount of sulphur, which would suffice for the manufacture of all the alkali produced in the kingdom, with the additional effect of absolutely *destroying* the vegetation of the neighbourhood. And the annual loss of coal, in the smelting of iron alone,—owing to the careless manner in which it is employed, the great plenty of the material, and the command which the British iron-master still has of the market,—is stated by the Editors at not less than *three millions* of tons.

> "We have no *Gewerbe Schule*, no *Ecole des Mines*, no *Ecole Polytechnique*; and it is no reply to assert that without such institutions, this nation has attained its position. We have had the race entirely to ourselves, to within a very recent date; and if the means of acquiring scientific information were as easy with us, as it is amongst our competitors in the application of chemical facts abroad, we should in all probability no longer have to complain of Glasgow goods being dyed at Barmen, of the importation of nearly all the ultramarine used here, and other similar indications of our inefficient knowledge or manipulation." (Preface, p. vii.)

The influence of government restrictions, also, imposed for the purposes of revenue, has had a most cramping effect upon various branches of manufacturing industry. This is particularly exemplified by the nearly stationary condition of the glass manufacture, so long as it was fettered by excise-regulations, and the rapid progress which it has made since these have been removed. The soap manufacture, from the same cause, has been for some time in a nearly stationary condition. When the Government finds itself in a position to lighten the burthen of taxation, we hope, for the sake of "the great unwashed," that this will be among the first of the imposts removed; the window-tax alone possessing, in our apprehension, a prior claim. The Editors justly remark, that "the interests of the whole country are sacrificed, when the excise-laws interfere with the improvements of processes, and put a bar, as they do in so many cases, to the development or progress of our manufactures."

Intimately connected, as our profession is, with one department of the practical applications of chemical science, we cannot but think that many of our readers will be glad to avail themselves of the opportunity presented to them by this treatise, of improving their acquaintance with others.

ART. VII.—*Report by the Committee of the Royal College of Physicians (Edinburgh), appointed to consider any Bills that may be brought into Parliament for the Improvement of the Health of Towns, and the applicability of such measures to Scotland.—Edinburgh*, 1848. 8vo, pp. 12.

THE Committee in question consisted of Professors Alison, Christison, and Gregory, with Drs. Stark and Spittal; any opinion emanating from it, therefore, is entitled to great respect; and we give the following extract in full, because we deem the views contained in it to be especially important at the present time, and to have all the weight which accurate observation, long and ample experience, and sound philosophy can give them.

> "The Committee think it necessary to add, that while they fully concur in the importance of the measures in question, they do by no means indulge in any such sanguine anticipations, as to their immediate consequences, as are contained in the Report of the Committee of the Health of Towns Association in London, and in other recent publications; and they consider this a point of great importance, because, if the public are led to form such expectations of the result of these measures as cannot be realised in practice, the necessary consequences must be, *first*, that other measures, which may be equally necessary for the health and comfort of the inhabitants of towns may be neglected; and *afterwards*, that a little experience of the state of the public health, after these measures shall have come into operation, may cause a general and hurtful feeling of distrust, as to the principles on which they are founded. Thus there is a great risk of the real efficacy of such measures being first over-estimated, and afterwards undervalued—in both cases to the public disadvantage.
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> "In particular, the Committee cannot concur in the statement (at § 10 of the Report above quoted), that 'science might secure to the whole population of this country such sanitary measures (involving protection from disease, suffering, and premature death), as are actually extended to the inmates of well-regulated prisons;' where, it is to be remembered, the diet, clothing, and whole mode of life, and intercourse with the rest of the community, are completely under the power of the authorities regulating those institutions.
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> "The Members of this Committee have witnessed too many and too extensive epidemics of fever, and are too well aware of the varying circumstances under which these may be diffused, to entertain any expectation, that, by all the sanitary measures here proposed, such epidemics can be prevented from recurring occasionally; or their symptoms and mortality from varying, as they often do, in all ranks of society, or their extension among the poorest people—who will inevitably crowd together, particularly in cold weather, in rooms, the atmosphere of which will necessarily become foul and offensive—be controlled as in a barrack or prison. They very much doubt whether the 'slaughter of the living by the dead;' i. e., the extension of epidemic disease by the retention of dead bodies in inhabited houses, has been a material cause of the propagation of fever in the great towns of Scotland, which are known to have suffered much more from this cause than those in England. And they cannot concur in the general and unqualified assertion, that, 'wherever animal and vegetable matters are in a state of decomposition, a poison is generated,' capable of producing fever or other epidemic disease. If this last assertion had been correct, there are many parts of this town in which such diseases would constantly prevail; or, at least, always be apt to affect strangers taking up their abode there; whereas it consists with the knowledge of this Committee, that there are no parts of the town constantly liable to such diseases; that although these spread most rapidly and most extensively in the filthiest and worst-aired parts, yet, even in these, they prevail only occasionally; and that when they do prevail, their origin may generally be traced to *importation* from places previously affected, and their extension to the effluvia arising from the

living subjects, applied, directly or indirectly, to those who become successively affected.

"All that these sanitary measures can effect will not purify the air of the interior of the crowded rooms, inhabited by the dissipated and improvident poor, or by the destitute poor; or do more than diminish the variety of disease and suffering, which may be ascribed, in part, to the impurity of that air. And although much may be done by religious and moral instruction,—by an enlightened and general system of *improved secular education*, especially as addressed to those who stand most in need of sanitary improvement,—and by the well-regulated relief of destitution, to correct the evils which spring from dissipation or from indigence; yet it is only necessary to advert to the great mass of suffering, permanently included under the name of the Irish Poor, in every large town in this country,—to the effects of stagnation of trade in any of the manufacturing districts,—or to the faulty construction of large portions of such towns as this, which are private property, and cannot be rapidly altered,—in order to perceive that any such measures, for a long time to come, can only be partially successful.

"But if the public expectation, as to the effect of these measures, be not raised above a reasonable height,—if it be only affirmed that the health of all towns may be gradually but materially improved; the extension of epidemics in them be restrained; the probability of life, even in large and ill-situated towns, be very considerably increased; and the comfort and happiness of all classes of the inhabitants be materially promoted:—The Committee have the greatest pleasure in saying, that they think all these benefits may be confidently anticipated from these measures. W. P. Alison, *Convener*."

Art. VIII.—*An Address delivered in the Theatre of the Meath Hospital, at the opening of the Session* 1847-8. By William Stokes, m.d., Regius Professor of Physic in the University of Dublin, &c. pp. 32.

We have read this address with very great pleasure, not more for the sound practical wisdom it contains, than for the warm-hearted patriotic spirit that it breathes. After alluding to the circumstances which have tended during the last twenty-five years, to raise the character of the Dublin School of Medicine so high, and having passed a well-merited eulogium on his Colleague, Dr. Graves, as the chief agent in this change, Dr. Stokes delivers some admirable remarks on fever, that want of space alone prevents us from transferring to our pages. He professes himself a follower of "the old maxims (expanded and improved) of Hippocratic medicine," an essentialist in doctrine, a symptom-observer in practice. The subject of fever naturally leads him to notice its dreadful prevalence in Ireland of late, and the sad and melancholy results that have followed everywhere, more especially in the ranks of the medical profession. No men have ever shown a more heroic spirit of devotedness, or greater Christian philanthropy, under circumstances not less dangerous than distressing, than our brethren in Ireland have uniformly done in the discharge of their duties as ministers to the sick and suffering. Can we then wonder at the indignant tone of remonstrance which has been expressed by almost the entire body of the profession there on the subject of the remuneration—call it rather wages—recently offered to the medical officers of fever hospitals? No,—we cordially and entirely sympathise with them, and most heartily wish success to their cause. Let them but calmly and steadfastly persevere under the guidance of such men as Graves, Cusack, Stokes; and who will despair of the result? Our best wishes attend them.

Art. IX.—*Elements of Natural Philosophy; being an Experimental Introduction to the Study of the Physical Sciences.* By Golding Bird, A.M. M.D. F.R.S., Assistant Physician to Guy's Hospital, &c. &c. Third Edition, revised and enlarged.—*London*, 1848. Fcap. 8vo, pp. 552.

We rejoice to see, in the continued demand for this excellent Manual, notwithstanding the publication of other valuable works on the same subject, an evidence of the increasing attention which is being paid to the study of physical science, as a branch of general education. We know of no treatise which contains within so narrow a compass so large an amount of valuable information, so clearly and concisely expressed; and we cannot, therefore, too strongly recommend it to our readers as a work to be placed in the hands of every medical student at the commencement of his curriculum. We have looked into all those parts of this new edition, in which the progress of science has rendered modifications or additions desirable; and have found that Dr. G. Bird has left nothing undone to render his treatise a complete epitome of the principles and facts of physical science as at present known. We would suggest to him whether, in his next edition, the Introductory Discourse might not contain a little more of the philosophy of the subject; by being made to present, somewhat after the manner of Professor Grove, a general view of the correlations of the physical forces.

Art. X.—*Pocket Dispensatory and Therapeutical Remembrancer; comprising the entire lists of Materia Medica, Preparations, and Compounds, with a full and distinct version of every practical formula, in the London, Edinburgh, and Dublin Pharmacopœias; to which are subjoined copious Relative Tables, exemplifying approved forms under which compatible Medicines, &c. may be extemporaneously compounded, &c. &c. &c.* By John Mayne, M.D., L.R.C.S. Edin.—*London*, 1848. Fcap. 8vo, pp. 271.

The lengthy title of this little book is almost of itself a sufficient description of its plan and objects. The principal features in which it differs from other pocket-companions of the same kind, are, in the first place, the classification of the various articles according to their reputed action,—as Alteratives, Deobstruents, Antacids, Absorbents, &c.; and secondly the introduction, at the bottom of every page, of formulæ that may suggest the best modes of exhibition and combination of the substances to which they are annexed. Each of these novelties has its advantages and disadvantages. The classification will be found useful to some, embarrassing to others; and we are disposed to think that, on the whole, it may tend to the perpetuation of wrong ideas on the agencies of the several medicines thus grouped together, although we readily admit its convenience, as presenting at one view all the remedies whose operation is supposed to be of the same general nature. The formulæ, again, will be useful and instructive to some; whilst others will be apt to trust too much to them, and to think too little for themselves.

Art. XI.—*The Journal of Psychological Medicine, and Mental Pathology.* Edited by Forbes Winslow, m.d. No. I, January, 1848.—8vo, pp. 192.

There can be no doubt that the extension and improvement of rational knowledge in regard to the nature, causes, and treatment of mental alienation, as well as the advance of psychological science itself, are likely to be promoted by a well-conducted Journal devoted exclusively to these subjects; and we are consequently disposed to look with a favourable eye upon any attempt at the establishment of such a periodical, notwithstanding the mass of this kind of literature that already crowds our shelves. That it can be a remunerating speculation, in a pecuniary point of view, is not to be expected; since a body of readers sufficiently interested in the subject, to give to the work the requisite support, does not, we fear, at present exist in this country. Its maintenance must be a labour of love, therefore, to the parties concerned in sustaining it; and this constitutes an additional reason for extending to it a friendly welcome. If carried on in a right spirit, however, we have little doubt that it will in time succeed; as, in spite of the mechanical character of our age, there is a growing interest in psychological inquiry, and an increasing disposition to study the disordered conditions of the human mind in a scientific as well as in a practical point of view. We would recommend to the editor to endeavour to introduce as much variety, both of subject and of authorship, as the resources at his command may permit; and especially to invite the cooperation of all those whose lines of scientific inquiry are related to his own.—We may mention that the Journal will contain Analytical and Critical Reviews, Original Articles, and Abstracts of Reports of Lunatic Asylums, which last, if well digested, will form a collection of great value.

Art. XII. *The Wonders of Geology; or a Familiar Exposition of Geological Phenomena.* By Gideon A. Mantell, ll.d., f.r.s., Fellow of the Royal College of Surgeons of England, &c. In Two Volumes. Sixth Edition.—*London*, 1848. Fcap. 8vo, pp. 938. With Six Coloured plates, and 198 Wood Engravings.

We are glad to have the opportunity of bringing this new edition of a very excellent treatise under the notice of our readers. To those of them who are not acquainted with the lustre which has been cast on the body of which Dr. Mantell is a member, by his scientific labours, we can only apply the ancient proverb, that "a prophet has no honour in his own country." Amongst the comparative anatomists and geologists of every other country, as of our own, his name ranks as one of the "household words" of palæontology; his researches on the Geology of the South-east of England, including his restoration of the gigantic Iguanodon and other extinct reptiles of the Wealden district, having procured for him a reputation which will place him, in the estimation of future generations, amongst the fathers of modern geology.

We always rejoice to find men of first-rate attainments in science coming forward to instruct the public by means of lectures and elementary treatises; and it is, we conceive, a very low view of their mission, which would regard them as in any way degrading themselves by so doing. If *they* will not concern themselves in the elevation of the popular mind, it must be left to men of inferior capacity, who possess a smattering of all sorts of knowledge, and make a trade of communicating to the world the mixture of truth and error, the crude novelties, or the obsolete figments, with which their own minds are charged. The volumes before us had, as their original groundwork, a course of lectures delivered at Brighton, by Dr. Mantell, as part of an attempt (which unfortunately proved unsuccessful) to establish in that town a public museum, illustrative of the geology and palæontology of the South-east of England. The adaptation of this unpretending treatise to meet a popular want, is sufficiently proved by the rapid sale of five successive editions; and we rejoice to welcome a sixth, augmented by a large amount of valuable matter, and embodying a digest of all the most recent investigations in the different departments of geology and palæontology. The title of the work by no means indicates its real merits. Originally intended, perhaps, to excite public attention towards some of the more imposing and remarkable features of the science, it is scarcely applicable to a production that has been gradually expanded into the form of a complete systematic treatise, which, although elementary enough for the general reader, is sufficiently comprehensive in its range, as well as scientific in its character, to meet the wants of those who are disposed to rest satisfied with a good *general* acquaintance with this most interesting subject. The following recommendation of his favourite study will convey a good idea of Dr. Mantell's pleasing style:

> "Geology, beyond almost every other science, offers fields of research adapted to all capacities, and to every condition and circumstance in life, in which we may be placed. For while some of its phenomena require the highest intellectual powers, and the greatest attainments in abstract science, for their successful investigation, many of its problems may be solved by the most ordinary intellect, and facts replete with the deepest interest may be gleaned by the most casual observer.
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> "To the medical philosopher, geology presents peculiar attractions for those hours of leisure and relaxation, which are indispensable to maintain a healthy state of mind; for it requires the cultivation and application of chemistry, botany, comparative anatomy, zoology, and physiology,—sciences which form the very foundation of medical knowledge. It exerts, too, the most salutary influence, by calling forth the continual exercise of our intellectual powers; for the desire to explain what is obscure in the natural records of the past, induces a more accurate examination of existing physical phenomena, and of the organization and habits of the living beings within reach of actual observation. It enforces the necessity of weighing the conflicting evidence of apparently irreconcilable phenomena, of detecting differences, and seeking analogies, and of generalizing and combining an immense number of isolated facts. The mind thus acquires the power of acute observation, of patient investigation, and of salutary caution in drawing inferences and arriving at conclusions,—habits of the first importance in the discrimination and treatment of diseases.
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> "And however little, in the present state of the public mind, such qualities may be appreciated, the labour will bring an ample reward in the self-conviction that the talents intrusted to us have not been given in vain.
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> 'Better than fame is still the toil for fame—
> The constant training for the glorious strife.'
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> "For it should ever be borne in mind that the primary object of every study

ought to be an inward one—that of enlarging and elevating the intellect; and the direct aim of science should be the discovery of the principles of unity, order, and connexion, which are everywhere manifest in the universal life of nature." (Preface, p. xi.)

We rejoice to know that Dr. Mantell has a son, who seems likely to be a worthy follower in his father's steps; having recently transmitted from New Zealand a magnificent collection of the bones of those numerous extinct struthious birds, whose existence was originally predicated by Professor Owen from a single fragment of the shaft of a femur destitute of articular extremities. The collection formed by Mr. Walter Mantell includes the nearly complete cranium of the *Dinornis giganteus* (a bird of about ten feet in height), the bill of which strongly reminds one of the broad end of a stout pickaxe; also fragments of the egg-shells of the Dinornis; and numerous bones referable to distinct genera. The entire absence of any rudiments of wing-bones in this large collection is a most interesting verification of Professor Owen's original deduction from the peculiarity of the cancellated structure in the fragment just adverted to;—namely, that the Dinornis must have been even more destitute of anterior extremities than the ostrich; a deduction which, although looked upon by many as a clever guess, was in reality the result of a profound acquaintance with physiological laws, and of an implicit faith in the great principle of unity of design.

Art. XIII.—*The London and Provincial Medical Directory for* 1848.—*London.* 12mo, pp. 634.

This very useful and comprehensive work has undergone considerable changes since its last annual appearance; of which some, at least, are very decided improvements. The Provincial Directory has been augmented by a local list of the practitioners in each town and village, arranged under their respective counties, which will be, for a great variety of purposes, a most useful addition; and there is a similar list of the Poor Law Medical Staff, including the names of the officers, their counties, unions, and districts; so that, by reference to the name of any town or district of England or Wales, the names of all the practitioners resident in that locality, and of those acting under the authority of the Poor Law Commission, will at once be found. We would suggest that, in the next edition, the names of those who hold hospital or dispensary appointments, be indicated by an asterisk or some other distinctive mark. The London list is augmented by a diary, which embodies all the information of an almanac, with a variety of notices useful and interesting to the medical practitioner. We apprehend, however, that this will be but little used for the purpose of *entries*, for which it is specially designed,—a separate and more portable memorandum-book being usually found much more convenient; and we would therefore suggest to the editor the desirableness of compressing the diary to the dimensions of an almanac (retaining all the information which it contains, and even adding to this), and of again enlarging the list of references to the literary contributions of the several individuals enumerated, which list has always appeared to us so valuable and interesting a feature in the 'Directory,' that we much regret the curtailment of it. A Letts's Diary may be procured for a shilling or two by those who need it; but the information we allude to cannot be obtained elsewhere. We quite agree with the editor in the propriety of omitting the distinctive terms physician, surgeon, and general practitioner, leaving the nature of each individual's practice to be inferred from his qualification; and also in the inutility of inserting memberships of medical and other societies, which are held merely through the payment of a yearly subscription, and do not involve the possession of superior professional acquirements. We would suggest the addition of the names of the lecturers on the several departments of medical study in the different schools, both metropolitan and provincial; which, if arranged in a tabular form, would occupy even less space than is at present taken up, in many instances, by the account of a single school.

ART. XIV.—*On the Nature and Elements of the External World; or, Universal Immaterialism fully explained and newly demonstrated.—London*, 1847. 8vo, pp. 269.

THIS is a work, written by one who has evidently grasped his problem with great clearness, and who expounds it with considerable success. The misfortune attaching to the whole production is, that it comes more than half a century too late. At the time when Hume and Reid took up the question, the work before us would probably have caused them considerable inconvenience; at present, it can serve for nothing more than a piece of mental gymnastics. The author, though possessing some distant idea of the philosophy of Germany since Kant, has manifestly never thought himself clear upon it. Had this been the case, he would never have penned the work now under consideration. The terms Matter and Mind have long ceased, amongst the higher order of philosophical thinkers, to be the stalking-horses of logical disputation. The whole controversy is now removed far above the region of those very indefinite terms, into the higher region of *subject and object*. That it may be possible to maintain universal immaterialism, in the sense of asserting the dynamical theory of nature, it is not for us at present to deny; but to return to the standpoint of Berkeley, is to turn the stream of philosophical thinking backwards,—to descend to a level, above which we have long been elevated. At the same time, we are not sorry to see the work before us. It shows, at least, a growing tendency to philosophical thinking; and just as logical dilemmas, like those which our author has exhibited, have before led to a deeper research into the fundamental principles of human knowledge, so they may again serve to force the thinkers of *our* day to a fresh inquiry into the bases of their philosophical belief.

We should add that, in addition to a very lucid statement of the old arguments in favour of the Berkeleyian scheme, the author adduces two new ones: with the importance of which he is himself so strongly impressed, that he offers a prize of a hundred pounds to any one who shall confute them. The method of adjudication is rather complex, and too long to detail here; but we may mention that it is devised in such a manner, as to present peculiar advantages to the competitor; and those of our readers who have a turn for metaphysical speculation, may find many worse employments for their leisure hours, than the attempt to crack a philosophical nut with so valuable a kernel.

PART THIRD.

Periscope.

ANATOMY AND PHYSIOLOGY.

On the Structure and Development of the Liver. By C. HANDFIELD JONES, M.B.Cantab. Communicated by Sir BENJAMIN C. BRODIE, Bart., F.R.S. &c.

THE author gives a detailed description of the structure of the liver in animals belonging to various classes of the animal kingdom. He states that in the Bryozoon, a highly organized polype, it is clearly of the follicular type; and that in the Asterias, the function of the liver is probably shared between the closed appendage of the stomach and the terminal cæca of the large ramifying prolongations of the digestive sac contained in the several rays. Among the Annulosa, the earthworm presents an arrangement of the elements of the hepatic organ, corresponding in simplicity with the general configuration of the body, a single layer of large biliary cells being applied as a kind of coating over the greater part of the intestinal canal. In another member of the same class, the Leech, in which the digestive cavity is much less simple, and presents a number of sacculi on each side, these elements have a very different disposition; and the secreting cells, although some remain isolated, for the most part coalesce to form tubes, having a succession of dilatations and constrictions, and finally uniting and opening into the intestine. In Insects, the usual arrangement is that of long curved filamentary tubes, which wind about the intestine; these, in the meat-fly, are sacculated throughout the greater part of their course, till they arrive quite close to the pylorus, where they open; near their origin they appear to consist of separate vesicles, which become gradually fused together, but occasionally they are seen quite separate. The basement membrane of the tubes is strongly marked, and incloses a large quantity of granular matter of a yellowish tinge, with secreting cells; another portion of the liver consists of separate cells lying in a granular blastema, which cells, in a later stage of development, are seen to be included in vesicles or short tubes of homogeneous membrane, often coalescing and exhibiting a more or less manifestly plexiform arrangement. The author has termed this the *Parenchymatous portion* of the liver, on account of its general appearance and mode of development, though he has not been able to determine whether the tubes always originate from it. Among the Arachnida, the follicular type of arrangement prevails; and the same is the case with the Crustacea, the follicles in these last being distinctly visible to the naked eye. In Mollusca also, we find the follicular arrangement universally to obtain; yet in certain cases the limiting membrane of the follicles cannot be shown to exist, and the author therefore thinks that its importance is probably not great, but that it serves chiefly to fulfil the mechanical function which its synonym "*basement*" indicates. The quantity of retained secretion in the liver of molluscs seems clearly to imply that the bile in them is not an excrementitious fluid; it is used slowly on account of the imperfect character of the respiration.

In passing from the Invertebrata to the Vertebrate division of the animal kingdom, and beginning with the class of Fishes, a great change is immediately manifest in the form and character of the biliary organ; it is now a gland of solid texture, to which the term *parenchymal* is justly applied. Two portions may be distinguished in it, namely, the secreting parenchyma, consisting of delicate cells, or very often of nuclei, granular and elaborated matters in great part, and the excreting ducts, which, though completely obscured by the surrounding bulky parenchyma, may yet be satisfactorily demonstrated, and traced often to their terminal extremities in the following manner. If a branch of the hepatic duct be taken up in the forceps, it may be dissected out without much difficulty from the surrounding substance, which is very soft, and yields readily to gentle manipulation; when a trunk is in this way removed and placed under the microscope, a multitude of minute ramifications are seen adhering to it; among these not a few may be discovered, which do not appear to have suffered injury; some are occasionally seen terminating by distinctly closed extremities; more usually the duct becomes very minute, and gradually loses all definite structure, appearing at last like a mere tract of granular matter; in either case there is no communication by continuity with the surrounding parenchyma. Large yellow corpuscles, peculiar cells, and a considerable quantity of free oily matter usually existing in the liver of various fishes, seem generally to indicate a great superiority in the amount of secretory over that of excretory action, and to betoken clearly the feeble intensity of the aërating function.

In Reptiles, there is the same arrangement in the liver, namely, a secreting parenchyma of cells and an apparatus of excretory ducts, which have the same essential characters as those of fishes; but there exist very frequently in the parenchyma remarkable dark corpuscles, which appear to be masses of retained biliary matter, the import of which, in the situation they occupy, is doubtless the same as that of the similar masses existing in fishes.

In Birds, the parenchyma of the liver is remarkably free from oily or retained biliary matters; it often consists almost wholly of free nuclei and granular matter, with scarcely a single perfect cell; the excretory ducts often greatly resemble those of reptiles, sometimes rather those of mammalia; the essential character is, however, always the same, namely, that they terminate without forming any important connexion with the parenchyma.

In Mammalia, the parenchyma of the liver consists usually of perfect cells, which are arranged often in linear series of considerable length, radiating from the axis of each lobule; these unite at various points with each other, so as to present a more or less decidedly plexiform appearance. Each lobule, as described by Mr. Kiernan, is separated from the adjacent ones by the terminal twigs of the portal vein, and to a greater or less extent by a "fissure," though in most animals the lobules are continuous with each other both above and below the fissure. The elaboration of the secreted product seems to be most completely effected in the cells adjoining the margins of the lobules, which are often seen to contain a larger quantity of biliary matter than those in the interior, and to be apparently in the act of discharging it into the fissure; the margin of the lobule then presents an irregular surface with large globules of the secretion clustering together all over it. The capsule of Glisson surrounding the vessels in the portal canals, gives a fibrous investment to those surfaces of the lobules which are towards the canal; but when it has arrived in the fissures, it forms a continuous membrane lining the surfaces of opposite lobules; this membrane is often truly homogeneous, and closely resembles the basement tissue; there appears occasionally to be a delicate epithelium on its free surface; but this, as well as the membrane itself, is often absent when the margin of the lobules is in that condition which has just been described and which may be termed *active*. The minute branches of the hepatic duct, as they approach their termination, undergo a remarkable alteration in their structure; they lose their fibrous coat, which blends itself with the membranous expansions of the capsule of Glisson; their basement membrane becomes gradually indistinct, and at last ceases to exist, and the epithelial particles no longer retain their individuality, but appear to be reduced to mere nuclei, set very closely together in a faintly granular basis-substance. The mode of their termination is not uniformly the same;

frequently they present distinctly closed rounded extremities, between one and two thousandths of an inch in diameter; at other times they seem to cease gradually in the midst of fibrous tissue, the nuclei alone being disposed for some little way in such a manner as to convey the idea of a continuation of the duct. These ducts can seldom be discerned in the fissures, but have several times been seen in the "spaces," where several fissures unite; they do not form anything like a plexus between the lobules. From the anatomical relation of the ducts to the parenchyma, and from the circumstance that a distinct vessel conveying a different kind of blood is distributed to the hepatic duct, as soon as the liver assumes the parenchymal form, it seems probable that the mode in which the secreted bile is conveyed out of the organ, is by permeating the coats of the minute ducts in obedience to an endosmotic attraction, which takes place between the bile in which the ducts may be said to be bathed, and a denser (perhaps mucous) fluid formed in their interior. The large quantity of oily matter frequently existing in a free state in the secreting parenchyma of the liver, which must be regarded as a product of secretory action, seems to suggest the idea, that a certain quantity of the biliary secretion may be directly absorbed into the blood, and in this manner conveyed away from the organs, just as occurs in the thyroid body, suprarenal capsules, and other glands unprovided with efferent ducts.

With respect to the development of the liver, the author considers the opinion of Reichert to be decidedly the correct one, namely, that its formation commences by a cellular growth from the germinal membrane, independently of any protrusion of the intestinal canal. On the morning of the fifth day, the œsophagus and stomach are clearly discernible, the liver lying between the heart, which is in front, and the stomach, which is behind; it is manifestly a parenchymal mass, and its border is quite distinct and separate from the digestive canal; at this period the vitelline duct is wide, it does not open into the abdominal cavity, but its canal is continued into an anterior and posterior division, which are tubes of homogeneous membrane, filled, like the duct, with opaque oily contents; the anterior one runs forwards, and forms behind the liver a terminal expanded cavity, from which there passes one offset, which, gradually dilating, opens into the stomach; a second, which runs in a direction upwards and backwards, and forms apparently a cæcal prolongation; and a third and fourth, which are of smaller size, arise from the anterior part of the cavity and run to the liver, though they cannot be seen to ramify in its substance; at a somewhat later period, these offsets waste away, excepting the one which is continued into the stomach, and then the mass of the liver is completely free and unconnected with any part of the intestine. As the vitelline duct contracts, the anterior and posterior prolongations of it become fairly continuous and form a loop of intestine, the posterior division being evidently destined to form the cloaca and lower part of the canal. The final development of the hepatic duct takes place about the ninth day, by a growth proceeding from the liver itself, and consisting of exactly similar material; this growth extends towards the lower part of the loop of the duodenum, which is now distinct, and appears to blend with the coats of the intestine; around it, at its lower part, the structure of the pancreas is seen to be in process of formation. The further progress of development of the hepatic duct will, the author thinks, require to be carefully examined, but the details he has given in this paper have satisfied him of the correctness of the statement that the structure of the liver is essentially parenchymal. —*Proceedings of the Royal Society*, 1847.

We have given the full abstract of this paper, on account of the value we attach to it. We have long been convinced that the difficulties attending the determination of the true relation between the hepatic cells and the hepatic ducts in the liver of the higher vertebrata, can only be solved by carefully tracing the gradual alterations which this organ presents in its plan of structure, from the higher invertebrata, through the ascending series of vertebrata; and the results obtained by Mr. H. Jones fully confirm this view.

Researches illustrative of the Influence of the Movements of Respiration upon the Flow of the Blood in the Aortic System. By C. LUDWIG.

THE author employed instruments essentially the same as those of Poisseuille, by which he could accurately measure the amount of pressure exerted by the lung on the sac of the pleura, and also on the blood moving in the aorta; and by a simple contrivance, the varying results in the latter instance were registered by the instrument itself. These results are illustrated by a series of diagrams, in which, by means of curved lines alternately rising and sinking, the increased and diminished pressure on the blood are shown.

In the case of four horses on which the experiment was tried, it was found that in the quiet state of the respiration, or what may be called the normal condition, the respiratory movements had no influence upon the circulation of the blood in the aortic system; whilst in the dog it is different, owing, according to M. Ludwig, to the rapidity of the respiratory actions; the latter animal offering, in this respect, a great contrast with the horse, in which they are, it is well known, extremely slow.

In the state of excitement very different results were obtained; and in order to analyse these, the motion of expiration was distinguished from that of inspiration; the systole and diastole of the ventricle were also investigated separately.

Influence of expiration on the pressure of the blood. Expiration exerts an increased pressure upon the aorta; and an acceleration of the blood in the various arteries is thereby produced. If the expiration corresponds with the *systole* of the heart, a sensible elevation of the curved line, indicating an increased pressure on the arteries of the second order, ensues. When the expiratory movement is synchronous with the *diastole* of the ventricle, the fall in the arteries is either very slight, or no change takes place, or even the pressure rises. If the act of expiration extend over several pulsations of the heart, the same results may be detected; that is to say, there will be a rise, with small or scarcely perceptible pauses, so long as the expiration continues.

M. Ludwig further ascertained that the amount of pressure on the blood during expiration is proportionate to the force and the velocity of the expiratory action, as ascertained by the apparatus; that the sinking during the diastole disappears in proportion as the pulsations of the heart follow each other; and, lastly, that the expiratory movement changes essentially the velocity and the intensity of the cardiac pulsations. Thus, in the dog, the pulsations of the heart become more frequent during expiration than at other times; the force is also increased, the contraction of the heart being short but energetic. In the horse there is also a similar change, but by no means equal to that noticed in the dog; a difference not yet explained by these researches.

The author thus explains how the parietes of the chest may, during expiration, press on the blood-vessels. He conceives that the parietes act on the vessels in two ways: firstly, by means of the solid parts, especially the heart, in those animals in which that organ is placed against the thoracic walls; secondly, by the air contained within the lungs.

Influence of inspiration. In the second part of these inquiries, M. Ludwig states the results he obtained with respect to inspiration. These are, generally, exactly the reverse of those observed during expiration; that is to say, the effect of the systole on the arteries is either lessened, or disappears entirely; again, if a diastole occurs with an inspiration, there is a proportionate diminution of pressure on the blood within the artery; lastly, if the act of inspiration extends over several pulsations of the heart, the reverse of what has been described in the case of expiration is observed. It is further stated, that during inspiration in the dog, the pulsations are accelerated, but less so than in expiration.

M. Ludwig so far applies these researches to human physiology, that he conceives man may be placed, in respect to the motion of the thorax, and probably also as concerns the facility with which the equilibrium of the heart's action is disturbed, between the horse and the dog; and likewise that, *cæteris paribus*, the pulsations of the child will resemble those of the latter animal, whilst those of an old person more assimilate with the pulse of the horse.—*Müller's Archiv für Anatomie, &c.*

ORGANIC CHEMISTRY.

On the Theory of Types. By MM. Laurent, Gerhardt, and Gmelin.

The doctrines of Dumas, of which we have given a general exposition in our present Number (p. 486), have been completely adopted by the above-named chemists; who have even extended them so as to form a tolerably complete system of classification and nomenclature. They believe that the structure of organic compounds may be advantageously compared to that of an inorganic or crystalline body, and that it may be regarded as consisting of a primitive nucleus, around which the atoms of other bodies may be deposited, so as to give rise to various secondary forms. Both of these have some determinate mathematical figure, which is retained, however much their composition may be altered by substitution.

Laurent applies the terms *primitive or fundamental nucleus* to the original group, and he says it is always composed at first of carbon and hydrogen in some even or simple proportion, such as 2 : 2; 4 : 4; 8 : 8; 32 : 32; or 6 : 4; 12 : 6; 12 : 8; 14 : 6; 18 : 8, and so on. It is possible, however, to take away any amount of the hydrogen, and to replace it, atom for atom, with some other body, such as chlorine, bromine, iodine, sulphur, oxygen, &c., without destroying its original form or type. The nucleus which remains after such a substitution, he terms a *derivative nucleus.* If we remove any of the hydrogen without attempting a replacement of it by some other body, the nucleus is broken up, and it resolves itself into one of some other type.

The persistance of this original nucleus, therefore, constitutes a basis of classification, whereby he is enabled to arrange a great number of organic compounds into one series.

The fact also of its retaining its form after the substitution of some other element for hydrogen, enables him to sketch out a plan of nomenclature which shall indicate the composition of the body; thus to take an example, he mentions a primitive nucleus composed of $C_4 H_4$ which he calls ethene; now suppose that one equivalent of its hydrogen has been replaced by one of chlorine, thus $C_4 H_3 Cl$, he would call such a body chlorethase, the termination *se* being indicative of its derivative character, that something in fact has displaced hydrogen, and the prefix *chlor* tells what that something is; if bromine had been the substituted element, it would have been termined bromethase, and so on; when, however, there is no prefix to the word it is understood that oxygen has been substituted for hydrogen, thus ethase would represent a body of this composition, $C_4 H_3 O$.

Again, he has suggested a plan whereby we may indicate the number of elements which have been replaced; he makes use of the vowels *a*, *e*, *i*, *o*, *u*, before the terminal *se* to represent 1, 2, 3, 4, or 5 atoms of substitution, for example, ethene is composed of $C_4 H_4$; if one of its hydrogen has been substituted by one of oxygen, the resulting compound would be termed ethase, $C_4 H_3 O$, and if two, ethese, $C_4 H_2 O_2$; if three, ethise, $C_4 H O_3$; if all four had been replaced thus, $C O_4$, he would term it ethose, and if there had been five atoms of hydrogen replaced, its name would terminate in *use;* the prefix *chlor*, *brom*, &c. would indicate that chlorine, bromine, &c., were the displacing elements. When the number of displaced atoms goes beyond 5, he terminates the word in *alase*, *alese*, *alise*, *alose*, and *aluse*, which represent 6, 7, 8, 9, and 10 elements of substitution.

In a great number of cases, secondary deposits occur either upon the primitive or derivative nucleus; and though the products which so result belong to the same series, they form new figures, which may be termed secondary nuclei or types.

The substance thus deposited is called the envelope, and with few exceptions it consists of an even number of atoms, as of 2, 4, or 6. If more than 6 are thrown down, the secondary nucleus has a great disposition to break up into simpler groups.

Of all the elements thus deposited, hydrogen is the least frequent; and it is generally thrown down in the proportion first mentioned.

Chlorine, bromine, iodine, sulphur and oxygen, are frequently deposited as an envelope, and in all the proportions named. With respect to oxygen, it it remarkable that the proportion determines the property of the compound; thus if 2 atoms have been thrown down, the product is generally neutral or but feebly acid; if 4 it forms, for the most part, a monobasic acid; and if 6, a bibasic one.

Water also, or rather oxygen and hydrogen in the proportion to form water, is frequently the constituent of the envelope, and so are a few other compound molecules, as NO_4, NH_2, &c. &c.—*Comptes Rendus*, tom. xix. p. 1089.

The views of Laurent have been recognised and extended by Gerhardt and Gmelin, both of whom have made them the bases of their systems of classification and nomenclature; systems which bid fair to supersede the radical theories of Berzelius and Liebig. At present they are scarcely matured for general adoption; but they already mark an era in chemistry, which has not been approached since the days of Lavoisier.

The peculiar advantages which they offer are, that the nuclei are for the most part insoluble; they present a system of classification which is much more natural than any other; they keep compounds better together, and they indicate the kind of reaction which is most likely to occur when any substance is broken up by stronger affinities. They also give the chemist an opportunity of assigning a place at once to any new compound; and, above all, they suggest names which indicate the composition of the body.

Researches on the Composition of the Bile of the Ox. By Dr. Strecker.

Of these important researches, which have been carried on under the direction of Professor Liebig, the following are the principal results:

The bile of the ox appears to be composed of salts whose bases are soda, potash, and ammonia, in combination with two azotized acids, of which only one contains sulphur amongst its elements. The azotized acid destitute of sulphur is that which was first discovered in the bile by Gmelin, and described by him under the name of *choleic* acid. When separated and purified, it crystallizes in minute, silky, needle-like crystals; 3·3 parts of it are soluble in 1000 of cold water, and 8·3 parts in 1000 of boiling water; it is readily soluble in alcohol, but cannot be made to crystallize from the alcoholic as from the aqueous solution. Its composition is expressed by the formula, $C_{52}\,H_{43}\,NO_{12}$. This substance undergoes most remarkable transformations under the influence of alkalies and acids. Heated with an excess of caustic baryta in an aqueous solution, the choleic acid is gradually decomposed; after some hours' boiling it disappears, and its place is supplied by a new acid devoid of azote. M. Strecker designates the new body (which is identical with the choleic acid of M. Demarçay) *chololic* acid. Its composition, when dried at 286°, is expressed by the formula $C_{48}\,H_{40}\,O_{10}$; in the crystalline state it contains two prop. of water.

If, from the formula of choleic acid, ...	$C_{52}\,H_{43}\,NO_{12}$
we subtract that of chololic acid, ...	$C_{48}\,H_{40}\,O_{10}$
the remainder, ...	$C_4\,H_3\,NO_2$
with the addition of two proportionals of water, .	$H_2\,O_2$
corresponds exactly with the composition of *glycocoll*	$C_4\,H_5\,NO_4$

the gelatine-sugar of M. Braconnot. Guided by these views, M. Strecker has actually succeeded in obtaining from the liquid that remains after the separation of the chololic acid and the baryta, beautiful crystals of true glycocoll.—An analogous decomposition is produced by the action of strong acids on choleic acid. By means of hydrochloric acid, hydrochlorate of glycocoll is obtained; but in place of chololic acid, the *choloidic* acid of Demarçay is produced; and if the action of the hydrochloric acid be prolonged, the *dyslisine* of Berzelius is the result. The relation of these three substances is very simple, as they only differ from one another by a certain quantity of oxygen and hydrogen in the proportion to form water.

Chololic acid . $C_{48} H_{40} O_{10}$
Choloidic acid . $C_{48} H_{39} O_{9}$
Dyslisine . . $C_{48} H_{36} O_{6}$

The decomposition of choleic acid by mineral acids is then analogous to that of hippuric acid; which, according to the interesting discovery of M. Dessaigne, resolves itself in the same circumstances into glycocoll and benzoic acid.

The sulphurized acid of ox-bile, also, when treated by acids, furnishes choloidic acid and dyslisine; but instead of glycocoll, *taurine* is obtained, which includes all the sulphur of the bile. Dr. Bensch has determined the quantity of sulphur in the bile of different animals; and has found it to vary from 3·5 to 7·20 parts in 100 of the organic constituents of bile.

From the relations thus disclosed between gelatine and the choleic and hippuric acids,—the latter, like the former, resolving themselves under the influence of reagents into glycocoll and a non-azotized acid,—it may be inferred that they proceed from the decomposition of the gelatinous tissues; whilst the sulphurized acid of bile would seem to have its origin in the albuminous.—*Wöhler's Annalen der Chemie, &c.*, Jan. 1848; and *Comptes Rendus*, Dec. 1847.

On the Presence of Urea in the Normal Sweat. By Dr. Landerer.

Landerer obtained a considerable quantity of this fluid in a diluted state, by washing the flannel shirts of several persons who had been perspiring freely. The fluid thus obtained had the peculiar odour of sweat, a strongly saline taste, and exhibited a well-marked acid reaction. On due evaporation, it yielded a yellow syrupy mass, from which, in a few days, an amorphous sediment of phosphates was deposited. The residue was extracted with absolute alcohol, and the solution allowed to evaporate spontaneously. On dissolving the residue in water and adding a dilute solution of oxalic acid, small crystals were formed in the course of 36 hours, which were clearly shown to consist of oxalate of urea. To leave no doubt on the subject, this salt was decomposed, and the nitrate of urea exhibited from it. Besides urea there were found chloride of sodium, traces of sulphates, acetates, and lactates, osmazome, and a substance soluble in ether.—*Heller's Archiv für Physiologische u. Pathologische Chemie u. Mikroskopie*, vol. iv. p. 196.

On the Chemical Composition of various Tumours. By Dr. Von Bibra.

The following analyses have been recently made by Von Bibra.

	1	2	3	4	5	6
Water	84·82	80·83	82·20	70·0	81·09	84·89
Solid residue	15·18	19·17	17·80	30·0	18·91	15·11
Proteine substances	4·83	3·70	5·00	16·99	14·29	1·97
Soluble albumen, and a little hæmatine	2·08	2·47	3·98	traces	.....	0·21
Extractive matters	2·44	1·63	1·06	0·57	3·02	5·07
Glutine	5·47	5·90	7·32	3·78	.....	7·24
Fat	0·36	5·47	0·44	8·66	1·60	0·62

(1) was a fibroid tumour; (2) a scirrhous tumour; (3) an encephaloid tumour; (4) and (5) encysted tumours; and (6) the contents of a tumour lying between the pulmonary and costal pleura.

The salts left on incineration consisted chiefly of chloride of sodium, sulphate and phosphate of soda, and phosphate of lime, in very varying proportion.—*Archiv für Physiolog. Heilk.* 1847. *Heft* 2.

Analysis of a Concretion from the Aorta. By Dr. LANDERER.

THE concretion of which we give the following analysis was taken from the aorta of a man who died from effusion into the pericardium, consequent on gouty metastasis. It is remarkable for the large amount of uric acid.

Uric acid	14
Animal matters	6
Phosphate of lime	62
Carbonate of lime	16
Carbonate of magnesia	2

Buchner's Repertorium, vol. 45, p. 60.

PATHOLOGY.

On Ozone as a Cause of Disease. By Professor SCHÖNBEIN.

THE experiments made by Professor Schönbein some years since tended to show that when a current of ordinary electricity passes from pointed bodies into the air, a substance is produced similar to that which becomes apparent, together with oxygen, at the positive electrode on the decomposition of water by a voltaic pile, or by the action of phosphorus on moist air. To this substance the term *ozone* has been applied, as expressive of its powerful odour. The subsequent experiments of the same distinguished chemist have further shown, that this remarkable substance is not only an eminently oxidising agent, but that when inhaled, even if blended with a large quantity of air, it produces effects similar to those occasioned by chlorine and bromine; irritating the mucous membrane of the respiratory canals, and inducing acute catarrhal affections. As electricity is continually being evolved in the atmosphere, ozone must likewise be constantly produced in quantities which are probably proportionate to the intensity of the electrical discharges. As ozone at an ordinary temperature decomposes iodide of potassium by the separation of the iodine, it is evident that this iodide may, when mixed with a solution of starch, be used (if free from iodate of potash), for the purpose of detecting the presence of an infinitely small quantity of ozone in the atmosphere; since neither ordinary oxygen, nitrogen, nor a mixture of atmospheric air and carbonic acid, produce the same effect on iodide of potassium. This iodide paste will of course be coloured blue, with an intensity proportionate to the quantity of ozone present in the atmosphere. Professor Schönbein has found that the period in which the iodide paste turns blue in the open air varies extremely at different times; being occasionally coloured more intensely in a few hours than at another period in the course of several days. On an average, this blue coloration is effected most rapidly in the colder seasons of the year; there are some days, however, during summer, in which the paste is very rapidly coloured, and this is especially the case in thunder-storms. Experience has shown that ozone is more rapidly produced by ordinary and voltaic electricity in proportion to the low degree of the temperature. It is a well-known fact, that in winter catarrhal affections of the mucous membrane of the respiratory organs frequently occur with such prevalence as to assume the character of an epidemic. Now if it were shown, that at certain periods, characterised by a general prevalence of catarrhal affections, large quantities of chlorine or bromine were present in the atmosphere, no one would hesitate to ascribe the cause of these diseases to the above substances. But it is an established fact, that by the inhalation of proportionally small quantities of ozone, physiological effects are produced similar to those which are occasioned by the inhalation of air charged with chlorine or bromine. This led Schönbein many years ago to conjecture that many catarrhal affections might be owing to the presence of ozone in the atmosphere. In the course of last winter several catarrhal epidemics occurred in Bâsle, so that very few persons escaped; and Schönbein and many physicians of that town, instituted a series of daily observations, with the view of ascertaining how far the rapidity and intensity of the blue coloration of the iodide paste was connected with the prevalence and intensity of the catarrhal symptoms: the results were conclusive as to the simultaneity of the maximum of the coloration with extremest intensity of the epidemic. Further and more exact observations are, however, necessary for the establishment of this fact; and as an investigation of this nature may be so easily effected, we would call attention to the subject, which would be much elucidated by thermometrical and barometrical experiments. (A simple method of making the observation, is by mixing pulverised iodide of potassium with a solution of starch, and exposing to the open air a strip of paper that has been rubbed over with the mixture.) As ozone is immediately decomposed by sulphuretted hydrogen and sulphurous acid, it would not be wholly devoid of interest to determine, whether persons living in the neighbourhood of sulphur-springs, or workmen engaged in metallurgic operations in which sulphurous acid is liberated, are less liable to catarrhal affections than those living in a purer atmosphere. In conclusion, we would remark that two hypotheses prevail with reference to the nature of ozone. According to Delarive and Berzelius, ozone is an allotropic modification of oxygen, occasioned either by electricity or by the catalytic activity of certain substances (as for instance phosphorus); whilst Professor Schönbein regards it as a higher stage of the oxidation of hydrogen, the combination of which is effected either by an electric or a catalytic influence.—*Henle u. Pfeufer's Zeitschrift für rationelle Medizin.* Bd. vi, Heft 2.

Rupture of the Interventricular Septum of the Heart from external violence.

By H. J. Carter, Esq. Assistant Surgeon.

The subject of this case (which we believe to be unique) was a robust, well-formed Parsee, about 40 years of age, who was knocked down and run over by a four-wheeled vehicle, and died just forty-eight hours after the accident, of the injuries he had received. On examining the body, the injuries at first appeared to have been superficial merely; there was no abnormal condition of the brain or of its vessels; and the immediate cause of death seemed to have been congestion of the lungs, the substance of which was fragile, and exuded much frothy blood on pressure. The connexion of this condition with the external violence was not made manifest,—there being neither fracture of the ribs nor anything beyond capillary congestion of the pleura,—until, on laying open the heart, an aperture was found, about three quarters of an inch in diameter, close to the apex of the interventricular partition. This was not circular, but narrow and irregular, with bevelled edges, which were neither polished or fibrous (as in congenital deficiency of the intraventricular partition), nor raised or defined (as the edge of an ulcer), but were soft and pulpy, like the edges of a wound in which the first stage of the healing process has commenced, and were whiter than the muscular tissue in which it was situated.

It can scarcely be questioned that this aperture was occasioned by external violence; though the precise manner in which so strange a lesion was effected is scarcely explicable. It is much to be regretted that no account has been preserved of the symptoms exhibited after the accident.—*Transactions of the Medical and Physical Society of Bombay*, No. 8.

PRACTICAL MEDICINE.

On the Accidents which may result from the Employment of the Iodide of Potassium, and the best means of their prevention. By M. Rodet, Surgeon de l'Hospice de l'Antiquaille de Lyon.

In the review department of our last number (p. 129), we have adverted to a paper by M. Cullerier, upon the ill effects occasionally resulting from the use of iodine; and M. Rodet's essay will be found to contain some very useful observations upon the causes and prevention of another class of these. Admitting the high claims of the medicine to attention, he sets out with protesting against the great abuse that has been made of it in French practice. The article is thrown into certain propositions, which it will be best to state in the author's own words:

"Proposition 1. In the physiological condition, the iodide of potassium exerts its action on certain special organs, and when this becomes pathogenetic, its effect is exerted upon one of these organs, or upon any organ which is already suffering, and is liable to an habitual irritation or fluxion."

All practitioners who have much employed this medicine in large doses, must have observed the excitement it imparts to certain organs or tissues, giving rise to increased appetite, a deeper colour of the mucous membranes, diuresis, &c.; and that in some cases this goes on to the production of irritation or inflammation, having certain special characters, and constituting forms of gastralgia, exanthemata, coryza, bronchitis, or nephritis, and a peculiar condition of the brain termed *iodic intoxication*. M. Ricord maintains that on the mere suspension of the medicine, all these symptoms soon disappear. Other practitioners have not been so fortunate, but have found themselves obliged to combat intense inflammation by active procedures. It is true that in such patients there frequently exists a peculiar predisposition.

"2. The iodide would rarely, if ever, produce serious ill effects, if it were only given in cases which evidently call for its employment." This is the general result of the author's observation, and in which he places the more confidence from its agreement with sound reasoning. When the cases have been properly selected, so far from its producing any ill effects, it has been borne even in enormous doses by the weakest and most irritable stomachs, and has exerted a powerful influence in re-establishing that regularity of the various functions which constitutes health. Practitioners, seeing the really surprising cures it operates under these circumstances, have hastily generalized its application, by prescribing it in all the stages of syphilis, and in the most opposite diseases, and then are surprised at the disappointment which ensues. Convinced of its innocuousness, and awaiting the surprising effects they have led themselves to expect, they allow the mischief it produces to increase more and more, until at last the patient's relief becomes a matter of great difficulty. It is no panacea, and its really remarkable specific effects are only to be expected in certain rare cases, which have been carefully discriminated.

"3. The iodide, as a general rule, is ill borne in cases in which the employment of mercury is indicated; so that, in place of being a succedaneum to this drug, it is only complementary to it." The author well observes that, in spite of all that has been said against it, and the prejudices of the public, sometimes fostered by those of the profession, mercury is our most certain antidote against the venereal disease, and much mischief has resulted from the indiscriminate substitution of iodine for it. Mercury often fails, from the ineffectual mode in which it is administered; and the iodide then resorted to is given without hesitation in the most enormous doses. Experience has, however, confirmed the truth of M. Ricord's observation that mercury is inefficacious in proportion to the long-standing of the disease, while in the very same degree is iodine useful; but another fact which has not been remarked upon is, that the more powerful the agency of iodine against the morbid symptoms it is employed to relieve, the less capable it is of producing the *iodic accidents;* and, on the contrary, these are easily induced, just in proportion as it is powerless against the diseased conditions. Mercury suffices for the early stages of venereal disease, and iodine for the latest, while for the medium ones a combination often best succeeds; but if the order be inverted, a train of accidents arise: *mercurial*, if the mercury has been too long persisted in; *iodic*, if the iodine has been prematurely employed. So much do these accidents resemble each other, that until recently they were all referred to the action of mercury; but the author has never seen either drug give rise to them, when the caution now recommended has been observed.

"4. The iodine acts so much the more favourably, if the patient have not been already treated by other measures. The fact of one or two mercurial courses having been recently undergone, should make us fear the production of iodic accidents, especially such as relate to the brain." A most mischievous error has been the admission of the claims of the iodide of potassium as an antidote of the mercurial cachexia. This has arisen from confounding syphilitic cachexia with mercurial, and from attributing to the influence of mercury the exostoses, caries, necroses, and large ulcerations, which are, in fact, but the consequences of an advanced stage of the disease. In proof of the truth of this observation the author remarks that,—(1) he has never seen iodine produce any mischief when employed against old syphilis in patients who had never undergone mercurial treatment; (2) the same remark holds good when the mercury had been employed at a very early period to combat the primary phenomena of the disease; (3) on the other hand, he has seen this medicine give rise to the most serious lesions when used shortly after the employment of mercury. Several cases observed by the author, or communicated by his friends, are cited in proof of this. The question then presents itself, as to how far the mercury may have been influential in inducing the accidents in question. It is somewhat difficult of solution, and may be best answered by examining the different categories of the cases. (1.) For the mercury, employed for the relief of secondary symptoms, the iodide was, after a longer or shorter period, substituted. Here the accidents may depend upon the absence of an indication for the use of iodine, an excited condition of the various organs produced by the mercury predisposing them to become easily irritated, or upon the combination of the mercury yet remaining in the tissue conferring on the iodine more active and irritating power. (2.) The patient was submitted to one or more mercurializations, more or less complete, for secondary symptoms, and iodine then employed for the treatment of a relapse. The same explanations may be adduced as in the former case; but it may be remarked in reference to the latter portion of these, that although the opinion that mercury may remain accumulated in certain tissues for an indefinite period is evidently incorrect, yet it is certain that it may continue to influence the economy for some time after its use has been discontinued; and although the period required for the removal of this from the system cannot be determined, and is probably variable, yet the shorter the time which elapses between the discontinuance of the one drug and the commencement of the other, the greater is the danger of any ill effects resulting from their combination. (3.) The mercury was employed for the relief of tertiary symptoms, and iodine had recourse to after its failure. As here the iodine was indicated, the ill effects following its use must have chiefly arisen from some such combination adverted to. In some cases they seem to have depended upon the new irritation of the iodine having checked some habitual secretion. "The cerebral symptoms resulting from the single action of iodine differ from those which are produced by the combination of it with mercury. In the first case, as M. Baumès has already remarked, the patient complains of an uneasiness, giddiness, and heaviness of the head, rather than of a true headache. There is also a diminished power of hearing, sight, and recollection, and a difficulty or uncertainty in walking, accompanied often with diminished sensation and warmth in the legs. There is almost always constipation, but I have never seen any difficulty in passing water. In a word, the symptoms indicate rather an oppression of the brain than an inflammation of that organ. In the other case, the same symptoms exist, but there are likewise pain and congestion of the head, seeming to threaten a true inflammation of the brain."

Means of prevention. The precepts for the attainment of this end naturally flow from the consideration already bestowed upon the causes of the accidents in question.—1. *The iodide should never be administered except in cases which rigorously call for its employment.* This seems like the mere stating a truism; but the abuse of this medicine has been so great as scarcely to render it superfluous. It is sufficient for a dubious case to present itself, and the history of the patient, to exhibit the fact of syphilis having once existed, for a vitiation of the blood to be assumed, and this medicine at once given; and that in cases in which, were mercury the durg in question, much greater circumspection would be employed in prescribing it. It is the erroneous opinion of the inoffensiveness of the iodide, which has led to the great abuse made of it. In other cases, where syphilis is out of the question, it has been ordered because reputed to possess certain chemical properties, and because other means have failed. If this is no more successful, the dose is increased with a rashness observed with regard to no other remedy. Frequently it gives rise to irritation of the throat, which, mistaken for syphilis, leads to a still further abuse of the means that has caused it. The cases recorded in the journals of wonderful cures in such opposite diseases, which are often but errors in diagnosis, have given the medicine a celebrity it does not deserve. With the exception of syphilis, the iodide can only be said to be advantageous in scrofulous diseases, and in glandular and other *engorgements*. In syphilis, when discriminately employed, it is truly an heroic remedy; but M. Rodet recommends us,—(1) never to employ it for primary symptoms, unless to fulfil some accessory indication; (2) never to employ it for secondary symptoms; (3) never at first to employ it for the mere symptoms of transition, and to associate it with mercury only when this proves slow and uncertain in its operation; (4) always to employ it alone in tertiary affections at first, and afterwards, if it prove not efficacious enough, to associate mercury with it rather than give it in excessive doses.

2. *Iodine must be employed with the more circumspection in proportion to the greater amount of mercury the patient may have taken, and as the epoch at which this has been taken is more recent.* To prevent the mischief already pointed out, we should never give iodine when mercury is still indicated and may be employed without fear; as it saturates the system, and induces consequent accidents. If mercury has already been taken at a recent epoch in large quantities, and it is feared to have recourse to it again, the *chloride of gold*, not the iodide of potassium, is the eligible substitute. In general, the patients are cured by its agency; and if its effects are not sufficiently favourable, we can discontinue it at the end of five or six weeks, and then have recourse to the iodine alone or conjoined with mercury; the system having had time to rid itself of the overdose of the mercury, and recover from the condition of excitement this may have produced. As might be expected, the gold gives rise to no accident, producing no injurious chemical reaction with the mercury or iodine. If the first mercurial treatment has taken place somewhat remotely, then may the new symptoms be treated either by mercury or iodine, according as they are secondary or tertiary, or in those of transition by the two combined; but in the use of iodine after mercury we should always proceed with much circumspection, and that in proportion to the shortness of the interval.

3. *Whenever the disease for which iodine is given is complicated with inflammatory action of any organ, or by any other affection not directly dependent upon it, we should remove such complication before employing iodine.* It must be acknowledged that frequently great difficulty exists, in distinguishing whether a given morbid condition of an organ arises from, or is independent of, the constitutional disease. Acute affections, as inflammations, cannot give rise to doubt. Their removal must be accomplished before commencing the special treatment. Sometimes, some habitual discharge is suppressed, such as sweating from the feet, epistaxis, hemorrhoids, &c. When this gives rise to the irritation or inflammation of some organ, the course to be pursued is obvious; but when there is only a feeling of general uneasiness, an abnormal condition of the system which requires only a slight cause to localize itself in some particular part, the administration of iodine may easily play the part of such occasional cause. Before having recourse to it, therefore, we should endeavour to restore the deranged equilibrium by reproducing the fluxion.

4. *The action of the iodine should be carefully watched; it should never be given in larger doses than strict necessity requires, and should be suspended as soon as it seems to excite any deleterious effect upon the system.* Even in small doses it produces very injurious effects in certain idiosyncrasies; in others, after having long tolerated it, the system suddenly refuses to bear it. "But such intolerance is of far more frequent occurrence, when it is given in too large doses or continued too long. The desire

of producing prompt effects, and such as may surprise the patient, has sometimes led to its being given from the first in large doses. But the physician should always know how to resist desires of this sort, and have nothing in view but the true interest of the patient confided to his care." The dose required varies surprisingly amid apparently identical conditions, according to the idiosyncrasy of the patient; but we should always *commence* with small ones, and only reach the large doses sometimes required gradually, and when we have assured ourselves of their necessity and of our patient's power of tolerating them. As a general rule, M. Rodet commences with five or six grains, augments the dose only weekly, does not exceed from two scruples to a drachm daily, or prolong the treatment beyond two months. Cases in which larger doses and their long continuance are required are exceptional, for which no rules can be laid down, their management depending upon the tact and skill of the practitioner.—*Gazette Médicale*, 1847, Nos. 46, 47, 48.

We have seldom perused a paper with more satisfaction than the above, replete as it is with sound principle and much-needed caution. Perhaps in the whole range of the materia medica, there is not a drug that has been more rashly and preposterously prescribed than the one in question.

On the Ill Consequences of the Repression of Cutaneous Diseases. By M. DEVERGIE.

M. GOGUE reports several cases from M. Devergie's practice, illustrative of the fact that various morbid conditions of the internal organs may be induced by the repression of cutaneous disease of some standing. The fact has indeed been long acknowledged, but we think insufficiently borne in mind in practice, important as it is, especially in reference to persons somewhat advanced in life, or of enfeebled powers. He sums up the results of his observations with the following conclusions, in the shape of reasons for believing the effects in question to be really due to the "repercussion" of the eruption:

1. The functional disturbances occur simultaneously with some cause manifestly modifying the condition of the skin, as the action of cold or local repellants. 2. The severity of the symptoms is proportioned to the species, extent, and duration of the skin diseased. 3. The symptoms cease on the return of the eruption or the induction of an artificial one. 4. The symptoms presenting themselves are not those of any simple or ordinary disease indicated in our nosologies. 5. From this unusual morbid condition, the diagnosis of which is so unprecise, death may result far more rapidly than from ordinary acute inflammations of the same organs. 6. The autopsy does not exhibit lesions proportioned to the severity of the symptoms. 7. If this morbid condition is treated by the usual antiphlogistic means, we only hasten death; and, moreover, a fatal result is certain if the eruption is not or cannot be quickly reproduced, or an artificial one substituted.

The occurrence of these fatal cases should teach us caution in repressing by medicinal agents eruptions of considerable extent or duration, or of constitutional origin, especially when occurring in the aged and asthmatic—rapidly fatal disease of the chest being easily thus set up in these last. Whenever the powers of any organ are enfeebled, that is the one usually first affected, though others may likewise suffer. Notwithstanding every precaution, the sudden access of cold, moral emotion, &c., may produce the repression; and, however it may be induced, we should endeavour to renew the irritation of the skin by flannel clothing, blisters, irritating embrocations, &c. M. Devergie generally prefers the employment of repeated and extensive friction, by means of *croton oil.*—*Gazette des Hôpitaux*, No. 110.

On Erysipelas of the Hairy Scalp. By M. GRISOLLE.

ERYSIPELAS of the hairy scalp without consecutive extension to the face is so rare a disease, that M. Chomel has not seen more than three or four cases during his long practice. An example falling recently under M. Grisolle's observation, he made a few remarks upon it. In this case the disease occurred without any previous injury or wound of the scalp whatever; and it offered a good example of a characteristic of the affection pointed out long since by M. Chomel,—namely, enlargement of the cervical glands. This is not a constant though a very common symptom, and precedes the occurrence of the erysipelas by two or three days; and whenever M. Grisolle meets with it in an individual otherwise well, and suffering from no chronic affection of the head, or wound or erosion in the vicinity, he always prophesies the supervention of erysipelas of the scalp. In the present case, the earliest symptoms were a stiffness of the neck and an enlargement of these glands. These enlargements also occur in other affections, but then they do so *consecutively* only, and do not form, as in this erysipelas, a *precursory* symptom. It is of importance to attend to this, as erysipelas here manifests itself sometimes only by very obscure symptoms. Pain and œdema of the scalp are, however, usually present, and redness may be seen by close examination. Vesicles never form on the scalp, the disease usually terminating by desquamation, which may be prolonged for several weeks. Numerous small abscesses sometimes appear at the close of the disease. When the disease is traumatic, it may take on the phlegmonous form, and the case then becomes very serious. However great the detachment of parts this may cause, gangrene of the scalp never occurs; which Dupuytren explained by the fact of the vessels passing between the aponeurosis and the skin remaining uninjured amidst the destruction of the cellular tissue. Delirium is one of the symptoms most to be feared. It is, however, only sympathetic, and not due to inflammation of the brain or its membranes; for in the autopsies made in these cases by MM. Chomel, Louis, and Grisolle, no apparent lesions beyond a vivid injection of the scalp have been discovered. M. Grisolle predicted that the erysipelas in this case would not extend to the face, in consequence of the absence of the well-defined line and the red indurated border, which are always seen in erratic érysipelas.—*L'Union Médicale*, No. 14.

Condition of the Gums in Phthisis. By DR. FREDERICQ.

THE author's attention was first called to this subject in 1844, when he observed a line of a red brick colour near the free edge of the otherwise normal gums of a phthisical patient. The line was very narrow, and ran parallel with the edge of the gums, but only opposite the incisors and canine teeth. Since that period, he has examined the gums in numerous subjects of phthisis, and has always found this red line more or less distinctly visible, although sometimes only opposite the inferior median incisors. The researches of the author do not enable him to say whether this sign manifests itself as one of the earliest symptoms of phthisis, nor to declare absolutely that it is seen in no other disease, although he has never yet met with it in such.—*L'Union Médicale*, 1848, No. 5.

SURGERY.

Discussion on the Comparative Value of Lithotomy and Lithotrity.

WHEN the article upon M. Civiale's new work in our last Number was written, we did not anticipate that the French Academy of Medicine would have supplied so pertinent an illustration of its concluding observations. The discussion of the comparative merits of lithotomy and lithotrity occupied many of their meetings, the reports of which fill some two hundred pages or so of the 'Bulletin;' and it is really deplorable to find the able surgeons who took part in it descending to virulence of language and personal recrimination, which should not be tolerated among men avowedly met together for the advancement of science. The case becomes still worse, when it is considered that among the speakers little or no difference of opinion prevailed as to the great value of lithotrity, and that the contention arose in the vain endeavour to furnish an exact numerical statement of this. The accurate indication of the cases that are best suited for its agency, the truly important question, only incidentally occupied attention. Still upon this point, and upon some others of great interest, much valuable information was elicited, which, separated from the endless repetitions and personalities which surrounded it, is well worthy of a permanent record.

MM. Velpeau and Roux having at the commencement of the last year spoken somewhat disparagingly of lithotrity, *M. Civiale*, nowise averse to the combat, challenged a thorough investigation of the subject, and read to the Academy two voluminous documents, giving a statistical account of the comparative success of lithotomy and lithotrity. As respects the former of these operations, the figures he has accumulated are very numerous, but acquired from such a variety of sources, compiled with such uncertainty as to their accuracy, and exhibiting such discrepancy in their results, that we cannot admit them entirely as safe material to reason upon. In regard to lithotrity, his facts are more worthy of attention, as being chiefly furnished from the stores of his own extended experience. Moreover, even his adversaries admit his candour in publishing all his cases, successful and unsuccessful, and differ with him only in the interpretation of the results. The figures so much resemble those we have inserted at p. 101, that we may content ourselves with simply stating his general conclusions, which are: "1. That lithotrity, when well performed, and in only appropriate cases, saves from 96 to 98 per cent. 2. That a fourth of the patients who are refractory to it may be lithotomized. 3. By lithotomy, applied exclusively and without regard to age, 20 to 30 per cent. are lost. 4. Applied to children alone, it saves nine tenths. 5. Applied to adults and old men, it saves from 50 to 75 per cent."

It was not to be expected that a comparison so startling as this should pass by unchallenged, and accordingly *M. Velpeau* endeavoured to show that M. Civiale had unjustifiably exaggerated his success in lithotrity, by including among his cures a great number of cases which died within a twelvemonth, and by excluding from the list of deaths those persons who had died after exploratory examinations and attempts. It may be very true, as M. Civiale says, that several of these patients did not die directly of the operation, but in consequence of disease which might have killed them eventually had this not been performed; but surgeons are not accustomed to count analogous cases of this kind, occurring after the operations, among their cures; and the abstracting such in regard to lithotrity and not lithotomy, gives a factitious value to the former. Practitioners may be deceived by the mild or unusual characters of the symptoms of some of these diseases, which draw attention away from their true nature. Thus a fatal nephritis or cystitis may exist without inducing pain or fever. The patient gradually wastes away, losing his appetite and strength, until death at last takes place. Nothing, in fact, can be more insidious or deceptive than the purulent nephritis or cystitis of calculous patients. In other cases, the operation or exploration may be followed by fatal symptoms, exactly simulating febrile paroxysms or rheumatism. Often the rigors are only symptomatic of the formation of matter in important organs; and the patient is said to die of pleurisy, pneumonia, or various diseases foreign to the operation itself. M. Civiale, in allusion to the practice of the most eminent surgeons, and even of experienced lithotritists, states the amount of success to be very inferior to his own, but explains this by the want of skill of the operators; which is certainly pronouncing an adverse opinion to the reception of lithotrity as a general practice. But, in fact, in ordinary cases, it is by no means difficult; and when the fatal cases are restored to M. Civiale's list, the discrepancy between the results obtained by him and others is not considerable.

Another point in regard to which MM. Civiale and Velpeau are at issue, is the comparative frequency of *relapse* after the two operations. The latter maintains that while the influence of the calculous diathesis will be the same in both, lithotrity offers much more chance of leaving fragments of stone in the bladder to form nuclei for future concretion, while lithotomy enables the surgeon, by means of the finger, to ascertain the thorough evacuation of the organ. M. Civiale totally denies this, and alludes to the numerous cases in which the stone could not be found even after the incision had been made, the finger being frequently quite inadequate for a satisfactory exploration of the bladder. Stones or fragments may therefore be far more easily left in the bladder, than can be the case with the effectual exploratory resources of lithotrity. Moreover, relapse after lithotomy is more frequent in consequence of the little attention which is paid to persistent vesical catarrh and atony of the bladder. As so much depends in securing the expulsive powers of the organs after lithotrity, these conditions always receive due attention; and it is to be observed that secondary calculi are usually phosphatic, and are especially developed during the prevalence of inflammatory action maintaining catarrh.

M. Velpeau, although originally almost an opponent to the introduction of the new operation, now bestows upon it a qualified approval. He believes that in simple cases, when the calculi are of a moderate size, not too hard, or existing in too great numbers, and when the urinary organs are healthy, it is "a brilliant operation, far more successful than lithotomy; that in complicated cases it is scarcely more successful than lithotomy; while in the case of children it is difficult of performance, and not to be preferred." As far as published facts allow him to judge, he supposes the simple cases to exist in about the proportion of 350 in the 1000.

M. Segalas detailed to the Academy the conclusions at which his extensive practice in lithotrity during twenty years has led him to arrive. Taking calculous cases generally, they can oftenest be treated with success by lithotrity; sometimes it is better to employ lithotomy, and it is not rare to meet with cases in which either operation should be foregone. Our determination should depend upon the nature of the calculus, and the condition of the urinary organs. 1. The *size of the stone.* All *small* stones (less than 10 lines diameter), whatever their degree of hardness, or the condition of the urinary organs, are best treated by lithotrity. The same may be said of those of *medium* size (10 to 15 lines), unless conditions of local irritation or disordered general health demand their instant removal, e. g., in cystitis, threatened apoplexy, &c. *Large* stones (15 to 20 lines), if friable, as those of new formation, especially the phosphatic, generally are, are best treated by lithotrity, unless the bad condition of the health or disease of the kidneys contraindicate it. *Very large* stones (above 20 lines), when *phosphatic*, may generally be crushed, notwithstanding the coexisting catarrh of the bladder. If of *uric acid*, they are usually very hard and their fragments angular, and demand lithotomy. *Oxalate of lime* calculi rarely acquire such a volume, and are easily divided; and as they usually occur in the young, their treatment is very successful. 2. The *hardness* of a calculus rarely proves an obstacle to lithotrity; and when it does so, the stone is usually composed of almost pure uric acid, and has been long in forming. Oxalate-of-lime calculi generally yield readily to percussion, and the cases in which they occur terminate favourably and are little liable to relapse. 3. The *number* of the calculi is far from being the objection stated by MM. Roux and Velpeau, and admitted by M. Civiale; on the contrary, for an equal mass of calculous matter, the more the calculi the greater the facility of division and the chance of success. 4. *Condition of the urinary organs.* The too active contraction of the bladder upon the stone, if not subdued by medicines and baths, or if the stone be hard and large, may render lithotomy necessary, as may the existence of inflammation of this organ, or a sacculated condition of its walls. Defective action of the organ may favour the operative procedures, but it is usually connected with sacculi. Enlargement of the prostate merely renders the operation somewhat more difficult, but abscess contraindicates it. A diseased condition of the kidneys is a stronger contraindication for lithotrity than lithotomy, in consequence of the greater time required for the operation. Stricture of the urethra only retards its commencement for a short time. 5. *Sex.* Contrary to the usual opinion, the operation is more often applicable to women than to men. 6. *Age* is no obstacle to its performance. M. Segalas is entirely at issue with most surgeons, when he declares his preference for lithotrity for *children.* In these it requires great care, as, in consequence of the narrowness of the anterior portion of the urethra, and the comparative width of the posterior part, an impaction of fragments may easily occur. M. Segalas founds his preference upon the fact, that notwithstanding the ease with which lithotomy is performed in the child, the mortality after it is much greater than after lithotrity. Thus, of 37 children cut for the stone by that skilful operator, M. Paul Guersant, 7 perished; 4 from supervening disease, such as measles, &c. and 3 from the direct effects of the operation. But M. Segalas has not lost one of the 29 children he has lithotrized. In 23 of these cases the calculus was composed of oxalate of lime; this composition so different from that usually found in the adult being due to the vegetable regimen of the children of the poorer classes, just as uric-acid calculi are generated under the influence of a too animal regimen. The treatment of children by lithotrity is, however, tedious and difficult. Two of these required fifteen sittings, while in eight cases only one sufficed, the average for the entire number being 5¼. M. Segalas, on account of this tediousness, believes that children subjected to the anti-hygienic influence of an hospital, should, except in the simplest cases, be submitted to lithotomy; but when they can be properly attended to in private practice, lithotrity is to be preferred.

M. Blandin observed that if lithotomy heretofore seemed more favourable in its results than it now does, this has resulted from its having been formerly employed in both bad and good cases, whereas now it is only resorted to in cases refused by lithotrity, or in such as this has rendered less favourable by its manœuvres. Lithotritists commit the error of omitting incomplete operations, counting such only in which the stone is completely crushed as an operation. M. Blandin believes from a review of the facts, that calculous patients are in no better position now that a choice lies between the two operations, than they were when lithotomy only existed!

M. Roux, regarding lithotrity as a great acquisition, believes it will only make real progress by removing it from the exaggerated statements of its defenders into the domain of general surgery. Preposterous statements must be abandoned, such as that every patient should be submitted to the trial of lithotrity before undergoing lithotomy, as if this did not aggravate his case; nor should we ever perform the operation merely because the patient prefers it. "A patient must not be the judge of his own case. What does he seek at your hands? His cure, or, at all events, the measures you think most conducive to this. It is your duty to adopt the means most likely to realize the expectation. This has always been my rule of conduct; and nothing would induce me to undertake lithotrity in a case I deemed to require lithotomy." The evils of lithotomy have, in M. Roux's opinion, been exaggerated. In between 500 and 600 operations which he has performed, he has only opened the rectum once. Relapses, dependent upon the presence of calculous matter, he regards as occurring far more frequently in lithotrity. The statistics of lithotomy are very imperfect, and chiefly relate to hospitals in which the mortality is much greater than in private. Between 1812 and 1847, M. Roux has had 55 cases in private, 7 of these being less than 10 years of age, and all recovering. Of the 48 adults, 11 died. He has found the operation especially fatal in persons leading contemplative lives, as the members of the priesthood.

M. Malgaigne presented the following statement of the amount of mortality occurring in Parisian practice, as far as statistical data have enabled him to ascertain this:

	Hospital.	*Private.*
Lithotomy . .	1 in 2·62 .	1 in 4
Lithotrity . . .	1 in 4 . .	1 in 8

This, although not presenting so flattering an aspect as M. Civiale's selected cases, furnishes satisfactory evidence of acknowledgment of the value of the operation by one of the first French surgeons. By this table it appears that lithotomy is just twice as fatal as lithotrity, and that the latter is even as successful in the public hospitals as is the former in private practice.

After the delivery of M. Civiale's reply, MM. Leroy d'Etoilles and Amussat renewed the discussion, which seemed to bid fair to become interminable (having lasted some ten months or so); but as they are both distinguished lithotrists, and are equally opposed to M. Civiale's exaggerations of the advantages of lithotrity and to its depreciation by some of the Paris hospital surgeons, a brief notice of their opinions will render the subject more complete. M. Leroy maintains that the condition of the calculus may be sufficiently ascertained by the aid of the sound; and that when the lithotribe is employed, and perhaps often, the case, so far as the excitement of irritation of the bladder and the rousing into activity morbid conditions of the urinary organs or general system are concerned, is assimilated in its nature to an operation, and that it is unfair to remove such from the list of deaths. He admits, also, that relapse is more frequent after lithotrity than after lithotomy, and that vesical catarrh is more persistent. Retention of urine may follow contusion of the neck of the bladder; the lithotribe may break; while much mischief results from the impaction of fragments in the urethra. But then this operation does not, like lithotomy, expose the patient to hemorrhage, peritonitis, urinary infiltration, purulent resorption, in fact, to the most fatal accidents. "No one knows this better than the surgeons themselves; for when they become the subjects of stone they are lithotrized; witness Viguerie, Dubois, Lisfranc, and Sanson, who, with M. Velpeau, signed the adverse report of 1835." Lithotomy is very fatal in *old age*, while lithotrity is scarcely more so than in the adult. M. Leroy has performed the operation on 14 octogenarians without losing one; and of 27 septuagenarians he has lost but two. In *children* the success of the two operations being much alike, the most expeditious is to be preferred. In the case of a small stone, lithotrity is best; but when this exceeds 17 or 18 millimetres, so as to require two or three sittings, lithotomy is to be preferred.

M. Amussat observed, that surgeons committed a grave error in believing that they might choose one or other of these operations indifferently; and whenever a stone patient dies after lithotomy, the calculus being a small one, his attendant will be justly and severely blamed. So, on the other hand, when death follows lithotrity, undertaken for too hard or too large a stone, the operator is as inexcusable for not having preferred lithotomy. Taking cases generally, M. Amussat regards lithotomy as the exceptional procedure, and he believes few surgeons would themselves submit to it without having first made trial of lithotrity. It has been said that *etherization* has confirmed a superiority to lithotomy, and doubtless it should be always had recourse to during this operation, especially in children; but M. Amussat states that it is no less useful in lithotrity. Much misunderstanding prevails upon this subject, and it has even been said to be absurd to resort to means for the destruction of sensibility, when the presence of this is necessary to indicate when the mucous membrane of the bladder is implicated by the instrument. Whether ether is used or not, the bladder is distended by tepid water prior to commencing the operation, and, by careful manipulation of the instruments, pinching of the mucous membrane can be easily prevented; while pain, arising from this cause, is in nowise distinguishable from that necessarily caused by the mere rubbing of the instrument against the walls of the organ. As yet M. Amussat has only used the ether in four cases, but with the best results as regards the operation itself, and especially the *prevention of febrile reaction*, which is the effect most to be feared from the pain consequent on a prolonged sitting. Its use facilitates the removal of the fragments, which much rather constitute the danger of lithotrity than the play of the instruments; and in this way diminishes the number of sittings.—*Bulletin de l'Académie de Médecine*, tom. xii. and xiii., 1847.

On the Employment of Flour in Erysipelas. By DR. FAVROT.

DR. FAVROT, after testifying to the unsatisfactory results obtained from the nitrate of silver, sulphate of copper, &c., in erysipelas, details some cases in which he has found the application of *flour* of the greatest advantage. Resorting to it in his first case, after the various other means had been tried in vain, his success was so great as to induce him to employ it in future at an earlier period, which he did with equal benefit. With this means, indeed, English surgeons are familiar, and the late Mr. Earle was especially in the habit of attaching considerable importance to its use, but only in superficial erysipelas, and then rather as affording great relief to the irritated skin than possessing any marked curative power. The cases related by Dr. Favrot are, however, of a far more serious character than this, being examples of deep-seated erysipelas, and attended with great and sometimes alarming constitutional symptoms. The parts being completely smothered in flour, and this repeated every quarter of an hour, a remarkable amendment in these symptoms speedily took place, the patients also expressing their sense of comfort. M. Huguier, at the author's request, tested the remedy at the Beaujon, and with the same results. M. Favrot, though unwilling to speculate upon the subject, is disposed to refer the benefit derived to the fact of the exclusion of the air from the inflamed surface.

Another number of the journal from which we are quoting, contains a note from a Dr. Wolf, confirmatory of Dr. Favrot's statements, in which he also states that Vogt, Schwartze, and other German writers, some time since, expressed highly favourable opinions of the same means. M. Malgaigne, too, adds his testimony, stating that he has employed flour not only in erysipelas, but also in various deep-seated inflammations and extensive burns; and in most of these cases with undoubted advantage.—*Revue Médico-Chirurgicale*, vol. ii., pp. 257–263 and 368.

Treatment of Traumatic Erysipelas. By M. BLANDIN.

M. MACQUET has recently furnished an account of M. Blandin's views, according to which inflammation of the lymphatic vessels is the means by which a mere local affection becomes more generalized. The red lines denoting this, and an enlargement of the glands in the vicinity, may even precede any erysipelatous appearance around the wound. This is the element of the disease to be attacked; and as soon as M. Blandin recognises a painful enlargement of the glands, he orders twenty-five leeches to them, repeating these two, three, or even four times, if necessary, to subdue it. Poultices are afterwards applied. Since he has had recourse to this method, most of the threatened attacks of erysipelas have been cut short.—*L'Union Médicale*, 1848, No. 4.

The Application of Ice in Traumatic Lesions. By M. BAUDENS.

M. BAUDENS, senior surgeon of the Military Hospital Val-de-Grace, has long been in the habit of applying ice, either with or without salt, in the treatment of every description of traumatic lesion, for the purpose of keeping down the abnormal temperature which, in these, is so much higher and so much more persistent than would be supposed by those who had not examined into the subject. Not only is the cure expedited, but the patient's sufferings are much alleviated, while no ill consequence has resulted. The patient's feelings are taken as the guide indicating when the pathological caloric has become exhausted; and wet compresses are then gradually substituted. Not only is the ice employed at the commencement of the affection, but through its course, even while the wound is in a state of free suppuration.—*Gazette des Hôp.*, 1848, No. 15.

Diagnosis of Incomplete Fractures. By M. DEBRON.

M. DEBRON, relating a case of fissured fracture of the lower end of the femur, which was undetected during life, observes that while the obscurity of these cases, owing to the absence of crepitus and all the usual symptoms, renders detection difficult, it is very important for the patient that this should be effected; else he is not placed under restrictions in the movements of his limb, which are essential to his well-doing, inasmuch as incaution in this respect has led to the development of inflammation, which has terminated in death, or the loss of the limb. *Severe pain* at the seat of fracture, distinguishable from the more diffused, less fixed, and less intense pain of the accompanying contusion, is one of the best signs. If the indication furnished by this is overlooked or inappreciable, and the limb is not secured, another sign manifests itself, viz., *erysipelas arising at the very seat of fracture,* thus developing itself after the inflammation depending on the contusion has subsided. This erysipelas is accompanied, too, by an œdematous or pasty feeling of the part. The delay (perhaps several days) in the appearance of this form of erysipelas, arises from the inflammation first occurring among the soft parts around the bone, and spreading from within outwards (inversely to what it usually does), it being, in fact, but a symptom of the suppuration which is going on between the bone and the muscles.—*Archives Générales,* tom. xvi., pp. 42–56.

MIDWIFERY, &c.

Post-Puerperal Metritis. By MM. Chomel and Willemin.

Under this term M. Chomel has been long in the habit of describing a form of metritis, which does not manifest itself shortly after labour, as is the case with ordinary metritis, but at a period varying from eight to thirty days; the principal cause of its production being the resumption of the occupations of life prematurely, before the uterus has regained its normal volume. This organ becomes, under the influence of the metritis, much re-enlarged, while the os uteri is sensitive to the touch, tumid, irregular, and often lacerated. The treatment consists in baths and cataplasms, and laxatives in slight cases, bleeding where the pain and general symptoms require it, and afterwards local resolvents or exutories for the lessening the enlarged uterus.

Dr. Willemin has furnished a very good essay upon this subject. He prefers the term *simple idiopathic puerperal metritis,* inasmuch as it occasionally presents itself at a much earlier period than is understood by the term *post-puerperal;* but it is always quite distinct from that form of metritis connected with pyogenic disease. According to the analysis given of ten cases, it is shown that symptoms occurring in some of these may be wanting in others. Thus there are (1) cases in which pain, fever, and anormal volume are present. In others (2) there are pain and increased size, but no general reaction. 3. Neither pain nor fever is present, but there is anormal volume, with sanguinolent lochia, and, in some cases, deep laceration of the os uteri. 4. The rarest form occurs when there is absence of fever and enlargement, while there is pain and sanguinolent lochia. Any of these forms may, and frequently do, become complicated with inflammation of the surrounding cellular tissue of the pelvis, producing iliac phlegmon. The disease is generally, but not always, more acute in proportion as the time elapsed since the labour is short. The neck of the uterus is found to be changed in position, or not to have resumed its normal state; but the author has not observed that sensitiveness described by others. He is disposed to attach much importance to the deep *laceration* of this part observed in 4 out of 10 of his cases, and easily recognisable in one of them twenty days after labour. The sanguinolent character of the lochia is a symptom to be remarked, and when *iliac phlegmon* complicates the disease it occurs usually on the right side only. Bleeding, linseed cataplasms, and emollient glysters relieve the acute symptoms; and local application, with, above all things, rest, suffice for the subacute form. For the iliac phlegmon, M. Rayer employs with great advantage, first, a general bleeding and purgatives, and then a large flying blister. If fever persists, he repeats the bleeding, and covers the whole hypogastric region successively with blisters.—*L'Union Médicale,* 1847, No. 151, and *Archives Générales,* tom. xv. 290–315; xvi., 450–70.

Vomiting in Pregnancy. By M. TROUSSEAU.

M. TROUSSEAU, in one of his recent clinical lectures, took the opportunity of stating the great advantage he had seen accrue from the mode of treating obstinate and dangerous vomiting during pregnancy, adopted some years since by M. Bretonneau. It first occurred to that practitioner, owing to the fact of his patient suffering from violent uterine pain, for the relief of which, believing the vomiting to depend upon its presence, he ordered a *belladonna* lotion to be applied to the hypogàstric regiôn, and with the effect of removing both the pain and the vomiting. In subsequent cases the remedy proved as efficacious, although no pain was felt; and he explained its operation upon the supposition that the vomiting was then sympathetic of irritation of certain of the nerves of the ganglionic system only, prodùced by the enlargement of the uterus. However this may be, many others have adopted the practice with like success.—*Gazette des Hôp.*, 1848, No. I.

Treatment of Muguet. By M. TROUSSEAU.

SLIGHT as is this disease when occurring idiopathically, it is a serious one when complicating other diseases, or appearing towards the termination of prolonged affections. M. Trousseau, after trying every plan of treatment, now employs *Mel. Ros.*, *Borax*, equal parts, dipping a piece of lint in the mixture, and giving it to the child to suck five or six times daily. This generally suffices; but if the disease persists, he applies *Arg. nit.* 5 parts, *Aq. dest.*, 30 parts, once a day, by means of a pencil. Two or three applications generally suffice in the most obstinate cases. If, however, it still resists, he uses the solid nitrate.—*Rev. Méd-Chir.*, tom. ii, p. 350.

Jaundice of New-born Infants. By M. HERVIEUX.

M. HERVIEUX, in a recent thesis, has endeavoured to supply a more correct account of this disease than that hitherto given, derived from his observation of forty-five cases. He rejects the various divisions of the disease made by authors, as so many hypothetical suppositions, derived from observation of what occurs in the adult; and recognises but one form always appearing during the first month, and attended by the same symptoms and morbid changes. He combats the opinion of M. Leger, that the *induration of the cellular tissue* is always accompanied by jaundice; for in 90 cases of this disease, jaundice only manifested itself 31 times, and in 45 cases of jaundice the induration has also been met with but 31 times, showing a coincidence, but not cause and effect. He also denies that *enteritis* is a cause, although a common complication of this as of other diseases of this age. Nor does he admit the engorged or inflamed state of the *liver* as any other than an epiphenomenon, nowise connected with the yellow colour of the skin. Still it is from the disorder in the functions of this organ, in its transition from an organ of hæmatosis to one of biliary secretion, that the icterus arises.

His anatomical observations on the disease lead him to conclude—1. That all the organic tissues are in different degrees liable to the icteric effusion. 2. The intensity of colour which these tissues acquire is in direct proportion to the amount of their vascularity. He enumerates the various structures; but we need not follow him here, merely obsérving that the bones, ligaments, and cartilages were, in a certain number of cases, undoubtedly coloured, the sclerotica, however, being found so only once. Not only were the serous and mucous membranes coloured, but the fluids they secreted likewise. The biliary apparatus was found to have undergone just the same changes as it does in scleroma or enteritis—the so-frequent complications of icterus—but neither the biliary ducts nor umbilical veins presented the marks of inflammation described by some. The yellow patches in the parenchyma of the kidney, described by Billard, were of two sorts, the one arising from icteric suffusions, the other of a far brighter colour, unconnected with it. The respiratory apparatus furnishes evident marks of colour; but the *nervous system*, and the *brain* in particular, were intensely coloured in 31 cases. The hue of the skin is often modified into a coppery or orange colour, by its coexisting vascularity. The conjunctiva, contrary to what has been stated, is *always* coloured, as is also the gingival mucous membrane, and that lining the lower surface of the tongue. The author has never detected the colouring matter in the *urine*, as seen in adults, or a decoloured state of the fæces. The disease commences with the conjunctiva the second or third day after birth, and goes through all its stages in six or seven, or at most fifteen days; exists very rarely beyond the twentieth, and never beyond the thirtieth day. Its complications (from which alone arise any danger in this affection) are purely accidental, being mere coincidences. In these forty-five cases such complications consisted of scleroma 31 times, enteritis 15 times, muguet 5 times, pneumonia twice, purulent ophthalmia 10 times. M. Hervieux, regarding simple icterus as rather the sign of a new function temporarily exceeding its physiological limits, than as a disease, recommends that the case should be left entirely to the resources of nature.—*L'Union Médicale*, 1847, No. 154.

[The jaundice of infants is, in private practice, an affection of no account; and doubtless the severe cases occasionally related have depended upon the presence of some complication. It must not, however, be supposed from M. Hervieux's statement that such complications are common. He has selected these cases from those great establishments for the generation and observation of disease, the Children's Hospitals at Paris, because they were fatal ones, and offered the opportunities of anatomical inspection.]

Application of Nitrate of Silver to the Larynx in Croup.—By Dr. Ware.

The case, one of well-marked croup, occurred in a child of 5 years old. A solution of a drachm of the nitrate in an ounce of water was applied to the larynx by means of Dr. Green's whalebone staff, first twice a day, and then once, with the effect of producing temporary increase of irritation and subsequent relief, together with discharge of false membrane. The child recovered its voice in a week.—*Boston Med. and Surg. Journal*, vol. xxxvii, p. 419.

[This is a solitary case; but we have called attention to it in the conviction that were this means oftener had recourse to at an early period of the disease, many lives would be saved. In a note, the author observes that Dr. Blakeman of New York, has also employed the same remedy with like success; and in the Medico-Chirurg. Review (N. S.), vol. iv, p. 262, very strong testimony in its favour, and directions for its application, will be found furnished by M. Guersant.]

Puerperal Convulsions. By Dr. Veronese.

Dr. Veronese relates an interesting case of puerperal convulsions, for the purpose of showing that the forceps may be advantageously resorted to at an earlier period of the labour than is usually believed. He was called in to the patient (a *primipara*, æt. 22) by the midwife, during the fourth attack of convulsions, which had occurred within an hour. The struggles, of a frightful epileptic character, continued for ten or twelve minutes, and then subsided into an apoplectic condition, again in a quarter of an hour to be succeeded by the convulsions. Pulse full and wiry, countenance turgid and flushed; blood, which some hours before had been abstracted by the midwife [!] on account of plethora, buffed. A free bleeding was practised at once, and on examination, the mouth of the uterus was found open only to the extent of a florin, the membranes thick and unaffected by the slight pains, and the head still at the superior aperture of the pelvis, its diameters being rather large, compared with those of this cavity. The membranes were ruptured with difficulty, a little water flowing away. In this state of the parts, it was desirable to wait awhile before interfering further; but so frequent and alarmingly violent did the attacks become, that it was resolved to try and introduce the forceps. In the interval of a paroxysm this was effected, but with great difficulty; and, after repeated traction, the head was brought to the exit of the pelvis, when an attack, far more vehement than any, compelled temporary suspension of efforts, lest laceration might be caused. The woman now became pulseless, and as if dead; but after a while rallying a little, the extraction was completed. A violent hemorrhage succeeded, which was not checked until after the introduction of the hand, and the detachment of the placenta which adhered. The woman continued entirely senseless for two days afterwards; no attack of eclampsia, however, again occurring. Sinapisms and blisters to the extremities, ice to the head, assafœtida clysters, and free venesection were resorted to, and she eventually did very well.—*Annali Universali*, vol. cxxiii, pp. 124-9.

PHARMACY.

M. Marchal (de Calvi) on *Iodized Oil.*

This preparation has superseded the other forms of iodine at the Val-de-Grace. M. Marchal, reasoning from the fact of the virtues of cod-liver oil being due to the small portion of iodine it contains, concluded that a far more useful preparation of this substance than the iodide of potassium is found to be, might be made by combining it with an organic body. In this way a more complete assimilation of the substance, or, at all events, its longer retention in the economy, might be secured. He chose an oily body, because this forming an emulsion with the bile, would allow of the substance being digested in the small intestine, and enable the stomach to become relieved of its presence. In this way far larger doses can be administered, if requisite, without irritating the latter organ; while the iodine is eliminated by the urine much more slowly and in far less quantities than is the case with the iodide. The trials which have been made are very satisfactory in their results, the progress of the cure of buboes and other glandular enlargements being much expedited. The iodine is dissolved in fresh almond oil as wanted, in the proportion of 1 part to 15; and this is afterwards worked up into an almond emulsion. The minimum dose is one grain.—*Gazette des Hôp.*, 1848, No. 13.

Removal of Iodine Stains.

It having been stated in the 'Annales de Thérapeutique' that *milk* is an excellent means of removing from the fingers this stain now so commonly produced, Malgaigne found that it certainly did so when these were quite recent, but in nowise more easily than water. But however deep or dry they may be, they yield to alcohol.—*Rev. Méd.-Chir.*

On the Tartrate of Potassa and Iron. By M. Mialhe.

Although this compound, long known to chemists, has been but recently much employed in medicine, it possesses properties superior to most other martial preparations. Although it contains above 30 per cent. of the peroxide of iron, its ferruginous flavour is so slight, that it may be tolerated by stomachs which reject all other preparations of iron. It is very soluble, and as alkalies do not decompose the solution, it is as capable of absorption amid the alkaline juices of the intestine as in presence of the gastric acid. Moreover, it causes no constipation. It can, therefore, be well employed in all the pathological conditions demanding the use of iron, and especially such as require it in large doses, as the syphilitic cachexia. In a case related it changed, after long use, the colour of the hair from fair to dark, "a point well worthy the attention of physiologists and physicians, for it proves that, on the administration of iron, it is solely by the hairs that the excess of this metal is excreted, as M. Dumas had already supposed; for certainly to sulphuret of iron was the colour due in this case." The drug has been said to give rise to diarrhœa, but this it does only when adulterated, which it very often is. Nothing, however, can be easier than for practitioners to prepare it for themselves, since for this it suffices to act upon an excess of peroxide of iron by cream of tartar, dissolved in six or seven times its weight of water, in the bath used for the preparation of extracts. As soon as the saturation is completed, which it is recognised to be by the deep red colour and the sweetish flavour acquired by the liquor, it is to be filtered and evaporated. The salt may be given in the form of pills prepared with mucilage, or, better still, with *oily mucilage*, which is composed of 200 parts of gum arabic, 100 of sugar, 100 of almond oil, and 250 of water, and by means of which the pills may be very rapidly prepared. From two to eight five-grain pills, or more, may be taken daily. Children will very readily take a syrup formed of simple syrup 500 parts, the tartrate and canella water, of each 16 parts.—*L'Union Médicale*, 1848, No. 2.

FORENSIC MEDICINE.

Fatal Wounds of the Uterus, with tearing away of the Intestines, not productive of immediate death. By M. TARDIEU.

A HORRIBLE case which recently came before the French tribunals, and upon the medico-legal bearings of which the author was consulted, together with MM. Orfila and Cloquet, furnishes the subject for this paper. The violence in question was committed by a peasant on the person of his wife, seven months advanced in pregnancy. The evidence showed that the woman was heard supplicating and reproaching her murderer, *three quarters of an hour* after large portions of her intestines had been seen in the yard, having been thrown there by him. The fœtus found in the bed had breathed. On examining the body, no traces of external violence were observable. A large quantity of fluid blood was found in the cavity of the abdomen, extensive lacerations of the vagina, uterus, and peritoneum existing, the ragged edges of the parts showing that a cutting instrument had not been employed. The whole of the intestinal canal, from within 50 centimetres of the pylorus to 8 centimetres from the ileo-cœcal valve, had been torn away, a portion of the highly-injected mesentery remaining.

The question discussed by the author is, whether the subject of such dreadful injuries could retain consciousness so long as she is deposed to have done; and he lays down the proposition, "That immediate death does not necessarily follow the severest lesions of organs the most important to life; and the functions of relation may persist for more or less time, even when the wounds implicate the brain, heart, or lungs." Every surgeon, he observes, is aware that the most severe injuries may be inflicted upon the *brain*, without the patient always immediately losing his consciousness or power of speech; and M. Bayard has collected in a memoir, (Annales d'Hygiène, vol. xxvi,) several cases in which the individual was enabled to talk and walk for hours, and even days, after the accident. The same remark may be made with respect to wounds of the *lungs*, and, what is still more surprising, of the *heart and the large vessels*. M. Tardieu met himself with a case, in which a young man, whose heart had been largely opened with a poniard, lived a quarter of an hour, and talked to the last; and, in a recent murder in Paris, although the heart, lung, and stomach were pierced through and through, the victim descended one staircase and ascended another before he expired. In the present case it might be supposed that immediate death would result from hemorrhage produced by the opening of so many of the intestinal vessels; but no fact is better known than that wounds produced by tearing do not give rise to immediate hemorrhage; and, in the present case, the muscular and mucous tunics, in which the vessels are principally distributed, were found notably retracted. The blood, too, found in the abdomen was fluid, while, if it had been effused before death, a portion would have been found coagulated. Always, when death is produced from wounds of the heart or principal vessels of the abdomen, large coagula, swimming in the sanguinolent serum, are found in the cavity.

Cases analogous to the one in question are on record. Suicides have given issue to the abdominal viscera, without immediate death resulting. M. P. Dubois mentions the case of a chemist, who removed a large portion of the mesentery with a knife and recovered. Delmas, of Montpellier, relates one of a waggoner, who, after a complete laceration of the intestine, and the rupture of the spleen and diaphragm, finished his journey, dying only 18 hours afterwards. In illustration of this point, the observation of Dr. Gestin, of Quimper, may be adduced, that the horses in bull-fights continue their course after the most frightful wounds and eventrations. In respect to the *uterus* itself, M. Dezeimeris, in his essay on rupture of that organ (Journal d'Expérience, tom. iii, p. 241), cites fifteen cases, in which, in spite of extensive lacerations, hemorrhages, passage of the child into the peritoneum, &c., the women have survived for various periods, varying from an hour to six weeks. Writers on midwifery, too, give cases in which the intestines protruded through the uterine wound have become strangulated or gangrened, and yet the patients have remained sensible at least for several hours, some having even recovered. To come to cases more resembling the present, M. Tardieu cites one in which a pupil tore away the uterus itself, the woman uttering frightful cries, and living several minutes after. In another similar case she lived two hours, and in a third half an hour. Mauriceau relates a case in which the intestines, mesentery, and uterus were torn away, the woman living an hour.—*Annales d'Hygiène*, vol. xxxix, pp. 157–172.

[Apart from the physiological interest arising from the prolongation and even occasional preservation of life amid such appalling circumstances, the record of this description of cases is of high utility in a medico-legal point of view, as inculcating more caution than is frequently observed in courts of justice by medical witnesses in delivering their opinions upon the point in question, sometimes to the frustration of the ends of justice. This is the more necessary in this country, as, unfortunately, we have no class of professional men, as on the Continent, exclusively consulted upon medico-legal topics; so that we deny ourselves all the benefit of accumulated experience, and a practitioner may be called upon to give his evidence who is totally ignorant of all relating to the point at issue.]

Case of Poisoning from Arsenic, in which the symptoms of Narcotism were prominent at the commencement. By C. MOREHEAD, M.D.

A HINDOO goldsmith, of about 30 years of age, was one morning picked up by the police, in a comatose condition, in the public street of Bombay. His pupils were dilated, the breathing natural, the pulse frequent; there was no appearance of injury, and no emaciation or sign of long-continued sickness. As he was being lifted from the cart in which he had been conveyed to the hospital, he vomited a little bilious matter. Nothing being known of his history, his case was treated as one of narcotic poisoning. Cold affusion was used to the head, and an emetic of ipecacuanha and carbonate of ammonia was given. The emetic acted readily, and he became sensible. He vomited several times during the day, and was purged two or three times, the evacuations consisting of gelatinous-looking mucus. Towards evening, the pulse became very feeble, the breathing hurried, the thirst and anxiety considerable, with occasional retching. He died about 5 o'clock the next morning. The only statement he made was, that he had eaten some sweetmeats the night before he was brought to the hospital. The result of the coroner's inquest was that he had taken poison himself. The post-mortem appearances were those usually found in cases of arsenical poisoning; and analysis of the contents of the stomach gave distinct proof that arsenic had been taken. In order to justify us, however, in attributing the narcotic symptoms to the agency of that poison, we ought to be satisfied that no other had been taken with it. The only distinctive peculiarity of the narcotic symptoms was a fixed frown on the countenance; an expression of suffering not usual in simple narcotism.—*Transactions of the Medical and Physical Society of Bombay*, No. 8.

MISCELLANEOUS.

The Concours.

The three Faculties of Paris, Montpellier, and Strasburg, consulted by the late French government upon some of the principles which should prevail in the proposed reorganization of the medical profession, declared with almost entire unanimity in favour of the election to all medical appointments by the Concours. This opinion from the seniors of the profession, which, in some quarters, was very unexpected, would seem to be decisive in favour of the suitableness of the institution, at least for France. Our object in adverting to the subject just now, however, is merely to quote a table introduced by M. Berard, in his report of the Faculty of Paris, and which he has compiled in answer to the somewhat contradictory assertion put forth in the Chamber of Peers by M. Cousin, that the Concours is only calculated to furnish men either too young, or already worn out. The table furnishes the ages of the various Professors (since 1830) at the periods of their election:—

Name.	Age.	Date.	Name.	Age.	Date.
Breschet	53	1836	Trousseau	38	1839
Pelletan	49	1831	Dumas	38	1838
Sanson	46	1836	Richard	37	1831
Rostan	43	1833	Denonvilliers	37	1845
Piorry	43	1839	Gerdy	36	1833
Blandin	42	1840	Bouillaud	35	1831
Cloquet	41	1831	Royer-Collard	35	1837
Berard, junr	40	1842	Gavarret	34	1843
Velpeau	39	1834	Berard, senr	33	1831
Dubois	39	1834			

The general mean 39 years. In the Concours now holding there are ten candidates, the youngest of whom is 32, the oldest 52, the mean age being 43.—*Revue Médico-Chirurgicale*, vol. ii, p. 378.

DR. BLAKISTON'S "PRACTICAL OBSERVATIONS ON DISEASES OF THE CHEST."

We much regret that the illness of two of our most valued Contributors, to whom this work has been successively intrusted, has prevented us from fulfilling our intention of reviewing it in our present Number. We have obtained, however, sufficient acquaintance with its character, to be enabled to pronounce it a work of no common pretensions.

MR. FERGUSSON'S LITHOTRITE.

Our attention has been called by Mr. Fergusson to an erroneous statement in our last Number (p. 103), respecting his claims as an improver of lithotritic instruments, which we most willingly take the earliest opportunity of correcting. The "rack-and-pinion" lithotrite, delineated in Mr. Fergusson's 'Manual of Surgery,' is spoken of as "a French instrument, and used by many French surgeons;" whereas, the fact is, as we are now well convinced, that the merit of the application of the rack-and-pinion to the lithotrite,—an improvement of very great importance,—rests entirely with Mr. Fergusson.

The source of the error has lain in the unblushing impudence (we can use no milder term) with which M. Civiale has transferred to his pages *an actual copy* of Mr. Fergusson's woodcut—according to the confident assertion of Mr. Bagg, the artist who executed it,—as a representation of one of the early forms invented and used by himself. The writer of the review, not being prepared for such a gross plagiarism, fell into the trap laid for him by M. Civiale; an error for which we trust that Mr. Fergusson and our readers will excuse him.

BOOKS RECEIVED FOR REVIEW.

British Cholera; its Nature and Causes considered in connexion with Sanitary Improvement, and in comparison with Asiatic Cholera. By Spencer Thompson, M.D., L.R.C.S.E. and L.A.H.L. London, 1848. Fcap. 8vo, pp. 110.

A Discourse on the Asiatic Cholera, and its Relations to some other Epidemics; including General and Special Rules for its Prevention and Treatment. By Thomas Henry Starr, M.D., Senior Physician to the Warwick Dispensary.

Du Choléra moyens préservatifs et curatifs, ou Philosophie des Grands Epidemics. Par M. Bureaud-Riofrey, D.M.P., &c. &c. Paris, 1847. 8vo, pp. 116.

On the Nature and Treatment of the Epidemic or Asiatic Cholera; with simple Directions for the Suppression and Prevention of the Disease. By Robert Venables, A.M., M.B., Oxon. Fourth Edition. London, 1848. 12mo, pp. 59.

The four preceding treatises will be reviewed in our next.

Elements of General Pathology. By A. F. Chomel, Professor of Clinical Medicine to the Faculty of Paris, &c. &c. Third Edition. Translated from the French, by F. E. Oliver, M.D., and W. W. Morland, M.D. Boston (N.E.), 1848. 8vo, pp. 449.

Principles of Medicine; comprising General Pathology and Therapeutics, and a brief General View of Etiology, Nosology, Semeiology, Diagnosis, Prognosis, and Hygienics. By Charles J. B. Williams, M.D. F.R.S., Professor of the Principles and Practice of Medicine in University College, London, &c. &c. Second Edition, considerably enlarged. London, 1848. 8vo, pp. 533.

Will be reviewed in our next.

Dysphonia Clericorum, or Clergyman's Sore-throat; its Pathology, Treatment, and Prevention. By James Mackness, M.D., &c. London, 1848. 8vo, pp, 125.

Will be reviewed in our next.

Medicine an Art, and its Truths to be Attained; being an Address read at the Opening Meeting of the Devon and Exeter Pathological Society. By Thomas Shapter, M.D., Physician to the Devon and Exeter Hospital, &c.&c. London, 1848. 8vo, pp, 31.

Discourses on Medical Education, and on the Medical Profession. By John Ware, M.D., Professor of the Theory and Practice of Physic in the University of Cambridge (New England). Boston, 1847. 8vo, pp. 113.

On the Aims and Philosophic Method of Pathological Research: an Inaugural Address, delivered at St. Thomas's Hospital, Dec. 15, 1847. By John Simon, F.R.S., &c. London, 1848. 8vo, pp. 52.

Outlines of Medical Proof. By Thomas Mayo, M.D., F.R.S., &c. London, 1848. 8vo, pp. 47.

The four preceding pamphlets will be reviewed together in our next.

Pocket Dispensatory and Therapeutical Remembrancer; comprising the entire Lists of Materia Medica, Preparations, and Compounds, in the London, Edinburgh, and Dublin Pharmacopœias, &c., &c. By John Mayne, M.D., London, 1848. 12mo, pp. 269.

Portraits of Diseases of the Skin. By Erasmus Wilson, F.R.S., &c Fascic. II. With four plates. Folio. London, 1848.

Lettres sur la Lithotritie, ou l'Art de Broyer la Pierre. Par la Docteur Civiale. Sixième Lettre. Paris, 1848. 8vo, pp. 169.

This brochure consists chiefly of reports of the replies made by M. Civiale to his various opponents during the recent discussion on Lithotrity in the Academy of Medicine.

Etherization; with Surgical Remarks. By John C. Warren, M.D., Emeritus Professor of Anatomy and Surgery in the University at Cambridge (New England), &c., &c. Boston, 1848. 12mo, pp. 96.

The Advantages of Ether and Chloroform in Operative Surgery. An Address delivered to the Hunterian Society, Feb. 9, 1848. By T. B. Curling, Lecturer on Surgery at the London Hospital, &c. London, 1848. 8vo, pp. 36.

Remarks on the alleged Fatal Case of Chloroform Inhalation. By J. Y. Simpson, M.D., Professor of Midwifery in the University of Edinburgh. 8vo, pp. 8.

Chloroform in the Practice of Midwifery. By Edward A. Murphy, A.M., M.D., Professor of Midwifery in University College, London. Read at the Harveian Society, Feb. 5, 1848. 8vo, pp. 28.

Practical Observations on Midwifery, and the Diseases incident to the Puerperal State. By Alfred H. M'Clintock, M.D., F.R.C.S.I., and Samuel L. Hardy, M.D., F.R.C.S.I., Ex-Assistants of the Dublin Lying-in-Hospital. Dublin, 1848. 8vo, pp. 368.

Females and their Diseases: a Series of Letters to his Class. By Charles D. Meigs, M.D., Professor of Midwifery, and the Diseases of Women and Children in the Jefferson Medical College at Philadelphia, &c, &c. Philadelphia, 1848. 8vo, pp. 670.

The two preceding treatises will be reviewed in our next.

On the Archetype and Homologies of the Vertebrate Skeleton. By Richard Owen, F.R.S., &c. With Two Plates. London, 1848. 8vo, pp. 203.

Will be reviewed in our next.

Outlines of Physiology; for the Use of Students. Part I. By Allen Thompson, M.D., (late) Professor of the Institutes of Medicine in the University of Edinburgh. Edinburgh, 1848. 12mo, pp. 180.

Will be reviewed when complete.

A Treatise on Diet and Regimen. By William H. Robertson, M.D., &c. Fourth Edition, rewritten and much enlarged. Part V. London, 1848. 12mo, pp. 120.

The Microscopic Anatomy of the Human Body, in Health and Disease. By Arthur Hill Hassal. Part XII. With four plates.

The Philosophy of Animated Nature; or the Laws and Action of the Nervous System. By

G. Calvert Holland, M.D., Physician Extraordinary to the Sheffield General Infirmary. Lond. 1848. 8vo, pp. 512.

Will be reviewed in an early number.

The Use of the Body in Relation to the Mind. By George Moore, M.D., &c. Second Edition. London, 1847. Crown 8vo, pp. 433.

Vision in Health and Disease; the value of Glasses for its Restoration, and the Mischief caused by their Abuse. By Alfred Smee, F.R.S., &c. Illustrated by a plate and numerous woodcuts. London. 8vo, pp. 64.

The Natural History of the Human Species; its Typical Forms, Primæval Distribution, Filiations, and Migrations. By Lieut.-Col. Chas. Hamilton Smith, K.H., K.W., F.R.S., F.L.S., &c. With 34 engravings. London, 1848. 12mo, pp. 464.

Researches on the Motion of the Juices in the Animal Body. By Justus Liebig, M.D. Edited by William Gregory, M.D. London, 1848. 8vo, pp. 109.

First Report of the Commissioners appointed to inquire whether any and what Special Means may be requisite for the Improvement of the Health of the Metropolis; with Minutes of Evidence. London, 1848. 8vo, pp. 430.

Second Report of the Metropolitan Sanitary Commissioners. London, 1848. Folio, pp. 35.

Observations on the Prevention of Contagious Diseases, by the effectual Ventilation of the Houses of the Lower Classes. By Robert Collins, M.D., M.R.D.S., President of the College of Physicians in Ireland, &c. &c. Dublin, 1848. 8vo, pp. 20.

This very excellent Pamphlet has been reprinted, we are glad to learn, by order of Government, and has been sent to every Poor-Law Guardian and public Authority in Ireland.

Sanitary Reform and Agricultural Improvement; or how to promote Health and Abundance. In Three Letters. By Charles F. Ellerman. Letter I. Drainage, Urinaria. and Cloacæ. London, 1848. 8vo, pp. 70.

Report by the Committee of the Royal College of Physicians (Edinburgh), appointed to consider any Bills that may be brought into Parliament for the Improvement of the Health of Towns, and the applicability of such Measures to Scotland. Edinburgh, 1848. 8vo, pp. 12.

The Sanitary Condition of Great Yarmouth. A Lecture. By C. Lockhart Robertson, M.D. Yarmouth, 1847. 12mo. pp. 44.

Chemical Technology; or Chemistry applied to the Arts and Manufactures. By Dr. F. Knapp. Professor at the University of Giessen. Edited, with numerous notes and Additions, by Dr. Edmund Ronalds, Lecturer on Chemistry at the Middlesex Hospital, and Dr. Thos. Richardson, of Newcastle-on-Tyne. Vol. I. Illustrated with 212 wood-engravings. London, 1848. 8vo, pp. 568.

Elements of Natural Philosophy; being an Experimental Introduction to the Study of the Physical Sciences. By Golding Bird, A.M., M.D., F.R.S., &c. Third Edition, revised and enlarged. London, 1848. Fcap. 8vo, pp. 552.

The Wonders of Geology; or, a Familiar Exposition of Geological Phenomena. By Gideon A. Mantell, LL.D., F.R.S., &c. &c. Two Vols. Sixth Edition. With 6 coloured plates and 198 wood-engravings. London, 1848. 8vo, pp. 938.

A History of British Crustacea. By Thomas Bell, F.R.S., F.L.S., F.Z.S., &c., Professor of Zoology in King's College. London. Parts I to VI. Illustrated with wood-engravings. London, 1844–1848.

A History of British Mollusca and their Shells. By Prof. E. Forbes, F.R.S., of King's College, London; and Sylvanus Hanley, B.A., F.L.S. Parts I to III. Illustrated with copper-plates. London, 1848.

Cosmos: a Sketch of a Physical Description of the Universe. By Alex. von Humboldt. Translated under the Superintendence of Lieut. Col. Edward Sabine, For. Sec. R.S. Vol. II. London, 1848. Crown 8vo, pp. 522.

The Nature and Elements of the External World; or Universal Immaterialism fully explained and newly demonstrated. London, 1847. 8vo, pp. 269.

An Account of the Cultivation and Manufacture of Tea in China; derived from Personal Observation. By Samuel Ball, Esq., late Inspector of Teas to the H.E.I.C. in China. With three plates. London, 1848. 8vo, pp. 382.

The Scotch Epidemic Fever of 1843-44; its History, Pathology, and Treatment. By John R. Wardell, M.D. (Extracted from the London Medical Gazette.) London, 1848. 8vo, pp. 114.

Dissertatio Medica Inauguralis de Renum in Morbo Brightii Structura Penitiori; quam pro gradu doctoratus, in Acad. Lugduno-Batava, submittet Johannes A. Boogard. Roterdami, 1846. 8vo, pp. 63.

Dissertatio Medico–Inauguralis de causâ Rhythmi Respirationis; quam pro gradu doctoratus, in Acad. Lugduno-Batavia, submittet Fred. J. I. Schmidt. Roterdami, 1847. 8vo, pp. 77.

Erfahrungen über die Krankheiten des Gehöres und ihre Heilung. Vom Medizinalrathe Eduard Schmalz, Doktor der Medizin, &c. Mit 4 Tafeln in gr. Folio. Leipzig. 1846. 8vo, pp. 430.

Neue Methode die Blutmenge im thierischen. Organismus zu bestimmen. Von Dr. Johann Weisz. (Separat-Abdruck aus der Zeitschrift der R. R. Gesellschaft der Aerzte, Dec. 1847) 8vo, pp. 26.

The London and Provincial Medical Directory for 1848. London. 12mo, pp. 634.

Half-Yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D. Cantab., &c. July—Dec. 1847.

The Retrospect of Medicine. Edited by W. Braithwaite, Lecturer on Obstetric Medicine at the Leeds School of Medicine. July to December, 1847.

Transactions of the Medical and Physical Society of Bombay, for the Years 1845 and 1846. Bombay, 1847. 8vo, pp. 122.

A Companion to the Barometer, or a Table for showing what state of Weather will be likely to follow its Changes. By John Underwood. London, 1848.

This Table has been constructed as an expression of the results of prolonged observation; and may be recommended as coming as near to the truth as anything of the kind, in the present state of our knowledge, is likely to approach.

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